The Effects of Affirmative Action Policies in University Admissions on Human Capital Development of Minority Children: a Test of the Expectations Hypothesis.

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Abstract
It has been well documented that minority children leave primary school with lower levels of acquired skill than do their white counterparts. The causes of this "skill gap", however, are not well understood. This paper attempts to analyze one possible cause: the impact of perceived labor market discrimination on the human capital development of minority children. Using the CNLSY79 data, I take advantage of recent changes in affirmative action laws regarding university admissions in California and Texas as a natural experiment. I employ both difference-in-difference-in-difference and individual fixed effects methodologies to test for changes in achievement test scores among minority children between the ages of 7 and 14. The results show a significant drop in test scores among thirteen and fourteen year old African-Americans in the affected states relative to whites, but no significant impact among Hispanics. Younger age groups show negative, but insignificant effects. These results suggest that expectations do play a role in the human capital development of minority children and further research in this area is warranted.

1. Introduction

Historically, in the United States, minority wages have lagged significantly behind those of their white counterparts. In the decades following the civil rights legislation of the 1960's this "wage gap" began to close, however in recent years this convergence has halted and substantial gaps in market wages remain for both blacks and Hispanics relative to those of white Americans with similar educational backgrounds[1]. The traditional explanation offered for these otherwise unexplained differences in wages has been the presence of some form of labor market discrimination that has served to limit the earnings potential of many minorities. The implication is that minority workers with the same level of education as white workers receive lower returns to that education as a result of discrimination in the labor market. Recent research, however, has suggested an alternative explanation for the persistence of wage differentials between whites and minorities, namely that whites and minorities with similar education levels enter the labor market with differing levels of acquired skill, the so called "skill gap"[2]. Further research has shown that the acquired skill differences, as measured by achievement test scores[3], between races begin at a very early age and tend to get wider as children grow older. What is not entirely understood, however, is exactly why these skill gaps exist and what causes the gaps to widen as children progress through school.

[1] For an extensive treatment of these issues see *The Black-White Test Score Gap* (1998), edited by Christopher Jencks and Meredith Phillips.

[2] There have been some notable proposals that the skill-gap is in part or in whole the result of differences in innate ability between races and therefore not the product of *acquired* skill levels. To our knowledge there has been no scientific proof for this claim. See Nisbett (1998) for a discussion of the evidence surrounding this claim. It is assumed, for the remainder of this paper, that differences in skill level between races are exclusively due to differences in acquired skill levels, not due to genetic differences.

[3] Commonly referred to as the test-score gap. For the purposes of this paper we will assume the skill gap and test score gap refer to the same phenomenon.

One possible explanation for the widening of skill gaps as children age involves expectations about possible future discrimination or lack of opportunities that children may face once they enter the labor market. Whether based on reality or perception, these future expectations may reduce incentives for minorities to invest in human capital development by lowering the expected return to that investment. Unfortunately, very little work has been done regarding how early expectations regarding possible future discrimination are formed by children and parents, and whether or not these expectations have an impact on the human capital development of minority children.

The focus of this paper is to empirically test the hypothesis that expectations regarding possible future discrimination or lack of opportunities can result in a reduction in the human capital development of minority children. We take advantage of the changes in affirmative action laws regarding admissions into public universities in California and Texas to use as a natural experiment in determining if there are resulting changes in ability test scores among minorities. Our empirical results indicate that the changes in the affirmative action landscape resulted in a significant decrease in achievement test scores among thirteen and fourteen year old blacks relative to whites. Younger age groups were also tested and found to be insignificant, although the coefficient was consistently negative across all age groups and specifications of the model. These results suggest that future expectations do appear to play a role in the human capital development of minority children and that further investigation into this issue would be warranted.

The paper proceeds as follows. Section two of the paper provides a review of the relevant empirical literature. Section three provides a brief overview of the affirmative action laws and law changes pertaining to university admissions. Section four presents a simple intuitive theoretical framework. Section five describes the identification strategy. Section six provides a description of the data used. Empirical results are presented in section seven. Section eight discusses study limitations and other issues concerning the skill gap and section nine concludes.

2. Review of Relevant Literature[4]

Recent Literature Related to the Skill Gap

In an influential paper using the Armored Forces Qualifying Test (AFQT) as a proxy for acquired skill levels, Neal and Johnson (1996) show that much of the current differences in wages between black and white males and all of the differences in wages between black and white females as well as all the differences in wages between Hispanics and whites of both genders can be attributed to differences in pre-labor market skill levels, the so called “skill gap”. In their study, Neal and Johnson, using the National Longitudinal Survey of Youth 1979 (NLSY79), restrict their data to individuals taking the AFQT prior to entering the labor market in an attempt to capture ability scores for individuals that are unaffected by current labor market discrimination and can therefore be considered a true measure of pre-labor market skill levels.

The Neal and Johnson study has been criticized for a number of reasons due to econometrics issues or on the basis of whether AFQT scores are a good and racially unbiased measure of skills or ability (Mason (2000), Cordero-Guzman (2001), Bollinger (2003)). One criticism of particular interest involves the claim that the AFQT scores used by Neal and Johnson are a true measure of “pre-market” skill levels, and are not affected

[4] The literature review presented here is not intended to be an exhaustive overview of all literature pertaining to the skill gap and the affects of affirmative action, but to provide sufficient background and motivation for the issues dealt with in this paper. For a more lengthy exposition of the issues, background, and literature concerning the test-score gap, the reader is encouraged to read *The Black-White Test Score Gap* (1998), edited by Christopher Jencks and Meredith Phillips.

by experiences or expectations regarding discrimination in the labor market. Carneiro, Heckman, and Masterov (2005) suggest that there is a certain degree of “arbitrariness” in determining what is and is not a pre-market factor, and that decisions made by children and parents regarding human capital investment may be affected by expectations regarding future discrimination or the experiences of parents, even when the children are years away from the labor market. This criticism, however, does not significantly damage the primary implication of the Neal and Johnson paper that differences in skills among young adults just entering the labor market seem to account for much, if not all, of the differences in wages between races regardless of how these skill differences develop. The distinction, however, on whether these skill differences are truly the result of “pre-market” factors exclusively, as suggested by Neal and Johnson, or if labor market discrimination, real or perceived, plays a role in the human capital development of minority children does have potentially significant policy implications, in particular with regard to affirmative action type programs. If the Neal and Johnson assumption is true, and current labor market discrimination has very little impact on the current wage gap, directly or indirectly, then labor market solutions, such as affirmative action type programs, may be an inappropriate, and ultimately ineffective approach to solving this issue. As a result, the understanding of when and how these skill gaps develop and what role, if any, labor market solutions can play in closing these gaps is an important social and political question that needs to be more formally understood.

Since the publication of the Neal and Johnson article, much research has been done regarding the skill gap. It is now well documented that skill gaps exist between white and minority children long before children are ready to enter the labor force, in fact these skill gaps exist at very early ages and tend to widen as children get older. Carneiro, Heckman, and Masterov (2005) use the Children of the NLSY79 (CNLSY79) survey to test for the existence of skill gaps for children aged four to fourteen. Using the Peabody Individual Achievement Test in Mathematics (PIAT Math) as a measure of achievement they find that gaps exist between whites-blacks and whites-Hispanics as early as ages 5 and 6. Their results show that 5 and 6 year old black males score, on average, 18% lower than their white counterparts, while Hispanic males score 16% lower. Both Hispanic and black females also exhibited a gap in achievement test scores, although the gap is not quite as pronounced as it is for males. Additionally, the authors find that the gap between blacks and whites tends to widen as children age. By age 13 to 14, the black-white test score gap is measured to be approximately 22% among males and 21% for females[5]. Hispanics show a much smaller change over time with the test score gap between Hispanic males and white males at ages 13 to 14 at 16%, similar to that found at earlier ages.

Todd and Wolpin (2004) use CNLSY79 data merged with school level data from three sources to study the determinants of child test scores in reading and math using the PIAT math and PIAT reading scores as the measure of achievement. They also find that skill gaps open at early ages and tend to widen as children age. Additionally, their results suggest that home inputs are an important determinant for child test scores and that equalizing (giving blacks and Hispanics the same level of home inputs as whites) home

[5] The authors do propose the reduction in human capital development resulting from expectations about possible future discrimination as a possible explanation for the widening of the skill gap for black children relative to whites. They test this hypothesis using survey responses by parents and children regarding expectations on whether or not the child will be enrolled in school the following year. Ultimately, they conclude that expectations data based on survey questions about future expectations are ambiguous and difficult to interpret.

inputs would close approximately 25% of the black-white test score gap and 30% of the Hispanic-white test score gap.

Fryer and Levitt (2005) extend on an earlier 2004 paper in which they found that the black-white test score gap for kindergartners disappears once controls for a small number of covariates describing family background characteristics are added. They use a relatively new data set, the Early Childhood Longitudinal Study (ECLS), a nationally representative sample of over 20,000 children entering kindergarten in 1998 that includes 1000 schools, to extend their study through the 3rd grade. Their findings suggest that, while controlling for a small number of covariates erases the gap for incoming kindergarteners, by the 3rd grade the black-white test score gap in both math and reading widens again by an average of .1 standard deviations per year. These gaps that exist for older students cannot be explained in the data, even after controlling for observable characteristics. One plausible explanation suggested by the authors is differences in school quality between minorities and whites. However, they find that many of the blacks in the sample attend the same schools as whites and still experience a widening of test score gaps. Additionally, Hispanics tend to attend similar schools and do not experience a similar pattern in test score gap widening. They find no other empirical evidence to explain the widening of black-white test score gaps as children progress through school.

Lundberg and Startz (2000) provide a review of recent literature on discrimination and highlight the importance of focusing on incentives for human capital development among minorities as a means of understanding the perpetual gap in skill levels that exist between races. Building upon previously developed models of statistical discrimination (Lundberg and Startz (1983)) and racial segregation (Lundberg and Startz (1998)) they illustrate how skill gaps can persist even in the absence of current labor market discrimination. They point out that if individual investments in human capital depend, to some degree, on the expected return to that investment then the belief among minorities that they will be discriminated against or face lower future opportunities due to potential employer perceptions that minorities, on average, have a lower level of skill than their white counterparts, can result in a reduction in investment for their own human capital development as children, regardless if their expectations are accurate, and therefore create a self-fulfilling prophecy. It is this concept concerning the potential importance of future expectations on human capital development that serves as the theoretical economic framework for the current paper.

Recent Literature Related to the Impact of Affirmative Action on University Applications and SAT Scores

There exists a growing literature on the impact of changes in affirmative action policy on decisions regarding applications to universities among minorities and minority SAT scores. This line of literature is useful to mention because it strongly suggests that changes in affirmative action policy regarding public universities are an important enough issue that it does directly alter the behavior of minority children as they are preparing to enter college.

Looking at the university application behavior of minority high school students relative to non-minority students Mark Long (2003) uses the change in affirmative action policies in California and Texas to test if it resulted in changes in where minorities sent applications. He finds that the gap in SAT score reports sent by non-minority and minority students to in-state public universities did in fact widen significantly during this time period. Asking a similar question, Card and Krueger (2004) looked at the change in behavior among highly qualified minority applicants, those not directly affected by the policy changes, in California and Texas and found no change in application behavior among this group.

In looking at changes occurring in Washington State in response to the passing of Initiative 200, which eliminated the use of affirmative action in that state, Brown and Hirschman (2006) examine the effect on minorities transitioning into college. They find that the proportion of minority high school students moving from high school to college declined temporally in the years following the passage of the initiative. They further find that this reduction seemed to be more the result of a decrease in application rates among minorities than due to a reduction in admission rates.

Furstenberg (2005), using a similar methodology as used in this paper, examines the impact of the affirmative action policy changes in California and Texas on SAT scores for college bound minority students relative to whites. He finds a significant drop in relative SAT test scores among blacks in Texas and Hispanics in California. The paper suggests that minority test scores are affected by changes in the affirmative action landscape, however the use of SAT scores does pose some potential problems with interpreting the results in terms of the effect on human capital development due to some selection issues. The test measure used, SAT scores, is directly linked to the college admissions process and it is not immediately clear whether an increase in the SAT score gap is the result of reduced effort on the part of minorities to invest in human capital or if they are selectively under-performing on an exam used exclusively for college admissions. Additionally, the use of SAT scores may understate the effect given that the test is optional and some may simply opt out of taking the exam.

3. Recent Changes in Affirmative Action Policy with regards to University Admissions

In the landmark 1978 decision on Bakke vs the Regents of the University of California, involving the challenge to the University of California at Davis Medical School's admissions policies that included a separate admissions program for minorities, the Supreme Court established the legality of using race as a factor in college admissions. The ruling stated that colleges and universities may use admissions programs that take into account race in order to help foster diversity, however they could not use separate admissions procedures for minority candidates or quotas. This ruling set the stage for the inclusion of race in the decision making process for many academic admissions programs for the following 20 years.

On March 18, 1996 the US Court of Appeals for the Fifth Circuit, covering Louisiana, Mississippi, and Texas, ruled on Hopwood vs Texas involving a challenge to the University of Texas Law School admissions policies that included targeted percentages of Hispanic and African American students. The court held that the affirmative action programs used at the University of Texas were unconstitutional and that educational diversity should not be recognized as a compelling state interest. The Supreme Court refused to hear the case, effectively overturning the earlier Bakke decision and making the use of race in admissions policies illegal in the 5th circuit. In 1997 the Texas Attorney General announced that all universities in Texas should adopt a race-neutral admissions criteria, which was followed by a state law setting uniform admissions policies for all universities within Texas. This law forbade the explicit use of race in admissions policies, but did include the automatic admission into public universities in Texas for all high school seniors graduating in the top 10% of their graduating class.

Similar court cases occurred in the states of Georgia and Michigan. In 2001 a federal judge ruled in Johnson vs Board of Regents of the University of Georgia that using race as a factor in admissions in order to achieve a diverse student body is unconstitutional. In Michigan a pair of cases were brought before the courts regarding the use of race in admissions policies. In Gratz vs Bollinger (2000), a case involving the use of awarding admission bonus points for minority applicants at the University of Michigan was deemed permissible by a federal judge. In Grutter vs Bollinger (2001), however, a different federal judge ruled in the opposite direction with regard to the University of Michigan's Law School admissions policy. This latter case was ultimately appealed to the US Supreme Court where, on June 23 2003, the court upheld the University of Michigan's Law School admission policies. This decision effectively overturned the Hopwood vs Texas decision.

