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Abstract
An important claim made for affirmative action programs has been that they need not be permanent: they can be discontinued, the argument runs, once they have transformed employers' attitudes. Proposition 209, enacted in California in 1996 and made effective the following year, represents a natural experiment testing that claim. It ended long-standing state affirmative action programs not only in education, but also in public employment and government contracting. The author uses Current Population Survey data to gauge the labor market effects of this policy change. The key finding is that employment among women and minorities dropped sharply, almost wholly because of a decline in labor force participation rather than an increase in unemployment. This finding suggests that affirmative action programs in California either had been inefficient--that is, resulted in sub-optimal employment outcomes--or had been effective while in place but had failed to create lasting change in employers' prejudicial attitudes.

Keywords
affirmative action, California's Proposition 209, discrimination

A CURE FOR DISCRIMINATION? AFFIRMATIVE ACTION AND THE CASE OF CALIFORNIA'S PROPOSITION 209

CAITLIN KNOWLES MYERS*

An important claim made for affirmative action programs has been that they need not be permanent: they can be discontinued, the argument runs, once they have transformed employers' attitudes. Proposition 209, enacted in California in 1996 and made effective the following year, represents a natural experiment testing that claim. It ended long-standing state affirmative action programs not only in education, but also in public employment and government contracting. The author uses Current Population Survey data to gauge the labor market effects of this policy change. The key finding is that employment among women and minorities dropped sharply, almost wholly because of a decline in labor force participation rather than an increase in unemployment. This finding suggests that affirmative action programs in California either had been inefficient—that is, resulted in sub-optimal employment outcomes—or had been effective while in place but had failed to create lasting change in employers' prejudicial attitudes.

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

To date, there has been little opportunity to measure the impact of removing affirmative action programs. While federal support for enforcement has ebbed and flowed and Supreme Court rulings in the past decade have chipped away at affirmative action, it is difficult to say whether concurrent changes in minority outcomes were due to affirmative action policy or to other trends in inequality. A similar problem plagued attempts to measure the impact of instituting affirmative action in earlier years. While minorities and women made gains in the labor market in the 1970s and 1980s, it is not clear what portion of this was due to affirmative action and what

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Copies of the computer programs used to generate the results presented in the paper are available from the author at Department of Economics, Middlebury College, Middlebury, VT 05753.

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portion was due to other influences. Empirical studies of the impact of affirmative action on labor markets have relied on differences in outcomes between government contractors, who are subject to the program, and non-contractors, who are not. While these studies have provided evidence of minority gains among contracting firms, the results could be biased because contractor status is not exogenous: firms with the lowest cost of meeting affirmative action requirements may be more likely to be contractors. Hence, we are left with an incomplete picture of both the impact of a controversial program and the potential consequences of its removal. What is needed is a control group to which we can compare changes in outcomes for those affected by affirmative action.

The enactment of California Proposition 209 provides just such an opportunity. The measure, passed in the 1996 state elections and made effective in November 1997, essentially outlawed existing local and state affirmative action programs in education, public hiring, and contracting, except where federal law required such programs. This change in state policy presents a natural experiment for measuring the labor market impact of removing affirmative action programs. I use Current Population Survey (CPS) data to compare outcomes for minorities in California before and after affirmative action was removed to the corresponding outcomes for white men. Then, to control for national trends in minority differentials, I compare this difference to the difference for a control group: states not undergoing similar changes in the law.

The use of this triple difference technique to analyze the impact of removing affirmative action on employment, unemployment, labor force participation, and wages will provide evidence on the long-term effects of affirmative action.

The History and Consequences of Affirmative Action Policy

National Legislation and Effects

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

Early studies tended to indicate that affirmative action had a positive impact on the employment and occupational advancement of racial minorities.[1] Because of the inherent difficulty in separating gains attributable to affirmative action from general trends in racial inequality, these studies relied on data from the Equal Employment Opportunity Commission (EEOC) to compare outcomes between firms that are federal contractors, and hence subject to federal affirmative action programs, and firms that are not. Ashenfelter and Heckman (1976) found that the demand for black men increased 3.3% more among contractors than among non-contractors between 1966 and 1970. Heckman and Wolpin (1976) and Goldstein and Smith (1976) found similar employment gains for black men during the early 1970s, but for women at contractor establishments they found either no improvement or even declines in employment. However, as Leonard (1989) pointed out, affirmative action for women did not become stringently enforced until after the Equal Employment Opportunity Act of 1972.