In 1996 voters in California passed proposition 209, which banned all California affirmative action programs in public college admissions, government hiring, and government contracting. The proposition took effect on November 3rd 1997, after being delayed in the courts for almost a year. A similar law was passed in the State of Washington where voters passed Initiative 200 in November of 1998. The initiative prohibited the use of race as a factor in college admissions and state hiring and went into effect December 3, 1998.

In 2001 the State of Florida passed a law that banned race and gender preferences in college admissions. The law was the ultimate product of a 1999 announcement by Governor Jeb Bush that he intended to sign an executive order banning racial preferences in state employment, contracting, and higher education. Included in the law was a provision that guarantees high school seniors admission into one of the state's public universities if they graduate in the top 20% of their graduating class.

4. Theoretical Framework

As a theoretical basis to explain the intuitive mechanism through which changes in affirmative action policies may alter human capital development we propose a simple labor-leisure choice model of the form:

$$\text{Max } U = U(Y,L)$$

$$\text{s.t. } Y = rH + Y^o \qquad \text{(budget constraint)}$$

$$\text{s.t. } H = T - L \qquad \text{(time constraint)}$$

Where Y is the discounted sum of all future earnings, L is time spent in leisure activities, H is time spent on human capital investment, Y^o consists of factors affecting lifetime earnings unrelated to the stock of human capital, T is the total time available to split between leisure activities and human capital investment, and r represents the expected rate of return to human capital investment.

It is fairly straight forward to show that a change in the expected rate of return to human capital investment (r) may result in either an increase or decrease in the level of human capital investment depending on whether the accompanying income or substitution effect dominates. In the context of a reduction in the rate of return (r), time spent in leisure becomes relatively less costly resulting in a substitution away from time spent in human capital investment. However, it now becomes necessary to invest more time in order to overcome the reduction in (r) and achieve the same level of lifetime earnings.

In a broad sense, perceived labor market discrimination would result in a decrease in the expected value of (r) if the individual believes that they will receive a lower level of compensation for comparable levels of human capital development. It is important to note that perception alone may result in a reduction in human capital investment, given that H is chosen prior to entering the labor market, and therefore create a self-fulfilling prophecy of lower levels of human capital development among certain minority groups despite identical distributions of innate ability. In the context of the current study, it is assumed that removal of affirmative action policies reduce the expected rate of return for human capital investment among minorities. This occurs as a result of the reduced likelihood, real or perceived, of attending college and the accompanying reduction in lifetime earnings as a result. Ultimately, the question of whether a lower expected rate of return to human capital investment among minorities can help explain the existing skill gaps that exist is an empirical question.

5. Identification Strategy

In the empirical analysis, two methods are used to test the hypothesis that changes in affirmative action laws affect the human capital development of minority children. First, the data were organized as pooled independent cross-sections broken down by age groups. These data were then analyzed using both difference-in-difference and difference-in-difference-in-differences methodologies. Second, the data were arranged as a panel and analyzed using an individual fixed effects model. In all cases, the outcome being measured is changes in math test scores among minority children relative to white children. In the analysis, post-policy refers to the years after changes in the affirmative action laws in California and Texas (1998, 2000, and 2002), while pre-policy refers to all years prior.

5.1 Pooled Independent Cross-Sections

The pooled independent cross-sectional analysis was performed using data broken down into two-year age groups (7 & 8, 9 & 10, 11 & 12, 13 & 14). The advantage of this approach is that it allows for an analysis of the possible effect of the policy change on the human capital development of children at different stages of their development that may otherwise be missed in the analysis if the entire sample was included in each regression. The two-year age cycle was selected instead of single age groups in order to increase sample sizes. Additionally, the two-year age groupings match the biannual cycle of the CNLSY79 survey and ensures that no individual enters the sample more than once per age group.

The use of the difference-in-difference (DD) and difference-in-difference-in-difference (DDD) methodologies are becoming more and more common in the analysis of policy changes in labor economics and other fields[6]. The basic idea of the DD estimator is to compare before and after changes between treatment and control groups. In the present context this implies comparing changes in math test scores between white and minority children before and after the change in affirmative action laws in California and Texas. The DDD estimator adds in an additional level of variation by also contrasting with non-affected states. The purpose of using this methodology is to control for unobservable characteristics or events that may be affecting both groups over time and

[6] Bertand et al. (2002) cite 92 articles using difference-in-difference methodology from 1990 to 2000.

would otherwise lead to biased results. To illustrate, suppose we have the following simple two period regression equation (see also table 1):

$$Y_{it} = C + B_1 Treatment + B_2 Year2 + B_3 Treatment*Year2 + v_{it} \quad (1)$$

Where Y_{it} is the outcome of interest for individual i in period t, C is a constant, *Treatment* is a dummy variable equal to one if the individual is part of the treatment group and zero otherwise, and *Year2* is a dummy variable equal to one if the observation is in the second of two periods. Assume that a policy change occurs in period two and we want to test the hypothesis that this policy change differentially affects the treatment group in some way. The interaction term, *Treatment*Year2* represents the differential effect of the policy change on the treatment group, therefore we want to determine if B_3 is significantly different from zero[7]. If the estimator is unbiased then the expected value of the estimator will be equal to the true effect of the policy.

$$E[B_3] = B_3 \quad (2)$$

The advantage of using this methodology relative to a standard regression becomes evident when we examine what may happen to our estimates under certain circumstances.

Suppose that instead of having both a treatment and a control group the policy is analyzed using only the treatment group before and after the policy change. In this case the equation explicitly being estimated would become:

$$Y_{it} = C + B_3 Year2 + v_{it} \quad (3)$$

Where the coefficient on the variable *Year2* is intended to capture the effects of the policy on the treatment group as they move from period 1 to period 2. The estimation, assuming that (1) remains the true model, would give us:

$$E[B_3] = E[Y_{2,treatment}] - E[Y_{1,treatment}]$$

[7] This interaction term is commonly referred to as the difference-in-difference estimator.

$$= [C + B_1 + B_2 + B_3] – [C + B_1]$$
$$= B_2 + B_3 \tag{4}$$

In this case, the resulting estimate for B_3 will be unbiased and equal to the true value of B_3 only in the case where B_2 is equal to zero. In other words, if a time trend existed in the data that affected Y_{it}, then the effect of this time trend would be captured as part of the treatment effect and bias the estimator.

Now suppose that we estimated the equation considering only post-treatment groups, i.e. looking at both treatment and control groups but ignoring pre-treatment observations. In this case we would be using a single cross-section in our analysis.

$$Y_i = C + B_3 Treatment + v_i \tag{5}$$

Where the coefficient on the variable *Treatment* is intended to capture the differences in outcomes for the treatment group relative to the control group. This estimation, again assuming (1) as the true model, would give us:

$$E[B_3] = E[Y_{i,treatment}] – E[Y_{i,control}]$$
$$= [C + B_1 + B_2 + B_3] – [C + B_2]$$
$$= B_1 + B_3 \tag{6}$$

In this case the estimator would be biased if B_1 did not equal zero, or if there existed differences between the treatment and control groups unrelated to the actual treatment being tested. These differences would be captured by the coefficient on treatment and therefore bias the results.

In the current context of attempting to measure the impact of changes in the affirmative action laws regarding university admission policies on the human capital development of minority children through changes in math test scores, it is likely that both types of biases would exist if we attempted to do the estimation using standard regression methods either by looking only at changes in minority test scores over time, or by using only a single cross-section that included both minority and white children. In order for these results to be unbiased we would have to assume that no time trends existed in the data other than the policy change, or that there existed no differences in test score results between whites and minorities that were unrelated to the policy changes. Clearly, the latter assumption is violated in this case and the former assumption is a rather strong one.

Fortunately, these issues can be addressed by employing the difference-in-difference estimator where:

$$\begin{aligned} E[B_3] &= (E[Y_{2,treatment}] - E[Y_{1,treatment}]) - (E[Y_{2,control}] - E[Y_{1,control}]) \\ &= ([C + B_1 + B_2 + B_3] - [C + B_1]) - ([C + B_2] - [C]) \\ &= B_2 + B_3 - B_2 \\ &= B_3 \end{aligned} \tag{7}$$

Thus, the difference-in-difference estimation theoretically gives an unbiased estimator for the effect of the policy change on the treatment group.

One of the key underlying assumptions with regards to the difference-in-difference and difference-in-difference-in-difference estimators, as it is used in the context of this study, is that there are no contemporaneous shocks that differentially affect the outcomes of the treatment group in the same states and years as the affirmative action policy changes. However, the effect of shocks, such as changes in school funding policies, that affect all children equally would be picked up by the time and/or state dummy variables included in the DD and DDD[8] regressions and should not lead to a biased estimator. A shock would have to directly or differentially affect a particular

[8] Note: A shock that affects a only a specific minority group on a national level would likely bias the DD estimator in the California and Texas only model, but should be controlled for in the DDD model that includes members of that minority group from other states as part of the control group.

minority group(s) in the treatment states in order to confound the effect picked up by the difference-in-difference estimator.

Another potential issue with using pooled independent cross sections in the DD/DDD framework is the assumption that the observable and unobservable characteristics of each group are the same across time periods. Differences in observable characteristics can be dealt with by including controls in the model, however unobservable characteristics pose a much greater difficulty. If, for instance, the unobservable level of innate ability of the treatment group prior to the policy change is different than that of the treatment group after the policy change then the DD/DDD estimator will likely be biased as a result. We will discuss this issue at greater length later in the paper when comparing results with those from the fixed effects estimation.

5.1.1. Difference-in-Difference Estimation: California and Texas

The initial method of estimation using the independent pooled cross-sections for two-year age groups was done using the difference-in-difference estimator on data from California and Texas where the treatment groups are minority children (black and Hispanic) and the control group consists of white children. The estimating equation follows the form:

$$Math_{it} = C + B_1(Black) + B_2(Hispanic) + B_3(Policy*Black) + B_4(Policy*Hispanic) + Y_1(Year_1) + Y_2(Year_2) + X_{it} + v_{it} \quad (8)$$

Where $Math_{it}$ is the math raw score for individual i in year t, C is a constant, *black* is a dummy variable equal to one if the individual is black and zero otherwise, *Hispanic* is a dummy variable equal to one if the individual is Hispanic and zero otherwise, *Policy* is a dummy variable equal to one if the observation occurs in the post policy change era (i.e.

1998, 2000, or 2002) and zero otherwise, and v_{it} is the error term. X_{it} is a vector of individual and family characteristics that includes a dummy variable for female, birth order of the child, the number of times the child has taken the test in previous years[9], highest grade achieved by the mother, a dummy variable for Urban equal to one if the child's residence is classified as an urban area within the NLSY data and zero otherwise, age of the child in months and age-squared, and grade of the child and grade-squared[10]. *Year1* is a vector of year dummies for the pre-policy years (1988, 1990, 1992, 1994, and 1996). The year 1986, the first year in the sample, is excluded and is represented by the constant term *C*. *Year2* is a vector of year dummies for the post-policy years (1998, 2000, and 2002). The year dummy variables were added to capture any year to year shocks that affect all children. Their inclusion allows for the intercept to differ across years.

In this specification, the coefficients on the interaction terms *Policy*Black* and *Policy*Hispanic* are the coefficients of primary interest. These coefficients capture the effect of the change in affirmative action laws on the relative math test scores for blacks and Hispanics respectively. A further illustration of the difference-in-difference procedure with regards to this specification of the model can be found in table 2.

5.1.2 Difference-in-Difference-in-Difference Estimation: US Sample

In order to add an additional layer of variation to the model, the rest of the United States is used as an additional comparison group. This specification allows for the control of race specific time effects that may be missed in the difference-in-difference model restricted to California and Texas only. If, for instance, black or Hispanic test scores are

[9] This variable was included as a control on the child doing better on the test as a result of increased familiarity with the testing format. A specification which also included a variable for the number of times taken squared to allow for a non-linear effect was also tested and found to be insignificant.
[10] In the final specification of the model, grade and grade^2 are used in place of age.

changing over time relative to whites as a national trend, then restricting the analysis to California and Texas may mistakenly capture this change as being the result of the policy changes in those states. Adding in minorities residing in non-policy states as an additional comparison group will control for this possibility[11]. The difference-in-difference-in-difference estimator is found using the following regression equation:

$$Math_{it} = C + B_1(Black) + B_2(Hispanic) + B_3(Caltex) + B_4(Policy*Black) + B_5(Policy*Hispanic) + B_6(Caltex*Black) + B_7(Caltex*Hispanic) + B_8(Policy*Caltex) + B_9(Policy*Caltex*Black) + B_{10}(Policy*Caltex*Hispanic) + Y_1(Year_1) + Y_2(Year_2) + X_{it} + v_{it} \quad (10)$$

Where $Math_{it}$ is again the math test score for individual i in year t, *Caltex* is a dummy variable equal to one if the individual lived in either California or Texas and zero otherwise, and all other variables are as previously defined. In this framework, the treatment group remains the minority children (black and Hispanic) in California and Texas. The coefficients on the interaction terms *Policy*Caltex*Black* and *Policy*Caltex*Hispanic*, the difference-in-difference-in-difference estimators, capture the relative change in math test scores among minority children as a result of the change in affirmative action policies in California and Texas. A further illustration of the difference-in-difference-in-difference procedure used in this regression can be seen in table 3.

Several states were excluded from the primary regressions run using this specification of the model due to issues concerning changes in affirmative action laws in those states that may have resulted in biased results. These states include Florida, Washington, Louisiana, and Mississippi. The impact of these exclusions will be discussed

[11] An example of this type of trend may be increased English proficiency among Hispanics over time that results in higher overall test scores. Failure to control for this would result in a misinterpretation of the difference-in-difference estimator.

further in the results section. An additional caveat should also be made regarding affirmative action laws with respect to university admissions policies. While the above mentioned states are the only states in which a change was actually made to affirmative action laws over the time-period under study, several additional states had some form of legislative challenge to the existing affirmative action policies[12]. In most cases, these proposals were dismissed prior to coming to vote, but it is difficult to say what impact, if any, the proposals alone may have had on the behavior of minorities within those states. As a result, we present both the difference-in-difference model for California and Texas alone and the difference-in-difference-difference model including the entire US in our analysis.