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

Studies of affirmative action in the late 1970s and beyond tended again to find employment gains for racial minorities and additional, although smaller, gains for white women. Leonard (1984c) found that between 1974 and 1980, demand for black men, other minority men, and white women grew, respectively, 3.8% faster, 7.9% faster, and 2.8% faster among contractors than among non-contractors. Leonard (1984b) also found that affirmative action appeared to affect minorities more in skilled than in unskilled occupations, although Smith and Welch (1984) suggested that observed gains in occupational status may have been due to contractors re-classifying jobs rather than to any real upward mobility. Rodgers and Spriggs (1996) found that the positive impact on employment continued through 1992 for all groups except Hispanics, for whom they found a negative impact.

One of the few empirical studies of wide scope that did not depend on EEOC data was Holzer and Neumark (2000b). Using information from a survey of employers in four U.S. cities, Holzer and Neumark found that firms using affirmative action did tend to recruit and hire more minorities and women. Moreover, contrary to the results of most earlier studies, they found evidence that the use of affirmative action in hiring had the largest effect for white women. The last employee hired was 8% more likely to be a white woman and 3% more likely to be a black man in firms that reported using affirmative action than in other firms.

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

California Legislation and Effects

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

However, attacks on these state programs in the mid-1990s resulted in their formal dismantling. In 1995, then-governor Pete Wilson signed Executive Order 124-95, which directed state agencies to eliminate preferential treatments that exceed federal or state statutory requirements. Legally this could only apply to pre-standing executive orders, and thus should not have affected

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

state affirmative action laws, but it is not clear, in practice, what effect it might have had (Thomas and Garrett 1999). A year later, California voters passed Proposition 209, outlawing all state affirmative action programs and hence releasing public employers as well as state contractors from affirmative action requirements. After lengthy court challenges, the new law went into effect in November 1997.

While there has been a flurry of research on the effects of Proposition 209 on higher education in California, economists have neglected to pay attention to the legislation's effects on labor markets. Yet, given that in 1995 nearly 16% of California's minority work force was employed in the non-federal public sector[3] and nearly 15% of California small businesses claim California state and local governments as clients (Williams 1999), we might expect Proposition 209 to have affected more than educational institutions. On the other hand, Holzer and Neumark (1999) suggested that approximately 60% of firms are federal contractors and subject to federal affirmative action policy. Hence, while Proposition 209 is likely to have had an effect on public employers in California, it may have been considerably less binding on private firms that are still subject to federal law.

The Proposition 209 Experiment

The enactment of Proposition 209 provides an opportunity to address two shortcomings of the empirical evidence to date.

First, previous work has had to rely on the comparison of firms that participate in affirmative action to those that do not. Researchers have either used EEOC data to compare federal contractors to non-contractors or firm-level data to compare firms that report using affirmative action to those that do not. Yet, because firms self-select into using affirmative action (by choosing to be federal contractors or by voluntarily implementing their own programs), such approaches to evaluating the impact of affirmative action may produce downward-biased estimates. Federal contractors and voluntary participants may self-select precisely because they can implement affirmative action more cheaply than can most other potential adopters. Moreover, the results of these studies have only provided an indication of the firm- or sector-level impact of affirmative action, not of its economy-wide effects. For instance, it is known that minority employment was rising at both contracting and non-contracting firms that file EEO-1 reports (albeit more rapidly at the contracting firms), but what was happening at firms that do not have to provide data on their composition? Did this rise in employment mask a re-shuffling of minorities between sectors?

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

Depending on the assumptions made about the source of pre-existing inequality, affirmative action may or may not engender a long-term change in labor market differentials that would remain even if the program were removed. If labor market discrimination did not exist in the first place or if, as some models suggest (for example, Johnson and Welch 1976), affirmative action is not an efficient policy, then removing affirmative action may cause the labor market to revert to its competitive equilibrium. On the other hand, certain models of discrimination do suggest a long-term impact for affirmative action. If, for example, labor market inequalities are the result of classic employer discrimination, then it is possible that employers' forced interaction with minority groups will sufficiently erode their prejudices to put a durable end to inequal-

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

ity. Alternatively, Coate and Loury (1993) considered a form of statistical discrimination in which employers are less likely to place minority workers in high-skilled jobs because of negative stereotypes. As a result, minorities have less incentive to invest in human capital, leading to a self-fulfilling prophecy. Assuming that minority workers are fundamentally as capable as non-minorities, affirmative action could break this cycle and permanently eliminate negative stereotypes. A third theoretical alternative for predicting the continued effectiveness of affirmative action after its removal is that presented by Athey et al. (2000). In their model, entry-level employees receive more mentoring from senior employees with similar characteristics. As a result, there exists bias favoring one type of employee in promotion that can be permanently broken by a temporary affirmative action program that introduces diversity.