5.1.3. Difference-in-Difference Estimation: US Blacks

Analysis was also performed with a sample of just blacks before and after the policy changes using the following estimating equation:

$$Math_{it} = C + B_1(Caltex) + B_2(Policy*Caltex) + Y_1(Year_1) + Y_2(Year_2) + X_{it} + v_{it} \qquad (11)$$

In this case the treatment group is California and Texas blacks and the control group consists of all other blacks in the US[13]. The difference-in-difference estimator, *Policy*Caltex*, measures the change in math test scores among California and Texas blacks after the policy change relative to African-Americans elsewhere. Table 4 shows a further explanation for this regression. While highlighting the possible changes using a more homogeneous sample, blacks relative to blacks, this approach does fail to offer controls for other policy changes or events that may have occurred in either California or

[12] For a more comprehensive listing of recent legislative challenges to state level affirmative action laws see: http://fairchance.civilrights.org/the_facts/reports/aa_state_2005.pdf

[13] Florida, Washington, Louisiana, and Mississippi were also excluded from the control sample for these regressions.

Texas alone that may be captured by the *Policy*Caltex* estimator and therefore bias the results. The authors recognize this particular criticism and this specification is offered as merely an alternative way to view the data and not as the primary estimation of interest regarding the hypothesis being tested.

5.2 Individual Fixed Effects

Of particular concern with using the independent pooled cross sections as the primary means of analyzing the impact of policy changes across time is the effect of innate ability and other individual, unobservable characteristics on test score outcomes. As previously mentioned, it is assumed that the distribution of these unobservable characteristics is roughly the same for each group before and after the policy changes. If this assumption holds true, then the difference-in-difference methodology will difference away these unobservable characteristics, however, if this assumption is not true then the resulting difference-in-difference estimator will be biased. In order to deal with this potential problem we organize the data as a panel of same individuals across time and estimate using an individual fixed effects model.

As with the difference-in-difference estimates, we estimate the fixed effects model using three sets of panel data: a sample that includes all races but is restricted to California and Texas only, a sample that includes all races for the entire US, and a US sample that is restricted to blacks only. In all cases we restrict the samples to individuals who had valid math test scores in both 1996, the year prior to the policy changes, and 2000[14]. Additionally, we restrict the age group to individuals who were age 12 to 14 in the 2000 sample. This age group represents the group for whom the policy changes are

[14] Fixed effects regressions were also run using 1998 as the post policy year, but did not have significant results.

most relevant. In the first estimation we further restricted our data to include only observations from California and Texas using the following equation:

$$Math_{it} = C + B_1(Policy) + B_2(Policy*Black) + B_3(Policy*Hispanic) + X_{it} + a_i + u_{it} \quad (12)$$

Where X_{it} is a vector of observable time varying individual characteristics, a_i includes the unobservable individual characteristics that are constant over time (the individual fixed effects), and all other variables are as previously defined. Estimating this equation using ordinary least squares estimation would include a_i as part of the error term and result in biased estimates for explanatory variables included in the model that are correlated with any time invariant, unobservable characteristics. Using first differencing[15] across the two time periods to eliminate any individual fixed effects yields the following estimating equation:

$$(Math_{i2000} – Math_{i96}) = B_1(Policy) + B_2(Policy*Black) + B_3(Policy*Hispanic) + (X_{i2000} – X_{i96}) + (u_{i2000} – u_{i96}) \quad (13)$$

This is essentially the same procedure as was used in the difference-in-difference estimation with independent pooled cross-sections except in this case we are using the same individuals overtime and is therefore more restrictive in terms of data requirements.

We use the same procedure to estimate a fixed effects model using a sample that consists of observations for all races from the entire US, and a sample that was restricted to blacks only. These yield the following first-difference estimating equations for the US and blacks only sample respectively:

$$(Math_{i2000} – Math_{i96}) = B_1(Policy) + B_2(Policy*Black) + B_3(Policy*Hispanic) + B_4(Policy*Caltex) + B_5(Policy*Caltex*Black) + B_6(Policy*Caltex*Black) + (X_{i2000} – X_{i96}) + (u_{i2000} – u_{i96}) \quad (14)$$

[15] When using only two periods, first differencing and fixed-effects give identical estimates and test statistics.

and

$$(Math_{i2000} - Math_{i96}) = B_1(Policy) + B_2(Policy*Caltex) + (X_{i2000} - X_{i96}) + (u_{i2000} - u_{i96}) \quad (15)$$

All variables are as previously defined. As with the cross-sectional analysis, Florida, Mississippi, Louisiana, and Washington State are excluded from the national sample in order to avoid complications due to including states that also had some changes in affirmative action policy.

6. Data and Summary Statistics

The empirical analysis was done using data from the National Longitudinal Survey of Youth 1979 cohort (NLSY79) and the Children of NLSY79 survey (CNSLY79). The NLSY79 is a nationally representative sample of 12,686 men and women between the age of 14 and 21 in 1979. Interviews were conducted annually from 1979 to 1994 and biannually since 1994. The CNLSY79, initiated in 1986, consists of all children born to NLSY79 mothers. As of 2000, the sample included 6,417 children between the ages of 1 and 29, approximately 40% of which were between the ages of 9 and 15, the specific age group of interest for the post-affirmative action sample. The great benefit of using this data set is that it provides a rich set of background variables for both mothers and children, and the same individuals are continuously surveyed throughout the entire period of interest. Access was granted to the restricted use version of the data that allowed us to put a state-identifier with each individual in the survey.

The CNLSY79 sample includes a number of cognitive assessment measures including the Peabody Individual Achievement Test (PIAT) for Mathematics. This assessment test was administered to all children in the sample between the ages of 5 and 14 during each wave of the survey. The particular advantage of using this test as a measure of cognitive ability is that it has been administered repeatedly to children over the age of five, providing a development profile for each child in the sample. These measures have been used elsewhere as a proxy for acquired skill level (Carneiro et al 2005, Todd and Wolpin 2004, Yoshikawa 1999, Aughinbaugh 2005, James-Burdumy 2005), and are believed to be reliable and valid measures of ability, highly correlated with other cognitive measures and achievement assessments[16].

The PIAT math test consists of 84 multiple-choice questions of increasing difficulty, beginning with elementary arithmetic and progressing to advanced concepts in geometry and trigonometry. Scoring for the assessment test is done using the following procedure. A “ceiling” is reached when the child answers five of seven questions incorrectly. The non-normalized raw scores were then calculated by using the “ceiling” value and subtracting any incorrect responses. The use of a “ceiling” in the grading procedure helps to mitigate the possibility that “correct” answers will be recorded for questions involving material beyond the child’s ability for which correct responses were given randomly by the child. Raw scores were used as opposed to percentile or age adjusted standardized scores in order to provide an absolute measure of ability that allows for the measurement of gains over time as children age. This particular measure of child ability is ideally suited for the measurement of changes in achievement as children age since, essentially, the same test is given to children in all age groups. Additionally, the PIAT math assessment has the advantage of having a fairly high completion rate within the CNLSY79 dataset. Among all children in the CNLSY79, 91.6 percent had a valid

[16] See CNLSY79 users guide for further description: http://www.bls.gov/nls/y79cyaguide/nlsy79cusg.htm

assessment score, and completion rates among minority children were 93.4 percent for Hispanics and 92.5 for black children of all ages[17].

Only children known to be residing with their mother at the time of the survey were included in the regression analysis. This was done to ensure that the mother's background information used in the analysis was relevant for the children included in the sample. We also excluded children who did not reside in the United States at the time of the survey since law changes that might affect these individuals are more difficult to determine. Observations were also omitted if the test score or age of the child was missing within the data.

The control variables include the number of times the child has taken the test, to account for increased familiarity with the testing format, gender, birth order, age and age squared of the child, grade and grade squared, urban dummy, level of mother's education, and race or ethnicity of the child. Race is defined as white, non-Hispanic black, and Hispanic. In all cases included in the sample the race/ethnicity of the child matched that of the mother and was consistently coded for each individual for all years in which they appear in the data. It should also be noted that while the NLSY79 is a representative sample, the CNLSY79 is not. To account for this, sample weights included in the CNLSY79 data were utilized when performing the regressions.

Summary statistics for various groups are shown in tables 5 - 12. Tables 5 - 7 give means and standard deviations for math test scores and covariates included in the model for blacks, Hispanics, and white children respectively who were 13 or 14 years old at the time of the survey. Each table is subdivided into four groups: the California and Texas sample prior to policy changes, the California and Texas sample after policy

[17] Source: CNLSY79 data user guide: http://www.bls.gov/nls/y79cyaguide/nlsy79cusg.htm

changes, the rest of the US sample prior to policy changes, and the rest of the US sample after policy changes. The summary statistics for the 13 and 14 year old age group are reported because this is the group for whom policy changes regarding college admissions are most relevant and, as it turns out, the group for whom significant results were found.[18] Summary statistics are also shown for California and Texas independently in tables 8 and 9. Tables 10 - 12 exhibit the summary statistics for the panel data sample used in the individual fixed effects estimation.

Figure 1 shows the mean PIAT math test score by race and age. The vertical distance between the black-white and Hispanic-white mean test scores represents the test-score gap and is more explicitly shown in figure 2[19]. Of note is the size of the gap that exists even as early as age six. Additionally, the black-white gap begins at roughly the same size as the Hispanic-white gap, but grows to be almost twice the size of the Hispanic-white gap by the age of 14.

Based on the patterns exhibited here, it does appear possible that there are similarities in the cause of the widening of the black-white and Hispanic-white test score gap as children age. The widening of the gap shows a very similar pattern for both Hispanics and blacks, however it is considerably more pronounced for blacks. Whatever is causing these gaps to widen with age is apparently either a more prominent factor among blacks, or there are additional factors involved with the widening of the black-white test score gap that do not appear to be present among Hispanics.

7. Empirical Results

[18] Summary statistics for the other age groups are also available upon request.

[19] The mean scores and test-score gap shown here closely resemble those found by Carneiro, Heckman, and Masterov (2005).

The estimation results suggest a consistent and significant negative impact among middle-aged (13 to 14 year old) black children in California and Texas across all specifications used in the pooled independent cross-sectional analysis. Additionally, in the individual fixed effects model, the impact on black children is also found to be negative and highly significant. Hispanic children, however, show a much smaller effect and, in almost all cases, a statistically insignificant outcome.

7.1 Empirical Results: Independent Cross Sectional Analysis

7.1.1 Empirical Results: Difference-in-Difference Estimation: California and Texas

The empirical results from the difference-in-difference estimation for California and Texas are presented in tables 13 and 14. Table 13 shows the results for the 13 and 14 year old cross sections. Column 1 gives estimates for a model specification that only includes race and policy variables, column 2 includes a set of year dummy variables, columns 3 and 4 add a set of variables for individual and family background characteristics, and column 5 replaces the age and age-squared variables with grade and grade-squared.

The signs for all standard explanatory variables are consistent with a priori expectations and previous literature. Both the black and Hispanic estimates are negative and strongly significant in all specifications of the model. This corresponds to the large black-white and Hispanic-white test score gaps that exist in general for these racial groups relative to white children. The coefficient on female is also significant at the 1% level and negative. Mother's high grade received is also strongly significant and, as expected, positively correlated with child test scores. The signs on birth order and urban are both negative but insignificant in these specifications. Additionally, estimates on child grade is positive but insignificant, likely due to lack of variation in the cross-sectional analysis.

The estimates for the effect of the policy change on math test scores for black children relative to whites is significant for all specifications and consistently very large and negative. The estimates suggest that, controlling for other factors, black children in the post-policy period score more than five points lower than their white counterparts[20]. This drop in relative test scores is in addition to the standard differences that exist between blacks and whites. This is fairly large impact considering that the test has a score range of 1 to 84, and the mean score achieved for this age range is roughly 50 to 55 points. This translates into test score that is approximately twenty percent lower for a black child relative to a white child, and a test score that is ten percent lower due to the policy change alone[21]. Estimates for the effect on Hispanic children show a consistent, negative coefficient of approximately –1.9, however this estimate is insignificant at any reasonable level for all specifications of the model used.

Table 14 shows estimation results for race and policy variables for all age groups estimated. These regressions correspond to the same model specification found in column 5 of table 13 and include all of the same covariates. Of note are the results for black and Hispanic. As expected, in all cases and all age groups, the coefficients are negative and highly significant at the 1% level. The estimated effect of the policy changes on black children remains negative for all age groups, but is insignificant in all cases except for the

[20] We cannot actually attribute causality to the changing of the affirmative action policy, but the results suggest that either the changes in the policies or some other events happening concurrently are resulting in a significant negative impact on black math test scores.
[21] These percentages factor in both the estimated effect of black (-5.480) and the estimated effect of the policy changes on blacks (-5.175)

elder group. The policy effects for Hispanic children, while negative, show no significant results for any age group.

Difference-in-difference estimation results for 13 to 14 year olds from Texas and California independently are shown in tables 15 and 16 respectively. The results for Texas are quantitatively very similar to the results from the combined California & Texas estimation. Some interesting differences from the combined model are the lack of significance for female and the significance on birth order, in both cases however the coefficients are negative as would be expected.

The estimate for the policy impact on blacks in Texas is very similar to those found in the combined regression and is significant in all specifications of the model. The estimate for the impact on Hispanic children is insignificant in all specifications, as it was in the combined model. Table 17 shows results for all age groups from Texas. In all cases the estimates are similar in sign, significance, and magnitude as those found in the combined model.

The results for California are shown in table 16. In terms of sign and magnitude these results also mirror the results from the combined and Texas models quite closely. However, the results are insignificant in this case, and accompanied by fairly large standard errors. The estimate for Hispanic remains consistently negative, but is insignificant for two of the model specifications. Similar for the estimate on the effect of the policy on blacks, which is insignificant in three of the five specifications. In both cases this may be due to relatively large standard errors as the coefficients themselves are very similar in magnitude to the previous models. The results for all California age groups are shown in table 18. With the exception of the previously noted deviations in significance, these estimates appear to be very similar to those found in Texas and the combined model. The consistency of magnitude and sign in the results for both the Texas and California only models with the combined model suggests that the overall results are not being driven by some unusual event occurring in either California or Texas alone.

7.1.2 Empirical Results: Difference-in-Difference-in-Difference Estimation: US

The empirical results from the difference-in-difference-in-difference estimation using the entire US sample, reported in table 19, are roughly similar to those from the California and Texas sample. The estimates for female, mother's education, and birth order are all highly significant and have the expected sign. Child grade and grade-squared is also highly significant in this model, this was not the case in the California and Texas model and is possibly due to higher levels of variation in the larger sample size. The coefficient on black is highly significant and large in magnitude, however the coefficient on Hispanic is only significant at the 10% level in this model and the magnitude is fairly small.

In this version of the model, the estimates for *policy*black* and *policy*Hispanic* capture the relative math test scores for blacks and Hispanics in the later years of the sample (1998 to 2002) for the entire US. A significant estimate here would suggest some sort of time trend, either positive or negative, in the test scores of blacks and Hispanics. These outcomes, however, are all insignificant. These results suggest that the potential problem of using the difference-in-difference estimation for California and Texas only in which a national time trend in test scores for either blacks or Hispanics not also experienced by whites being captured as part of the post-policy effect in the DD estimator may not be a significant issue. Of some interest, however, is the outcome for the *policy*caltex* variable. This captures the impact of the policy changes on whites in California and Texas. In the fully specified model the coefficient is positive and significant at the 10% level. The interpretation is that, holding other factors constant, white test scores increased slightly after the policy changes in California and Texas relative to test scores for whites in the rest of the US.

Similar to the California and Texas model, the coefficient for the impact of the change in affirmative action policy on California and Texas blacks (*policy*caltex*black*) is highly negative and significant. The effect on Hispanics is found to be insignificant in similar fashion to the previous models discussed. Table 20 shows estimation results for all age groups. Overall, the results here are fairly consistent with the California and Texas model.

It was mentioned previously that some assumptions were made with regard to which states should be included in the US sample due to complications regarding possible changes in the affirmative action landscape within those states. It was decided that four states, Florida, Louisiana, Mississippi, and Washington, should be excluded from the sample. Regressions were also run with these states included as part of the control group and are presented in table 21. The results are qualitatively very similar to the results from the primary regression. The coefficient on *policy*caltex*black* , the variable of primary interest, is slightly smaller than under the original model, as would be expected a priori when including states within the control group who have also experienced changes in affirmative action laws, but the level of significance remains the same. Estimation results from a sample that excludes Michigan in addition to the previously excluded states are also reported in table 21. The coefficient of interest is almost identical to that found in the primary regression.[22]

7.1.3 Empirical Results: Difference-in-Difference Estimation: US Blacks

Table 22 shows the results from the black only sample for ages 13 to 14, and table 23 gives results for all age groups. Of particular note is the drop in significance to the 10% level for policy blacks in California and Texas relative to blacks elsewhere. However, the coefficient is still fairly large in magnitude. These results suggest blacks in California and Texas are actually decreasing their human capital development as a result of the policy changes as opposed to being the result of increases in white test scores alone.

7.1.4 Empirical Results: Mother's Education

Given the consistent and highly significant impact of mother's education on math test scores, the US sample was sub-divided into two groups, those with mothers who have 13 or more years of education and those with mothers having 12 or fewer years of education. The intuition is that children whose mother has at least some higher education may be more likely to attend college themselves and therefore may be more responsive to changes in affirmative action policies restricted to the domain of higher education admittance. The empirical results for 13 and 14 year old children are shown in table 24 and summary of results for all age groups is shown in table25. The results show a large and significant negative impact of the policy on California and Texas blacks in the high mother's education group. The low mother's education group has a much smaller and insignificant coefficient among African-Americans. These results suggest that the

[22] Regressions were also run with the US sample in which observations for the year 1998 were removed. This was done to control for possible ambiguity of the timing of the effect. The coefficient for the effect of the policy on blacks remained significant at the 5% level.

negative impact found in the combined regressions is being driven by children whose mother has at least some college education. This finding is particularly interesting considering that this is the group that is most likely considering college as a future option and therefore the group for whom the policy change would likely have the most impact on perceived future prospects. Given this assumption, the insignificant results among blacks whose mother has only a high school education is not surprising since, for this group, the return to investment in human capital may not be altered by decreasing the opportunities for college admissions.

Of additional interest is the positive and mildly significant results for blacks nationally in the post-policy years (Policy*Blacks). This suggests that blacks whose mother has at least some college education have been gaining ground relative to whites in states where affirmative action policies have been maintained. This result, coupled with the large, negative impact in post-policy years for California and Texas, where affirmative action in university admissions has been removed, highlight the potential importance of these policies toward closing the achievement test score gap among African-Americans most likely to attend college.

7.2 Empirical Results: Individual Fixed Effects

It was mentioned previously that there may be a concern with using pooled independent cross-sections for this analysis given the important assumption that unobservable individual and family characteristics such as innate ability or family norms be similar for each group across time, particularly in the case where the sample size of the treatment group is relatively small. To deal with this issue the sample was arranged as a panel, following the same individuals across time from 1996, the control year, to 2000, the treatment year. This approach ensures that any time-invariant individual or family characteristics will be the same for both the pre and post-policy groups. Results for the individual fixed effects estimation are presented in tables 26, 27, and 28 for the US sample, California and Texas sample, and blacks-only sample respectively. Also reported are OLS estimates using the panel data structure[23].

The results for the US sample, table 26, show very little difference between the fixed effects estimates and the OLS estimates in terms of magnitude and significance for most of the variables. Of notable exception to this is the coefficient on mother's education, which is substantially larger in the OLS regression and highly significant. This, however, is most likely due to the time-invariant characteristic of this variable in the fixed effects model[24]. Another exception is the significance of the relevant policy variable for Hispanics (*policy*caltex*hispanic*). In the OLS estimate this is found to be significant at the 1% level, while the fixed effects model shows significance only at the 10% level.

The estimate for the effects of the policy on blacks is large and very significant for all specifications. This estimate is slightly larger and more significant than found in the cross-sectional analysis and adds support to these previous findings. The similarity in estimates between the fixed effects model and the pooled independent cross-sections model would suggest that the potential problem with possible differences in unobserved characteristics between groups in the independent cross sections is not a significant issue.

[23] Included in the OLS regressions are additional controls for race, CalTex, gender, urban, and birth order.
[24] Estimations were also done without this variable in the model and the results were quantitatively similar.

Estimates from a panel consisting of California and Texas residents only, as well as blacks only can be found in tables 27 and 28. These results are quantitatively very similar to those found in the fixed effects analysis for the US panel data sample.

8. Study Limitations and Other Issues:

There exist a number of possible limitations and issues with the data and analysis that deserve further attention with regards to the results found. Among these are the sample size used in the analysis, the change in demographics that has occurred over the time period of the study and how this might impact the estimation, and other possible causes for the apparent increase in the black-white test score gap that occurred in California and Texas around the time of the policy changes. I will discuss each of these in turn.

8.1 Sample Size

The estimates for the effects of the policy changes on blacks are accompanied with fairly large standard errors, likely as the result of relatively small sample sizes. Despite this, the coefficients remain significant in all specifications and models, in many cases at the 1% significance level, for the 13 to 14 year old age group. For younger black age groups and for Hispanics, the estimates are consistently negative but insignificant. If anything, it would seem that the standard errors resulting from the small sample size would tend to bias the significance of the results against the hypothesis being tested. The fact that strongly significant results are found for some black children despite these standard errors serves to strengthen the results found.

Additionally, the data for both the cross-sectional analysis and the fixed effects analysis were carefully checked for inconceivable drops in test scores over time or other outliers that may suggest a miscode in the data leading to biased or otherwise misleading results. This is particularly important given the small sample size for the treatment group, where a single outlier would much more significantly affect the results than it would in a much larger sample.

8.2 Possible Changes in Demographic Composition

Another concern involves changes in demographics in the affected states that coincide with the changes in policy. The concern here is that changes in demographics that may have occurred as a result of the policy would not be fully accounted for in the analysis as it has been performed, or that general changes incidental to the policy changes may have indirectly impacted the results. Figures 11 - 22 illustrate some demographic characteristics for California and Texas relative to the rest of the US using 1990 and 2000 census data.

These figures suggest that, while the growth rate among blacks in California is small relative to the rest of the US, this appears to be part of a larger trend affecting all races in California. Texas, however, shows a larger growth rate in the black and white populations than does the rest of the US.

These figures, while certainly illustrating a rather broad view of the situation, suggest that there have in fact been some changes in demographics, particularly in California, that may have an impact on the estimate results. However, these changes appear to be more the result of trends affecting all races, including whites, and are not exclusively centered around changes in minority demographics alone.

Careful observation of the interstate migration patterns within the data sample used also does not suggest any causal relationship between changes in the affirmative action policies and migration of minorities to other states. In fact, there were very few changes in state of residence for California and Texas minorities within the sample around the time of the policy changes and as many were observed to move into the affected states as there were moving out. Only three blacks in the sample changed state of residence from an “affected” state prior to the policy change to an “unaffected” state soon after the policy change, and these three all came from the same family. During the same time period, three blacks moved into the affected states shortly after the policy changes. Similar findings were observed among Hispanics. These numbers, both in terms of how small they are relative to the overall sample and the ambiguity in pattern, suggest that, within the sample used for estimation, the results were not significantly tainted by increased migration as a result of the policy changes.

8.3 Concurrent Policy Changes

It is always possible that the effects found could be the result of concurrent policy changes or events and not the result of the changes in policy regarding affirmative action laws. To a large degree, there is very little that can be done to control for this other than to acknowledge the possibility and discuss what types of policies may lead to these results. Common policies that could result in changes in math test scores include changes in school funding, changes in curriculum, or even changes in welfare laws. It is important to note, however, that any policy that affects children of all races equally would be controlled for in both the difference-in-difference or fixed effects estimations. What would be required are policies that differentially affect minorities. Further, given that significant results are found for black children, but not Hispanic children, any concurrent policy changes would have to differentially affect black children in a negative fashion in order to produce these results. Certainly a case could be made that funding changes may differentially affect inner-city schools and therefore have a greater impact, positive or negative, on minority children. In many cases, however, blacks and Hispanics go to the same schools and should be affected equally by such an outcome.

9. Conclusion and Discussion

Recent literature has demonstrated the importance of white-minority skill gaps in explaining wage differentials between races. It has also been shown that these skill-gaps are present at very early ages and tend to widen as children get older. Given the significant role that skill-gaps have in explaining wage inequality it is important, from both an economic and social standpoint, to better understand the causes of these gaps in order to help make informed policy decisions that can be effective at remedying this issue. In this paper we have attempted to empirically analyze one potential cause for the widening of skill gaps as children age. Namely, that minorities take into account their perceived negative expectations regarding future labor market and educational opportunities when making decisions about investment into the human capital development of minority children.

Using changes in affirmative action laws in California and Texas as a natural experiment, we find a significant, negative impact on 13 and 14 year old black children that is consistent across all specifications of the difference-difference and individual fixed effects models. We also find consistently negative, although not always significant, coefficients among black children in all age groups and specifications tested. Among Hispanics, the results are considerably less revealing with insignificant results in almost all cases.

Given the current literature on the topic of skill-gaps, these results do fit a plausible explanation for expectations playing a role in the widening of the skill-gap among black children relative to whites. The significant results for blacks but not Hispanics is consistent with findings in the literature that black-white test-score gaps widen at a much more significant rate than does the Hispanic-white test score gap. Additionally, given the characteristics of the policy change utilized in this paper, it makes sense that the strongest results should be found among the oldest group of children. While the intent of the study is to test for the relevance of future expectations in general on decision making regarding human capital investment among minority children, the natural experiment used in the study specifically targets only a single domain of future opportunities; that of admissions into public universities. As a result, the a priori expectations would be that older children should have a stronger response given that these policy changes are more relevant to their immediate future than they are for younger children. Further illustrating this point is the fact that significant results were also found among children whose mother has more than 12 years of education, again the group for whom the policy changes are a more relevant issue.

We believe that these results, while not conclusive, do suggest that negative future expectations are playing a role in the decision-making process regarding human capital investment among blacks. What is not known is whether this is occurring through changes in behavior among the children themselves, or if parental inputs are being affected. If the former case is true, then this may help explain the widening of test-score gaps as black children age, but this explanation would offer very little insight into why these skill-gaps open up at such an early age given that it is fairly implausible that children would be formulating those types of expectations at that age. If the latter explanation is true, however, and minority parents are changing behaviors with regards to inputs into the human capital development of their children based upon their own perceptions about the future opportunities for their children, then this line of reasoning may in fact help to explain why minority children begin school so far behind their white counterparts. In any event, further research into this area would appear to be warranted.

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Table 1: General Difference-in-Difference Model

$Y_{it} = C + B_1(\text{Treatment}) + B_2(\text{Year2}) + B_3(\text{Treatment*Year2}) + v_{it}$

	Pre-Policy	Post-Policy	Difference
Treatment	$C + B_1$	$C + B_1 + B_2 + B_3$	$B_2 + B_3$
Control	C	$C + B_2$	B_2
Difference	B_1	$B_1 + B_3$	B_3

Table 2: Difference-in-Difference: California & Texas Only Sample

<u>Regression Model:</u>

$Math_{it} = C + B_1(\text{Black}) + B_2(\text{Hispanic}) + B_3(\text{Policy*Black}) + B_4(\text{Policy*Hispanic}) + Y_1(Year_1) + Y_2(Year_2) + X_{it} + v_{it}$

Where:
$Math_{it}$ = Math score for individual i in period t
Black = Dummy variable equal to 1 if individual is black
Hispanic = Dummy variable equal to 1 if individual is Hispanic
Policy = Dummy variable equal to 1 if period t is 1998 or beyond
$Year_1$ = Dummy variable equal to 1 if year is 1996 or prior
$Year_2$ = Dummy variable equal to 1 if year is 1998 or beyond

	Pre-Policy	Post-Policy	Difference
Blacks	$C + B_1 + Y_1$	$C + B_1 + B_3 + Y_2$	$B_3 + Y_2 - Y_1$
Whites	$C + Y_1$	$C + Y_2$	$Y_2 - Y_1$
Difference	B_1	$B_1 + B_3$	B_3

Table 3: Difference-in-Difference-in-Difference: US Sample

Regression Model:

$Math_{it}$ = C + B_1(Black) + B_2(Hispanic) + B_3(Caltex) + B_4(Policy*Black) + B_5(Policy*Hispanic) + B_6(Caltex*Black) + B_7(Caltex*Hispanic) + B_8(Policy*Caltex) + B_9(Policy*Caltex*Black) + B_{10}(Policy*Caltex*Hispanic) + Y_1($Year_1$) + Y_2($Year_2$) + X_{it} +v_{it}

Where:
$Math_{it}$ = Math score for individual i in period t
Black = Dummy variable equal to 1 if individual is black
Hispanic = Dummy variable equal to 1 if individual is Hispanic
Caltex = Dummy variable equal to 1 if individual resides in California or Texas
Policy = Dummy variable equal to 1 if period t is 1998 or beyond
$Year_1$ = Dummy variable equal to 1 if year is 1996 or prior
$Year_2$ = Dummy variable equal to 1 if year is 1998 or beyond