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

Data

I employ data from the outgoing rotation groups in the monthly Current Population Survey (CPS) from 1994–2001, placing emphasis on 1995, the year before the proposal of Proposition 209, and 1999, two years after the new law had gone into effect. Observations are dropped for individuals who were employed but reported no hours or pay, reported an unknown sector of employment, or were self-employed.[5] Observations from Washington State were also dropped because that state passed legislation similar to Proposition 209 in 1998. The triple difference estimates in this analysis will rely on three divisions of the data. First, the observations are categorized as before or after the enactment of proposition 209 (1995 or 1999, 1995 or 2000, and so on, depending on the years being used). Second, individuals are divided into eight mutually exclusive and collectively exhaustive categories: white men, white women, black men, black women, Hispanic men, Hispanic women, other men, and other women. And third, the country is divided into two groups: an experimental state (California) and the remaining control states or "nation."

Table 1 reports sample sizes for each cell. Because of its population, the sample sizes within California for even this detailed breakdown of minority groups are still fairly large. However, California is not necessarily representative of the country as a whole. Although its distribution of employment across sectors and industry is similar to that in the rest of the country, it is more minority-heavy and has slightly lower rates of employment. The fact that California is more diverse than the country as a whole means that extrapolations from its experi-

[4]Other states and political entities followed suit after Proposition 209 became the subject of a referendum. Washington State passed its own repeal of affirmative action in 1998; similar proposals have failed elsewhere.

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

Table 1. Sample Sizes.

	1995		1999	
Group	*Nation*	*California*	*Nation*	*California*
White Male	94,335	5,113	84,120	5,191
White Female	112,672	6,045	98,649	5,927
Black Male	11,582	595	10,238	597
Black Female	16,689	773	14,409	812
Hispanic Male	7,301	2,805	8,315	2,992
Hispanic Female	8,533	3,144	8,991	3,168
Other Male	4,857	1,310	4,431	1,246
Other Female	5,758	1,500	5,198	1,486
Total	261,727	21,285	234,351	21,419

ence with affirmative action to general predictions should be made cautiously.

Average Changes in Labor Force Status and Wages

Turning to the effects of affirmative action, Table 2 explores the change in non-participation in the labor force for white women after Proposition 209 was enacted. In 1995, 46.0% of white women in California over age 16 were not in the labor force, while 32.1% of white men were not participating. By 1999, after Proposition 209 had gone into effect, the percentage of white women who were not in the labor force had fallen to 44.5%, but the participation of men showed a similar change. Overall, there was no statistically significant change in the participation of white women relative to that of white men in California. As a control, I look at the same outcomes for the rest of the nation. Over the same period nationwide, the non-participation of white women had fallen by 1.3 percentage points relative to white men. Differencing these effects, relative to the rest of the country, non-participation among white women in California rose by 1.6 more percentage points than that for white men. However, this estimate is not statistically significant.

Proposition 209 may have affected racial minority groups as well as women. Table 3 presents triple difference average changes for individual minority groups as well as for all minority groups together. The outcomes examined are labor force category (employed, unemployed, or not in the labor force) and hourly wage. For each group, the triple difference is calculated as in the preceding example. Note in particular that because employment, unemployment, and non-participation are mutually exclusive and collectively exhaustive, the relative changes for each group across these categories sum to 0. The point estimates indicate that the employment of all California minority groups fell relative to that of white men, controlling for national changes in these averages. There was little change in unemployment, leaving a rise in non-participation to account for this shift. However, many of the point estimates are not statistically significant.

While there is also no statistically significant change in wages, the point estimates are positive for six of the seven groups, suggesting, for example, that the relative wage of white women rose by 76 cents. However, there is no clear prediction about the wage changes that might accompany a policy that can directly affect both wages and employment. It may be that the employees who are left are more skilled, causing average wages to rise. Alternatively, it might be the case that affirmative action also served to augment wage equality and that its removal precipitated a drop in wages. Given the possibility of opposing effects, it is not surprising to find no statistically significant impact on wages.

Individual-Specific Differences

Although it is primarily viewed as a cross-sectional data set, the CPS can also be used as a panel in which each individual in the outgoing rotation group is observed twice. To construct a panel, I match the respon-

Table 2. Change in Non-Participation, 1995–1999: White Men and Women.