	Pre-Policy	Post-Policy	Difference
Policy State			
Blacks	$C+B_1+B_3+B_6+Y_1$	$C+B_1+B_3+B_4+B_6+B_8+B_9+Y_2$	$B_4+B_8+B_9+Y_2-Y_1$
Whites	$C+B_3+Y_1$	$C+B_3+B_8+Y_2$	$B_8+Y_2-Y_1$
Difference	B_1+B_6	$B_1+B_4+B_6+B_9$	B_4+B_9
Non-Policy State			
Blacks	$C+B_1+Y_1$	$C+B_1+B_4+Y_2$	$B_4+Y_2-Y_1$
Whites	$C+Y_1$	$C+Y_2$	Y_2-Y_1
Difference	B_1	B_1+B_4	B_4
			B_9

Table 4: Difference-in-Difference: Blacks Only Sample (CalTex & US Blacks)

Regression Model:

$Math_{it}$ = C + B_1(Caltex) + B_2(Policy*Caltex) + Y_1($Year_1$) + Y_2($Year_2$) + X_{it} +v_{it}

Where:
$Math_{it}$ = Math score for individual i in period t
Caltex = Dummy variable equal to 1 if individual resides in California or Texas
Policy = Dummy variable equal to 1 if period t is 1998 or beyond
$Year_1$ = Dummy variable equal to 1 if year is 1996 or prior
$Year_2$ = Dummy variable equal to 1 if year is 1998 or beyond

	Pre-Policy	Post-Policy	Difference
Policy State	$C + B_1 + Y_1$	$C + B_1 + B_2 + Y_2$	$B_2 + Y_2 - Y_1$
Non-Policy State	$C + Y_1$	$C + Y_2$	$Y_2 - Y_1$
Difference	B_1	$B_1 + B_2$	B_2

Table 5:Summary Statistics – Pooled Cross Sections: 13 and 14 Year Old Blacks

	CA & Tex Pre-Policy	CA & Tex Post-Policy	US Pre-Policy	US Post-Policy
Math Score	51.142	49.867	49.514	51.369
	(8.319)	(10.166)	(9.085)	(10.919)
Times Taken	3.839	4.578	3.872	4.599
	(1.227)	(.691)	(1.080)	(.755)
Mother's High	12.604	13.044	11.874	12.793
Grade	(1.254)	(1.770)	(1.864)	(1.984)
Child's Age	165.188	165.178	165.176	164.577
(Months)	(5.936)	(5.478)	(6.110)	(5.241)
Birth Order	1.472	2.089	1.538	2.213
	(.8072)	(.925)	(.803)	(1.113)
Female	53 (50%)	20 (44%)	289 (49%)	188 (53%)
Family Size	4.425	4.2	4.598	4.543
	(1.530)	(1.057)	(1.614)	(1.672)
Mother's Age	32.717	38.267	32.724	38.465
(Years)	(2.544)	(2.453)	(2.446)	(2.743)
Child's High	7.538	7.711	7.312	7.585
Grade	(.762)	(.869)	(.916)	(.778)
Mother's AFQT	19.731	17.070	17.600	20.833
	(16.348)	(16.218)	(16.741)	(18.738)
Observations	106	45	587	352

Note: Standard Deviations are in Parenthesis.

Table 6:Summary Statistics – Pooled Cross Sections:13 and 14 Year Old Hispanics

	CA & Tex Pre-Policy	CA & Tex Post-Policy	US Pre-Policy	US Post-Policy
Math Score	50.127	52.980	52.778	54.809
	(9.623)	(10.522)	(10.368)	(10.483)
Times Taken	3.968	4.51	3.685	4.496
	(1.109)	(.7851)	(1.233)	(.882)
Mother's High	10.507	12.382	10.926	12.183
Grade	(3.077)	(2.478)	(2.967)	(2.564)
Child's Age	165	164.26	165.080	165.052
(Months)	(6.212)	(4.769)	(6.264)	(5.363)
Birth Order	1.484	2.174	1.494	2.009
	(.7298)	(1.158)	(.741)	(.903)
Female	114 (52%)	69 (46%)	75 (46%)	51 (44%)
Family Size	5.091	4.658	4.383	4.443
	(1.607)	(1.245)	(1.159)	(1.371)
Mothers Age	33.453	38.779	32.975	38.826
(Years)	(2.354)	(2.607)	(2.487)	(2.647)
Child's High	7.458	7.730	7.536	7.635
Grade	(.875)	(.788)	(.949)	(.831)
Mother's AFQT	17.971	21.576	21.486	28.324
	(18.508)	(18.854)	(19.551)	(25.006)
Observations	221	149	162	115

Note: Standard Deviations are in Parenthesis.

Table 7: Summary Statistics – Pooled Cross Sections: 13 and 14 Year Old Whites

	CA & Tex Pre-Policy	CA & Tex Post-Policy	US Pre-Policy	US Post-Policy
Math Score	56.205	60.717	56.440	58.793
	(11.233)	(10.068)	(10.083)	(10.562)
Times Taken	4.133	4.717	4.045	4.688
	(1.135)	(.715)	(1.075)	(.689)
Mother's High	11.952	13.364	11.982	13.343
Grade	(2.241)	(2.270)	(1.882)	(2.139)
Child's Age	164.771	164.899	165.130	164.426
(Months)	(6.27)	(5.384)	(6.200)	(5.388)
Birth Order	1.566	1.909	1.391	1.816
	(1.084)	(.949)	(.677)	(.947)
Female	35 (42%)	49 (49%)	284 (51%)	270 (47%)
Family Size	4.446	4.253	4.380	4.319
	(2.038)	(1.256)	(1.242)	(1.213)
Mothers Age	33.325	39.040	33.771	39.347
(Years)	(2.475)	(2.810)	(2.563)	(2.678)
Child's High	7.575	7.778	7.461	7.570
Grade	(.868)	(.679)	(.854)	(.745)
Mother's AFQT	39.974	49.194	42.300	50.707
	(25.976)	(24.548)	(24.671)	(26.726)
Observations	83	99	555	571

Note: Standard Deviations are in Parenthesis.

Table 8:Summary Statistics – Pooled Cross Sections: 13 and 14 Year Old Texas

	Blacks Pre	Blacks Post	Hispanics Pre	Hispanics Post	Whites Pre	Whites Post
Math Score	51	50.828	50.343	54.719	57.25	62.147
	(8.130)	(9.016)	(10.109)	(12.177)	(8.347)	(9.417)
Times Taken	3.769	4.483	4.123	4.544	3.929	4.5
	(1.235)	(.871)	(.999)	(.758)	(1.303)	(.826)
Mother's High	12.385	12.897	10.890	12.860	11.893	12.824
Grade	(1.208)	(1.398)	(2.430)	(1.757)	(2.409)	(2.367)
Child's Age	165.769	165.345	164.781	164.123	164.214	165.824
(Months)	(5.897)	(5.232)	(6.425)	(4.536)	(6.094)	(5.584)
Birth Order	1.477	1.966	1.438	2.140	1.429	2.059
	(.752)	(.731)	(.7635)	(1.202)	(.690)	(1.043)
Female	34 (52%)	13 (45%)	35 (48%)	27 (47%)	7 (25%)	20 (59%)
Family Size	4.169	4	4.945	4.702	3.857	4.029
	(1.219)	(1.035)	(1.755)	(1.195)	(.971)	(.969)
Mothers Age	32.477	38.379	33.233	38.526	33.607	39.059
(Years)	(2.306)	(2.541)	(2.288)	(2.626)	(2.671)	(2.730)
Child's High	7.524	7.690	7.111	7.526	7.222	7.735
Grade	(.780)	(.967)	(.881)	(.847)	(.892)	(.790)
Observations	65	29	73	57	28	34

Note: Standard Deviations are in Parenthesis.

Table 9:Summary Statistics –Pooled Cross Sections:13 and 14 Year Old California

	Blacks Pre	Blacks Post	Hispanics Pre	Hispanics Post	Whites Pre	Whites Post
Math Score	51.366	48.125	50.020	51.902	55.673	59.969
	(8.709)	(12.099)	(9.407)	(9.260)	(12.487)	(10.384)
Times Taken	3.756	4.625	3.878	4.457	4.036	4.492
	(1.410)	(.719)	(1.112)	(.818)	(1.17)	(1.032)
Mother's High Grade	12.951	13.313	10.318	12.087	11.981	13.646
	(1.264)	(2.330)	(3.342)	(2.804)	(2.173)	(2.183)
Child's Age (Months)	164.269	164.875	165.108	164.348	165.055	164.415
	(5.954)	(6.065)	(6.124)	(4.931)	(6.396)	(5.256)
Birth Order	1.463	2.313	1.507	2.196	1.636	1.831
	(.897)	(1.196)	(.714)	(.997)	(1.238)	(.894)
Female	19 (46%)	7 (44%)	79 (54%)	42 (46%)	28 (51%)	29 (45%)
Family Size	4.829	4.563	5.162	4.630	4.746	4.369
	(1.870)	(1.031)	(1.530)	(1.282)	(2.359)	(1.376)
Mothers Age (Years)	33.098	38.063	33.561	38.935	33.182	39.031
	(2.871)	(2.351)	(2.385)	(2.597)	(2.381)	(2.872)
Child's High Grade	7.561	7.75	7.634	7.857	7.755	7.8
	(.743)	(.683)	(.821)	(.724)	(.806)	(.618)
Observations	41	16	148	92	55	65

Note: Standard Deviations are in Parenthesis.

Table 10: Summary Statistics – Panel Blacks

	CA & Tex Pre-Policy	CA & Tex Post-Policy	US Pre-Policy	US Post-Policy
Math Score	36.950	47	34.581	50.325
	(10.999)	(11.064)	(10.897)	(10.679)
Times Taken	2.45	4.45	2.506	4.438
	(.510)	(.510)	(.526)	(.557)
Mother's High Grade	13.1	13.3	12.675	12.763
	(1.683)	(2.003)	(2.103)	(2.150)
Child's Age (Months)	108.2	157.35	107.813	156.956
	(13.481)	(14.016)	(10.193)	(10.322)
Birth Order	2.35		2.281	2.281
	(.988)		(1.128)	(1.128)
Female	14 (70%)		89 (56%)	
Family Size	5	4.4	4.713	4.588
	(1.717)	(1.429)	(1.665)	(1.728)
Mothers Age (Years)	34.15	38.25	34.438	38.563
	(1.872)	(1.943)	(2.273)	(2.303)
Child's High Grade	2.8	7.3	2.713	7.313
	(1.196)	(1.302)	(1.018)	(1.094)
Mother's AFQT	14.421		21.610	
	(11.067)		(18.943)	
Observations	20		160	

Note: Standard Deviations are in Parenthesis.

Table 11: Summary Statistics – Panel Hispanics

	CA & Tex Pre-Policy	CA & Tex Post-Policy	US Pre-Policy	US Post-Policy
Math Score	38	52.286	36.85	54.5
	(10.385)	(10.279)	(12.003)	(11.784)
Times Taken	2.529	4.429	2.5	4.4
	(.531)	(.579)	(.555)	(.709)
Mother's High	12.271	12.614	12.075	12.225
Grade	(2.874)	(2.850)	(2.433)	(2.326)
Child's Age	107.829	157.343	109.9	159.375
(Months)	(9.777)	(9.339)	(10.902)	(11.160)
Birth Order	2.143		1.975	1.975
	(.937)		(.660)	(.660)
Female	31 (44%)		16 (40%)	
Family Size	4.871	4.886	4.5	4.275
	(1.454)	(1.357)	(1.485)	(1.519)
Mothers Age	34.314	38.429	35.075	39.2
(Years)	(2.177)	(2.211)	(2.129)	(2.163)
Child's High	2.743	7.5	3.025	7.675
Grade	(.912)	(.959)	(.999)	(1.095)
Mother's AFQT	24.836		27.051	
	(21.026)		(27.081)	
Observations	70		40	

Note: Standard Deviations are in Parenthesis.

Table 12: Summary Statistics – Panel Whites

	CA & Tex Pre-Policy	CA & Tex Post-Policy	US Pre-Policy	US Post-Policy
Math Score	42.891	60.091	40.880	57.322
	(9.949)	(9.522)	(10.570)	(10.575)
Times Taken	2.582	4.545	2.508	4.463
	(.534)	(.571)	(.520)	(.567)
Mother's High	13.564	13.691	13.293	13.410
Grade	(1.960)	(2.089)	(2.016)	(2.074)
Child's Age	108.673	158.182	107.459	156.850
(Months)	(10.147)	(9.963)	(10.195)	(10.322)
Birth Order	2.018		1.925	
	(1.045)		(1.037)	
Female	28 (51%)		151 (49%)	
Family Size	4.673	4.455	4.427	4.362
	(1.203)	(1.260)	(1.201)	(1.121)
Mothers Age	35.018	39.055	35.228	39.342
(Years)	(2.257)	(2.272)	(2.102)	(2.111)
Child's High	2.873	7.491	2.632	7.270
Grade	(.982)	(.940)	(.966)	(1.039)
Mother's AFQT	50.745		53.808	
	(25.036)		(26.768)	
Observations	55		307	

Note: Standard Deviations are in Parenthesis.

Figure: 1

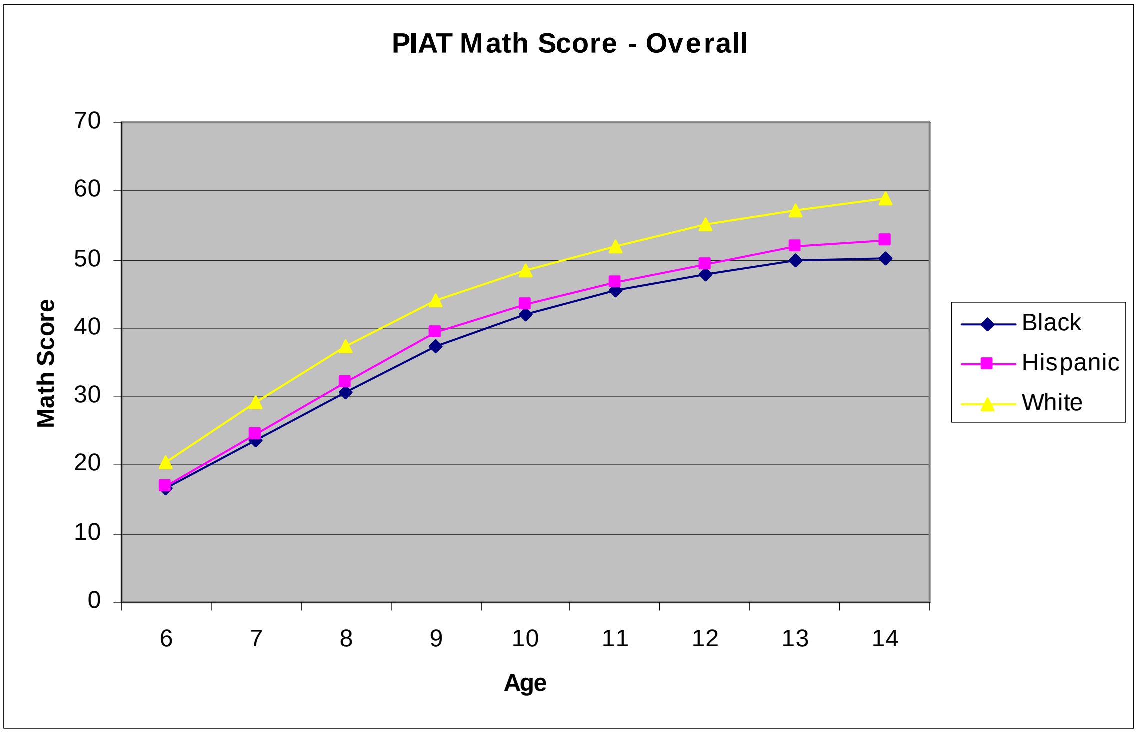


Figure: 2

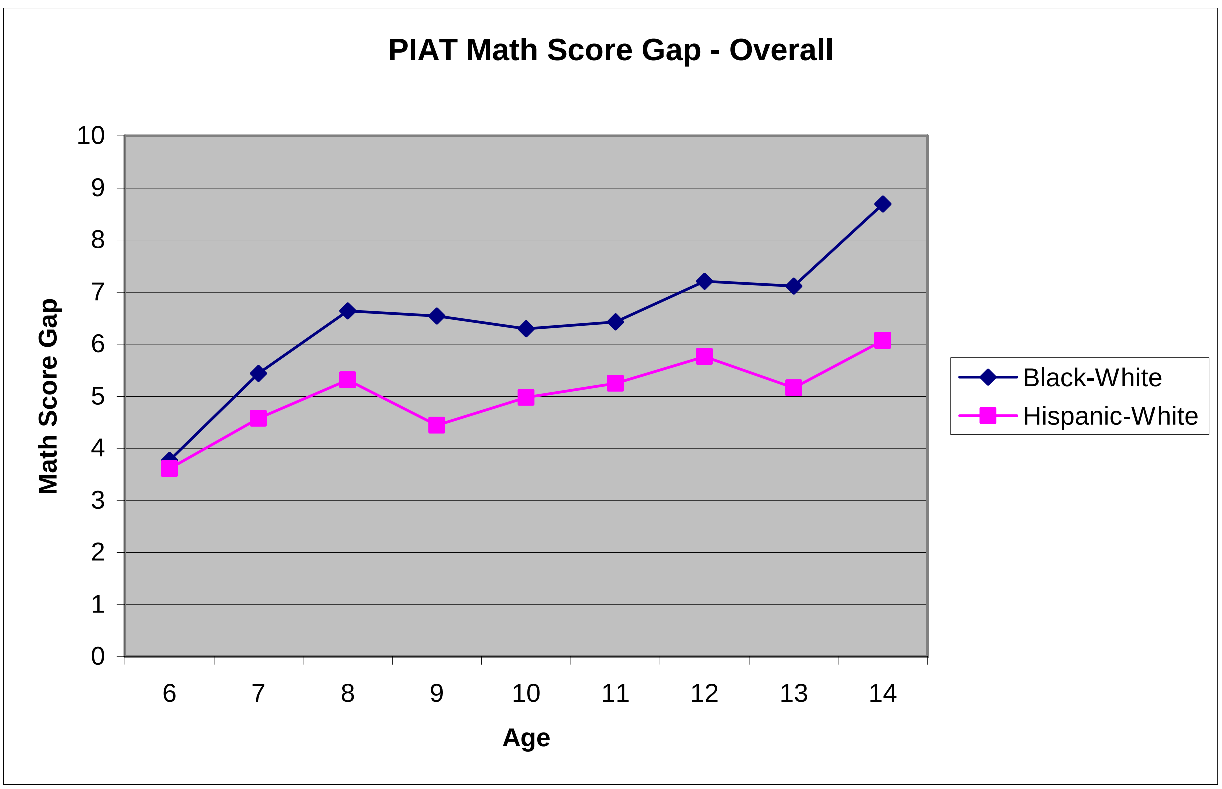


Table 13: Difference-in-Difference Estimation – California & Texas 13 & 14

	(1)	(2)	(3)	(4)	(5)
Black	-4.821	-5.276	-5.028	-5.768	-5.480
	(1.881)**	(1.800)***	(1.784)***	(1.549)***	(1.555)***
Hispanic	-5.986	-6.338	-6.153	-4.211	-4.226
	(1.837)***	(1.774)***	(1.766)***	(1.506)***	(1.472)***
Policy	4.929				
	(1.954)**				
Policy*Black	-6.765	-6.411	-6.755	-4.769	-5.175
	(2.485)***	(2.493)**	(2.545)***	(2.345)**	(2.369)**
Policy*Hispanic	-1.904	-1.630	-1.907	-1.901	-1.992
	(2.261)	(2.272)	(2.316)	(1.980)	(1.970)
Year88		9.603	8.603	7.171	2.355
		(3.212)***	(3.067)***	(3.301)**	(4.119)
Year90		5.932	4.254	2.031	-3.362
		(2.705)**	(2.955)	(3.299)	(3.366)
Year92		3.356	1.013	-1.656	-5.925
		(2.978)	(3.372)	(3.629)	(3.005)**
Year94		7.021	3.695	0.764	-3.534
		(2.816)**	(3.795)	(4.011)	(2.501)
Year96		6.062	2.928	-0.358	-4.321
		(2.870)**	(3.849)	(4.116)	(2.352)*
Year98		9.947	7.140	2.816	-1.436
		(3.050)***	(4.148)*	(4.205)	(1.850)
Year2000		10.433	7.485	2.519	-2.273
		(3.094)***	(4.265)*	(4.455)	(1.926)
Year2002		11.333	8.432	3.981	0.000
		(3.890)***	(4.839)*	(4.882)	(0.000)
Times Taken			0.757	0.690	0.564
			(0.839)	(0.762)	(0.745)
Female			-2.457	-2.458	-2.415
			(1.066)**	(0.886)***	(0.916)***
Mother's High Grade				1.319	1.294
				(0.231)***	(0.239)***
Urban				-2.398	-1.951
				(1.394)*	(1.394)
BIRTH ORDER				-0.901	-0.930
				(0.589)	(0.617)
Age			-1.469	-2.415	
			(5.858)	(5.387)	
Age^2			0.005	0.007	
			(0.018)	(0.016)	
Grade					2.435
					(5.816)
Grade^2					-0.066
					(0.388)
Constant	56.151	50.571	169.799	238.577	31.978
	(1.679)***	(2.814)***	(481.732)	(442.590)	(22.275)
Observations	703	703	703	703	690
R-squared	0.16	0.17	0.19	0.28	0.29

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 14: Difference-in-Difference Estimation – California & Texas – All Ages

Age Group	7 & 8	9 & 10	11 & 12	13 & 14
Black	-5.856	-4.772	-5.114	-5.480
	(1.014)***	(0.944)***	(1.121)***	(1.555)***
Hispanic	-4.272	-3.397	-4.876	-4.226
	(0.819)***	(0.816)***	(0.909)***	(1.472)***
Policy*Black	-2.752	-0.923	-2.258	-5.175
	(1.898)	(1.575)	(1.808)	(2.369)**
Policy*Hispanic	-0.166	-2.184	-1.396	-1.992
	(1.584)	(1.458)	(1.404)	(1.970)
Observations	1136	1113	998	690
R-squared	0.33	0.30	0.25	0.29

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 15: Difference-in-Difference Estimation - Texas 13 & 14

	(1)	(2)	(3)	(4)	(5)
Black	-5.826	-6.048	-6.404	-7.021	-6.967
	(1.881)***	(1.910)***	(1.985)***	(1.840)***	(1.821)***
Hispanic	-6.933	-7.533	-7.606	-5.773	-5.290
	(2.055)***	(2.036)***	(2.100)***	(1.937)***	(1.860)***
Policy	5.318				
	(2.230)**				
Policy*Black	-6.201	-6.432	-6.141	-5.614	-5.781
	(2.804)**	(3.079)**	(3.141)*	(2.698)**	(2.732)**
Policy*Hispanic	-0.322	-0.216	0.095	-1.735	-2.201
	(3.046)	(3.270)	(3.331)	(2.866)	(2.893)
Year88		4.799	3.020	2.534	-2.659
		(3.757)	(3.816)	(4.127)	(3.596)
Year90		6.548	3.100	2.736	-2.277
		(3.041)**	(3.320)	(4.054)	(3.272)
Year92		2.184	-1.393	-3.015	-7.226
		(3.305)	(3.665)	(4.273)	(2.113)***
Year94		6.420	1.106	-1.353	-6.165
		(3.324)*	(3.973)	(4.552)	(2.647)**
Year96		7.168	2.131	-0.809	-5.662
		(3.336)**	(4.153)	(4.601)	(2.524)**
Year98		11.883	6.852	4.895	0.000
		(3.820)***	(4.355)	(4.205)	(0.000)
Year2000		8.890	3.628	1.844	-3.570
		(3.829)**	(4.631)	(4.833)	(2.375)
Year2002		12.678	7.747	4.652	-0.238
		(5.741)**	(5.557)	(5.166)	(2.734)
Times Taken			1.019	1.009	1.045
			(0.854)	(0.692)	(0.660)
Female			-0.851	-1.789	-1.996
			(1.368)	(1.159)	(1.220)
Mother’s High Grade				1.459	1.392
				(0.239)***	(0.253)***
Urban				-0.961	-0.563
				(2.028)	(2.055)
BIRTH ORDER				-1.584	-1.637
				(0.668)**	(0.671)**
Age			-2.682	-2.424	
			(5.670)	(5.923)	
Age^2			0.009	0.008	
			(0.017)	(0.018)	
Grade					4.660
					(8.254)
Grade^2					-0.211
					(0.557)
Constant	56.784	51.455	253.829	227.867	22.955
	(1.600)***	(3.301)***	(469.313)	(489.179)	(30.875)
Observations	286	286	286	286	282
R-squared	0.22	0.25	0.28	0.40	0.40

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 16: Difference-in-Difference Estimation - California 13 & 14

	(1)	(2)	(3)	(4)	(5)
Black	-3.826	-4.742	-4.736	-5.221	-4.629
	(2.911)	(2.791)*	(2.628)*	(2.355)**	(2.328)**
Hispanic	-5.449	-5.819	-5.752	-3.234	-3.258
	(2.627)**	(2.543)**	(2.359)**	(2.085)	(2.045)
Policy	4.700				
	(2.775)*				
Policy*Black	-8.324	-7.035	-6.967	-4.263	-4.936
	(4.183)**	(4.233)*	(4.258)	(3.988)	(3.967)
Policy*Hispanic	-2.978	-2.544	-2.676	-1.680	-1.836
	(3.057)	(3.035)	(2.898)	(2.467)	(2.493)
Year88		12.266	10.286	5.965	0.000
		(3.148)***	(2.982)***	(3.170)*	(0.000)
Year90		6.557	5.116	-1.107	-8.887
		(2.204)***	(2.556)**	(2.995)	(2.857)***
Year92		5.184	2.823	-3.433	-9.540
		(3.401)	(3.872)	(3.982)	(3.415)***
Year94		8.578	5.873	-0.397	-6.456
		(2.667)***	(4.308)	(4.266)	(3.934)
Year96		6.628	3.825	-2.628	-7.750
		(2.637)**	(4.308)	(4.314)	(4.127)*
Year98		10.222	7.216	-1.177	-6.576
		(1.665)***	(4.093)*	(4.406)	(4.578)
Year2000		12.682	9.425	0.050	-6.109
		(1.824)***	(4.175)**	(4.586)	(4.918)
Year2002		11.525	9.141	0.480	-4.812
		(2.619)***	(4.918)*	(5.103)	(5.618)
Times Taken			0.498	0.414	0.236
			(1.175)	(1.047)	(1.060)
Female			-3.364	-3.075	-2.645
			(1.451)**	(1.222)**	(1.254)**
Mother's High Grade				1.265	1.293
				(0.309)***	(0.320)***
Urban				-4.469	-4.240
				(1.927)**	(1.895)**
BIRTH ORDER				-0.564	-0.677
				(0.766)	(0.830)
Age			-2.156	-2.403	
			(8.138)	(7.611)	
Age^2			0.006	0.007	
			(0.025)	(0.023)	
Grade					2.282
					(7.960)
Grade^2					-0.026
					(0.529)
Constant	55.786	49.000	238.368	251.565	36.356
	(2.464)***	(0.000)***	(668.517)	(624.712)	(29.731)
Observations	417	417	417	417	408
R-squared	0.13	0.15	0.17	0.26	0.28

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 17: Difference-in-Difference Estimation – Texas – All Ages

Age Group	7 & 8	9 & 10	11 & 12	13 & 14
Black	-6.491	-4.645	-5.110	-6.967
	(1.567)***	(1.061)***	(1.528)***	(1.821)***
Hispanic	-4.844	-3.691	-5.347	-5.290
	(1.462)***	(1.101)***	(1.377)***	(1.860)***
Policy*Black	-1.239	-0.376	-2.891	-5.781
	(3.174)	(2.204)	(2.308)	(2.732)**
Policy*Hispanic	-1.601	-0.761	-1.747	-2.201
	(2.985)	(2.176)	(1.989)	(2.893)
Observations	459	455	410	282
R-squared	0.33	0.36	0.34	0.40

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 18: Difference-in-Difference Estimation – California – All Ages

Age Group	7 & 8	9 & 10	11 & 12	13 & 14
Black	-5.434	-5.799	-5.734	-4.629
	(1.432)***	(1.556)***	(1.652)***	(2.328)**
Hispanic	-4.045	-3.260	-4.336	-3.258
	(1.031)***	(1.023)***	(1.310)***	(2.045)
Policy*Black	-3.706	-0.977	-2.858	-4.936
	(2.562)	(2.221)	(2.705)	(3.967)
Policy*Hispanic	0.739	-2.919	-1.000	-1.836
	(1.845)	(1.947)	(1.967)	(2.493)
Observations	677	658	588	408
R-squared	0.35	0.31	0.25	0.28

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 19: Difference-in-Difference-in-Difference Estimation - US 13 & 14

	(1)	(2)	(3)	(4)	(5)
Black	-8.256	-7.162	-7.126	-6.811	-6.511
	(0.571)***	(0.674)***	(0.676)***	(0.682)***	(0.642)***
Hispanic	-4.929	-3.723	-3.420	-2.023	-1.851
	(1.082)***	(1.142)***	(1.171)***	(1.135)*	(1.104)*
CalTex	-1.781	-0.591	-0.717	-0.315	-0.967
	(1.717)	(1.746)	(1.770)	(1.457)	(1.433)
Policy*Black	2.084	0.018	0.128	1.069	0.689
	(0.757)***	(1.014)	(1.024)	(0.998)	(0.981)
Policy*Hispanic	2.009	-0.249	-0.539	-0.128	-0.599
	(1.380)	(1.560)	(1.566)	(1.482)	(1.466)
CalTex*Black	3.435	2.170	2.338	1.122	1.116
	(1.961)*	(1.988)	(2.018)	(1.715)	(1.721)
CalTex*Hispanic	-1.057	-2.394	-2.486	-2.368	-2.390
	(2.127)	(2.158)	(2.204)	(1.952)	(1.898)
Policy*CalTex	4.929	3.033	3.150	2.815	3.104
	(1.948)**	(2.066)	(2.084)	(1.765)	(1.773)*
Policy*CalTex*Black	-8.849	-6.877	-7.147	-6.321	-6.251
	(2.589)***	(2.673)**	(2.713)***	(2.502)**	(2.509)**
Policy*CalTex*Hisp	-3.912	-1.737	-1.386	-1.724	-1.556
	(2.651)	(2.758)	(2.791)	(2.522)	(2.507)
Year92		2.807	-0.504	-0.034	-0.760
		(2.837)	(2.921)	(3.123)	(1.501)
Year94		3.951	0.129	0.354	-0.597
		(2.824)	(3.004)	(3.207)	(1.635)
Year96		3.653	-0.224	-0.186	-0.739
		(2.828)	(3.036)	(3.239)	(1.690)
Year98		4.917	1.059	0.571	-0.804
		(2.881)*	(3.088)	(3.295)	(1.787)
Year2000		4.724	0.670	0.060	-2.216
		(2.914)	(3.139)	(3.353)	(1.846)
Year2002		7.149	3.310	2.231	1.202
		(2.901)**	(3.102)	(3.335)	(1.816)
Times Taken			1.066	0.791	0.635
			(0.358)***	(0.333)**	(0.328)*
Female			-0.966	-0.981	-1.468
			(0.468)**	(0.441)**	(0.426)***
Mother's High Grade				1.191	1.108
				(0.126)***	(0.122)***
Urban				-0.444	-0.371
				(0.506)	(0.487)
BIRTH ORDER				-1.184	-0.914
				(0.283)***	(0.281)***
Age			-4.581	-3.993	
			(2.180)**	(2.109)*	
Age^2			0.014	0.012	
			(0.007)**	(0.006)*	
Grade					11.893
					(2.451)***
Grade^2					-0.612
					(0.163)***
Constant	57.933	53.216	424.038	364.092	-10.520
	(0.378)***	(2.826)***	(179.782)**	(173.924)**	(9.267)
Observations	3045	3045	3045	3045	2988
R-squared	0.09	0.11	0.12	0.18	0.22

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 20: Difference-in-Difference-in-Difference Estimation – US – All Ages

Age Group	7 & 8	9 & 10	11 & 12	13 & 14
Black	-5.823	-5.104	-5.373	-6.511
	(0.396)***	(0.432)***	(0.472)***	(0.642)***
Hispanic	-4.164	-3.707	-2.979	-1.851
	(0.599)***	(0.641)***	(0.795)***	(1.104)*
CalTex	-0.197	0.121	0.345	-0.967
	(0.717)	(0.701)	(0.801)	(1.433)
Policy*Black	0.910	-0.812	-1.340	0.689
	(0.797)	(0.748)	(0.724)*	(0.981)
Policy*Hispanic	-0.483	-0.999	-2.131	-0.599
	(1.114)	(1.315)	(1.310)	(1.466)
CalTex*Black	0.166	0.273	0.263	1.116
	(1.068)	(1.004)	(1.203)	(1.721)
CalTex*Hispanic	-0.089	0.434	-1.981	-2.390
	(1.019)	(1.023)	(1.196)*	(1.898)
Policy*CalTex	0.959	0.035	0.456	3.104
	(1.278)	(1.133)	(1.216)	(1.773)*
Policy*CalTex*Black	-3.662	0.467	-0.847	-6.251
	(2.017)*	(1.812)	(1.941)	(2.509)**
Policy*CalTex*Hispan	0.334	-0.808	0.711	-1.556
	(1.952)	(1.942)	(1.929)	(2.507)
Observations	5036	4950	4443	2988
R-squared	.28	.30	0.25	0.22

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 21: Difference-in-Difference-in-Difference Estimation - US

	All States	W/O Michigan	Primary Model
Black	-6.568	-6.662	-6.511
	(.602)***	(0.666)***	(0.642)***
Hispanic	-1.488	-1.897	-1.851
	(1.052)	(1.130)*	(1.104)*
CalTex	-.763	-0.995	-0.967
	(1.428)	(1.443)	(1.433)
Policy*Black	.270	0.751	0.689
	(.931)	(1.013)	(0.981)
Policy*Hispanic	-1.201	-0.512	-0.599
	(1.353)	(1.491)	(1.466)
CalTex*Black	1.181	1.251	1.116
	(1.709)	(1.733)	(1.721)
CalTex*Hispanic	-2.730	-2.305	-2.390
	(1.872)	(1.915)	(1.898)
Policy*CalTex	2.938	3.020	3.104
	(1.769)*	(1.785)*	(1.773)*
Policy*CalTex*Black	-5.793	-6.238	-6.251
	(2.492)**	(2.525)**	(2.509)**
Policy*CalTex*Hispan	-.935	-1.618	-1.556
	(2.448)	(2.522)	(2.507)
Observations	3287	2855	2988
R-squared	0.22	0.23	0.22

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 22: Difference-in-Difference Estimation - US Blacks 13 & 14

	(1)	(2)	(3)	(4)	(5)
CalTex	1.654	1.622	1.606	0.748	0.326
	(0.948)*	(0.973)*	(0.977)	(0.950)	(0.983)
Policy	2.084	3.553	1.667	1.424	2.987
	(0.757)***	(3.639)	(4.209)	(4.396)	(2.186)
Policy*CalTex	-3.920	-3.878	-3.799	-3.377	-3.186
	(1.705)**	(1.716)**	(1.713)**	(1.726)*	(1.725)*
Year88		-0.333	-0.871	-0.789	0.000
		(3.526)	(3.585)	(3.779)	(0.000)
Year90		4.273	3.383	3.339	4.108
		(3.489)	(3.661)	(3.866)	(1.627)**
Year92		2.088	0.619	0.706	2.104
		(3.389)	(3.787)	(3.972)	(1.718)
Year94		2.151	0.428	0.419	1.629
		(3.422)	(4.054)	(4.230)	(2.044)
Year96		2.974	1.095	1.051	2.453
		(3.426)	(4.086)	(4.257)	(2.080)
Year98		0.684	0.713	0.882	0.000
		(1.598)	(1.617)	(1.601)	(0.000)
Year2000		0.000	0.000	0.000	-1.566
		(0.000)	(0.000)	(0.000)	(1.612)
Year2002		2.139	2.274	2.488	1.875
		(1.641)	(1.645)	(1.610)	(1.215)
Times Taken			0.513	0.416	0.149
			(0.606)	(0.604)	(0.563)
Female			0.576	0.561	0.068
			(0.644)	(0.635)	(0.638)
Mother's High Grade				0.935	0.867
				(0.194)***	(0.192)***
Urban				0.960	0.868
				(0.680)	(0.655)
BIRTH ORDER				-0.917	-0.611
				(0.334)***	(0.330)*
Age			-1.685	-0.427	
			(3.021)	(3.032)	
Age^2			0.005	0.001	
			(0.009)	(0.009)	
Grade					10.284
					(4.234)**
Grade^2					-0.553
					(0.293)*
Constant	49.676	47.181	185.868	71.229	-8.420
	(0.429)***	(3.365)***	(250.513)	(251.401)	(15.793)
Observations	1090	1090	1090	1090	1072
R-squared	0.01	0.02	0.02	0.07	0.11

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 23: Difference-in-Difference Estimation – US – Blacks All Ages

Age Groups	7 & 8	9 & 10	11 & 12	13 & 14
CalTex	-0.125	0.661	0.854	0.326
	(0.780)	(0.724)	(0.908)	(0.983)
Policy	3.769	2.551	1.244	2.987
	(1.179)***	(1.299)**	(1.391)	(2.186)
Policy*CalTex	-2.574	0.231	-0.276	-3.186
	(1.526)*	(1.360)	(1.526)	(1.725)*
Observations	1519	1569	1497	1072
R-squared	0.28	0.25	0.18	0.11

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 24: Difference-in-Difference-in-Difference Estimation – US - Mother's Education

Mother's Education	12 or Less	13+
Black	-5.855	-8.695
	(0.716)***	(1.324)***
Hispanic	-2.632	-0.594
	(1.242)**	(2.311)
CalTex	-1.796	2.645
	(1.596)	(2.576)
Policy*Black	-0.405	3.227
	(1.217)	(1.703)*
Policy*Hispanic	-0.017	-2.056
	(1.751)	(2.892)
CalTex*Black	0.529	0.438
	(2.019)	(2.979)
CalTex*Hispanic	-0.832	-9.104
	(2.086)	(3.633)**
Policy*CalTex	3.381	-0.401
	(2.172)	(2.978)
Policy*CalTex*Black	-3.261	-7.779
	(3.221)	(3.932)**
Policy*CalTex*Hisp	-2.114	4.505
	(3.041)	(4.422)
Observations	2010	974
R-squared	0.18	0.21

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 25: Difference-in-Difference-in-Difference Estimation – US – All Ages

Age Group	7 & 8	9 & 10	11 & 12	13 & 14
Black	-6.820	-6.126	-6.048	-8.695
	(0.697)***	(0.787)***	(0.962)***	(1.324)***
Hispanic	-5.104	-5.582	-3.356	-0.594
	(1.130)***	(1.329)***	(1.543)**	(2.311)
CalTex	0.715	-0.846	1.131	2.645
	(1.258)	(1.389)	(1.482)	(2.576)
Policy*Black	2.989	0.144	-0.970	3.227
	(1.190)**	(1.139)	(1.257)	(1.703)*
Policy*Hispanic	2.561	4.287	-0.081	-2.056
	(1.733)	(2.128)**	(2.009)	(2.892)
CalTex*Black	-0.535	3.087	1.431	0.438
	(1.804)	(1.801)*	(2.019)	(2.979)
CalTex*Hispanic	-2.114	0.989	-4.159	-9.104
	(1.894)	(2.080)	(2.398)*	(3.633)**
Policy*CalTex	-0.420	0.066	-0.880	-0.401
	(1.762)	(1.787)	(1.777)	(2.978)
Policy*CalTex*Black	-5.196	-1.384	-1.664	-7.779
	(2.889)*	(2.840)	(2.601)	(3.932)**
Policy*CalTex*Hispan	1.190	-3.995	0.290	4.505
	(2.849)	(3.054)	(3.028)	(4.422)
Observations	1911	1795	1513	974
R-squared	0.28	0.26	0.22	0.21

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 26: US Panel

	(1)	(2)	(3)	(OLS)
Policy*Black	15.744	-0.571	-0.419	-0.403
	(1.170)***	(1.283)	(1.256)	(0.995)
Policy*Hispanic	17.650	2.108	2.747	2.583
	(2.154)***	(2.193)	(2.235)	(1.633)
Policy*CalTex	17.200	1.153	1.517	1.660
	(1.326)***	(1.452)	(1.484)	(1.237)
Policy*CalTex*Black	-22.894	-6.774	-7.105	-7.630
	(2.975)***	(2.629)**	(2.646)***	(2.089)***
Policy*CalTex*Hispanic	-20.564	-5.368	-5.913	-6.460
	(3.063)***	(3.064)*	(3.102)*	(2.400)***
Times Taken		0.843	0.911	0.649
		(2.146)	(2.197)	(0.805)
Age		1.030		
		(0.365)***		
Age^2		-0.004		
		(0.000)***		
Mother’s High Grade		0.407	0.457	1.275
		(0.876)	(0.773)	(0.189)***
Year2000		10.238	12.275	-0.872
		(17.045)	(5.932)**	(1.678)
Grade			4.520	7.691
			(0.953)***	(0.646)***
Grade^2			-0.404	-0.428
			(0.051)***	(0.052)***
Constant	42.698	-37.972	21.701	7.474
	(0.195)***	(39.900)	(12.087)*	(3.450)**
Observations	1304	1304	1304	1304
Number of ID CODE OF CHILD	652	652	652	
R-squared	0.38	0.79	0.79	0.55

Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

Table 27: California & Texas Panel

	(1)	(2)	(3)	(OLS)
Policy	17.200	2.766	25.030	3.473
	(1.337)***	(35.146)	(11.547)**	(3.406)
Policy*Black	-7.150	-6.881	-7.166	-7.313
	(2.758)**	(2.335)***	(2.375)***	(1.803)***
Policy*Hispanic	-2.914	-3.452	-3.527	-3.199
	(2.196)	(2.112)	(2.155)	(1.789)*
Times Taken		-5.881	-5.925	2.048
		(4.048)	(4.217)	(1.937)
Age		1.335		
		(0.724)*		
Age^2		-0.003		
		(0.001)***		
Mother's High Grade		-0.102	0.291	1.348
		(0.714)	(0.785)	(0.336)***
Grade			4.505	6.145
			(1.729)**	(1.513)***
Grade^2			-0.357	-0.410
			(0.096)***	(0.119)***
Constant	39.710	-52.868	41.269	9.084
	(0.518)***	(75.308)	(16.454)**	(9.145)
Observations	290	290	290	290
Number of ID CODE OF CHILD	145	145	145	
R-squared	0.76	0.80	0.80	0.57

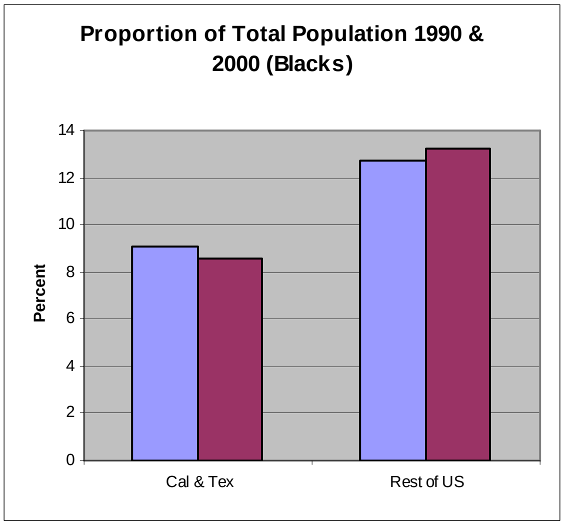
Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

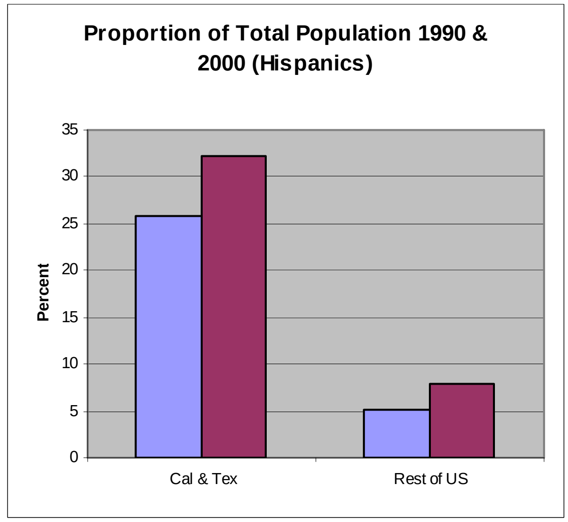
Table 28: US Black Panel

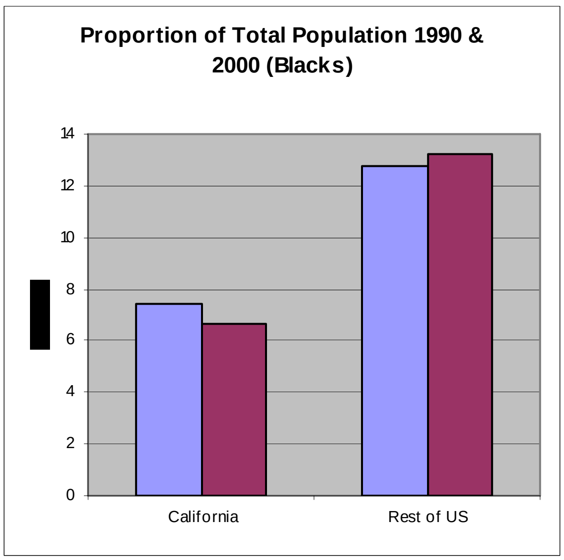
	(1)	(2)	(OLS)
Policy*CalTex	-5.723	-5.703	-6.368
	(2.283)**	(2.271)**	(1.650)***
Times Taken	2.237	3.513	5.559
	(3.450)	(3.513)	(1.582)***
Age	1.567		
	(0.705)**		
Age^2	-0.004		
	(0.001)***		
Mother's High Grade	0.783	0.893	1.622
	(2.884)	(2.217)	(0.338)***
Year2000	-9.432	3.669	-0.542
	(29.942)	(9.151)	(3.117)
Grade		6.266	6.575
		(1.658)***	(1.007)***
Grade^2		-0.510	-0.537
		(0.099)***	(0.087)***
Constant	-98.987	1.969	-17.977
	(79.248)	(30.996)	(6.442)***
Observations	360	360	360
Number of ID CODE OF CHILD	180	180	
R-squared	0.77	0.78	0.51

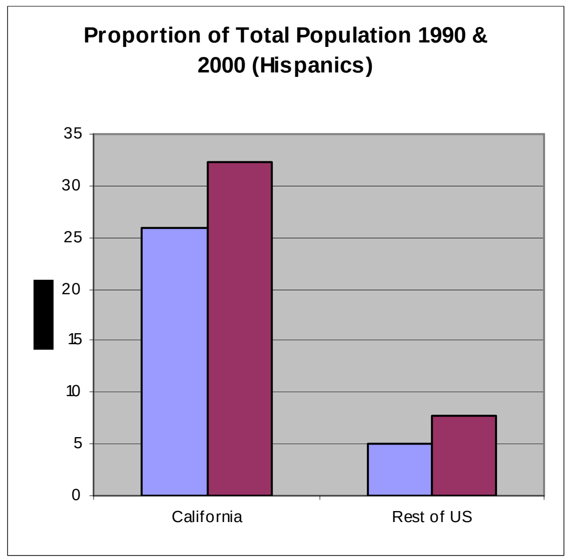
Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%

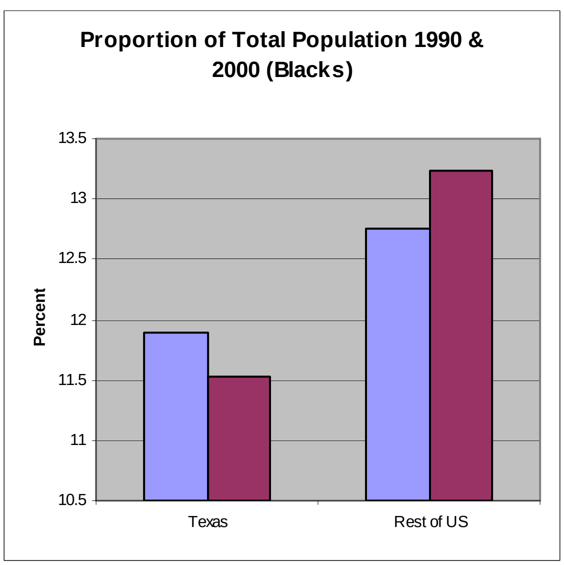
Figures 11 - 22

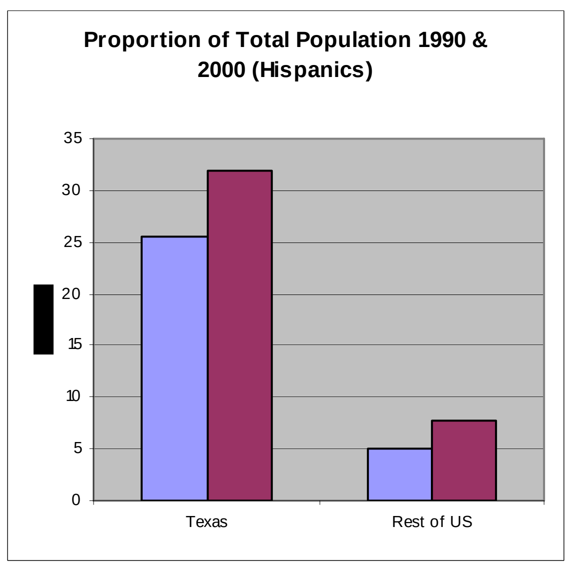


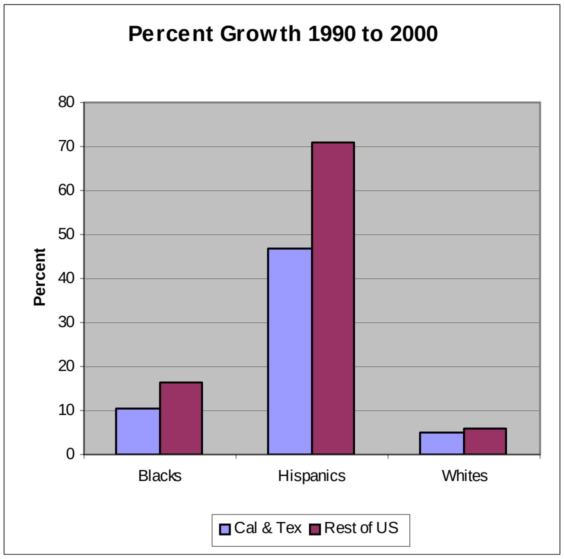


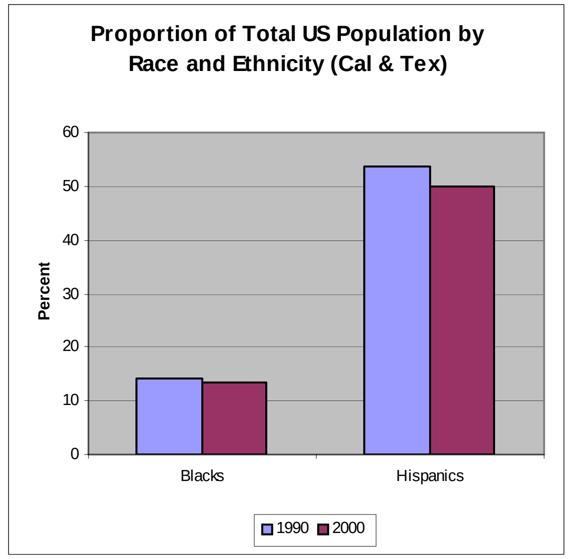


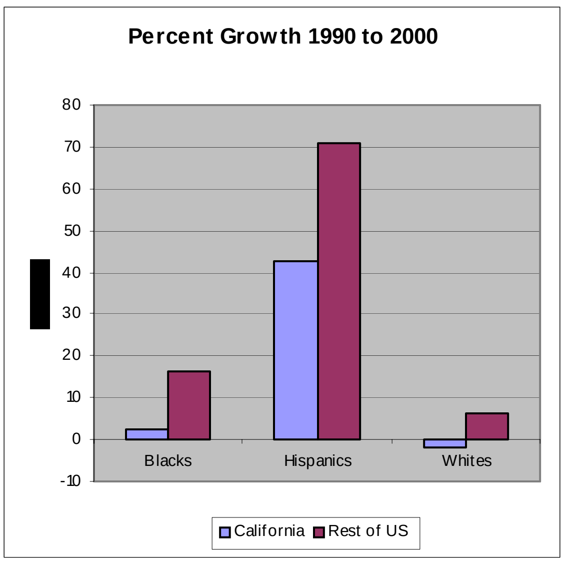


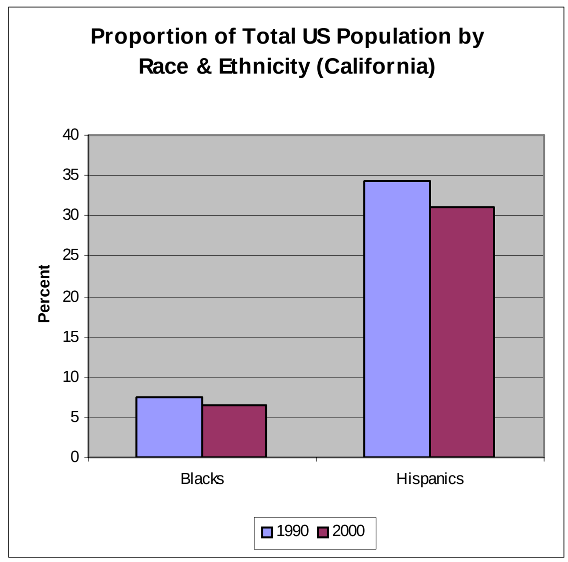


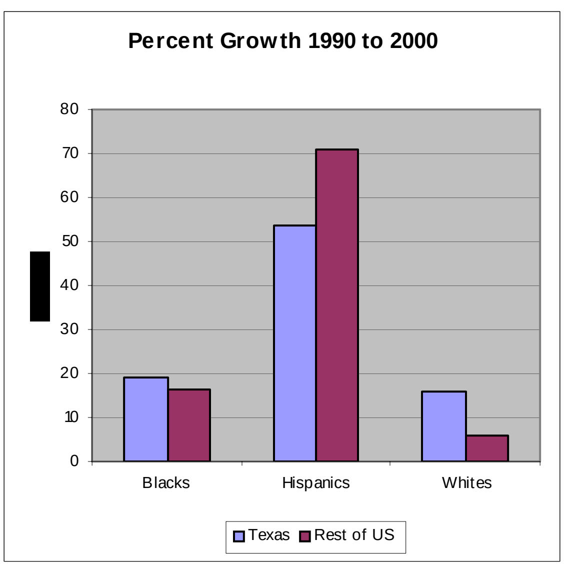


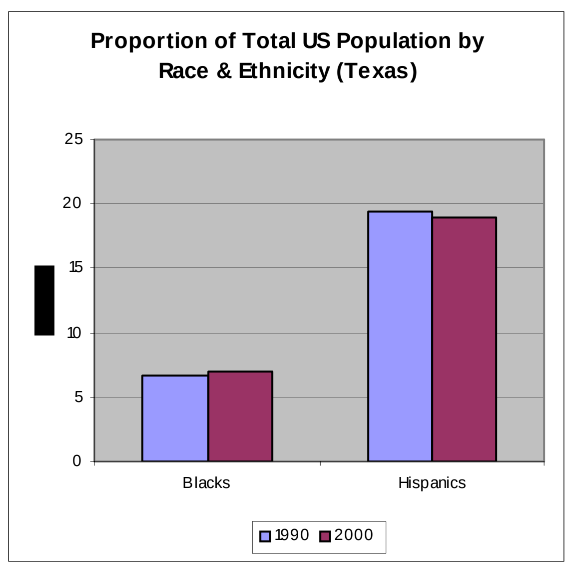

Percent Growth 1990 to 2000
Percent
80
70
60
50
40
30
20
10
0
Blacks
Hispanics
Whites
Cal & Tex
Rest of US


Proportion of Total US Population by Race and Ethnicity (Cal & Tex)
Percent
60
50
40
30
20
10
0
Blacks
Hispanics
1990
2000


Percent Growth 1990 to 2000
[redacted]
80
70
60
50
40
30
20
10
0
-10
Blacks
Hispanics
Whites
California
Rest of US


Proportion of Total US Population by Race & Ethnicity (California)
Percent
40
35
30
25
20
15
10
5
0
Blacks
Hispanics
1990
2000


Percent Growth 1990 to 2000
[redacted]
80
70
60
50
40
30
20
10
0
Blacks
Hispanics
Whites
Texas
Rest of US


Proportion of Total US Population by Race & Ethnicity (Texas)
[redacted]
25
20
15
10
5
0
Blacks
Hispanics
1990
2000