Group	*1995*		*1999*	*Time Difference for Group*
California				
White Women	0.460 (0.006)		0.445 (0.006)	–0.015* (0.009)
White Men	0.321 (0.007)		0.302 (0.006)	–0.018** (0.009)
Group Difference for a Given Year	0.140*** (0.009)		0.143*** (0.009)	
Double Difference		0.003 (.013)		
Nation				
White Women	0.432 (0.001)		0.421 (0.002)	–0.012*** (0.002)
White Men	0.284 (0.001)		0.285 (0.002)	0.001 (0.002)
Group Difference for a Given Year	0.148*** (0.002)		0.135*** (0.002)	
Double Difference		–0.013*** (0.003)		
Triple Difference			0.016 (0.013)	

Notes: Standard errors are in parentheses below estimates. Rounding is done after calculations.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

dents at a particular address across years and then assume that the respondent is the same person if sex and race did not change and if age increased by 0 to 2 years. This allows approximately two-thirds of the individuals in the outgoing rotation group in any given year to be matched to the previous year. However, because of a change in CPS methodology, matching is not possible for June–December of 1994 and 1995 and January–August of 1995 and 1996.

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

Table 4 reports the triple difference estimates of the relative probability that members of each minority group left the labor force given a change in participation. The first difference is the proportion of each group that left the labor force conditional on a change in participation. The second differ-

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

Table 3. Triple Differences Summary, 1995–1999.

Group	*Employed*	*Unemployed*	*Not in Labor Force*	*Hourly Wage*
White Men	—	—	—	—
White Women	–0.021	0.005	0.016	0.761
Black Men	–0.054*	0.008	0.047	–0.240
Black Women	–0.034	–0.019	0.053*	0.256
Hispanic Men	–0.002	–0.010	0.012	0.185
Hispanic Women	–0.029*	–0.004	0.033*	0.504
Other Men	–0.060**	0.003	0.057**	0.028
Other Women	–0.016	0.002	0.014	0.765
All Minorities	–0.017	–0.003	0.020	0.012

Note: All monetary values are in 1995 dollars.
*Statistically significant at the .10 level; **at the .05 level.

ence gives the proportion of each minority group that left the labor force relative to the proportion of white men who left the labor force. The third difference compares this change in California to the change in the rest of the nation. The estimates indicate that statistically significant changes took place between 1994 and 1995 and between 1995 and 1996.[7] Between 1994 and 1995 in California, white women were 16.7% more likely to have left the labor force given a change in participation than were white men relative to the nation as a whole. Black men, black women, and Hispanic women were also more likely to leave the labor force than to enter it. Between 1995 and 1996, black women and Hispanic women were again more likely to leave the labor force than to enter it, although the reverse was true for "other" women. As a whole, the estimates suggest a climb in the proportion of minorities who were leaving the labor force relative to entering it in the mid-1990s.

Note that the years for which these changes are observed are directly preceding or during the period when Proposition 209 was debated and passed. The observed decline in relative participation could be the result of an earlier policy change that was unrelated to affirmative action. However, it could also indicate an anticipation of the change in affirmative action policy, which seems plausible given the political environment in California at the time. While these results are indicative of change, they cannot be compared directly to estimates reported in succeeding sections, which are based on cross-sectional cuts of the data. Later results will provide similar evidence of an increase in non-participation, but the changes they show occur in years after 1994–95. However, the outcome variable in the regressions will not be conditioned on a change in participation. Moreover, using the CPS as a pseudo-panel restricts attention to changes that took place within a single year. Given that the labor market effects of the removal of affirmative action may have taken more than a year to emerge, the panel-data analysis would not capture the trend.

Econometric Model

To control further for the characteristics of the potential or actual labor force in estimating the effects of Proposition 209, I employ a triple difference regression framework. For simplicity, consider the case of only one minority treatment group. In the case of general outcome y, consider the equation

$$(1)\quad \begin{aligned} y_{ijt} = {} & x_{ijt}\gamma + \beta_1 \mathrm{Year}_t + \beta_2 \mathrm{Exper}_j \\ & + \beta_3 \mathrm{Treat}_i + \beta_4(\mathrm{Year}_t{*}\mathrm{Exper}_j) \\ & + \beta_5(\mathrm{Year}_t{*}\mathrm{Treat}_i) + \beta_6(\mathrm{Exper}_j{*}\mathrm{Treat}_i) \\ & + \beta_7(\mathrm{Year}_t{*}\mathrm{Exper}_j{*}\mathrm{Treat}_i) + \varepsilon_{ijt}, \end{aligned}$$

where i indexes an individual, j indexes location, and t indexes time. The vector x_{ijt} contains a constant and controls for age, marital status, education attainment, month of interview, central city location, nativity,

[7]These are the same years for which limited matching was possible due to a change in CPS methodology.

Table 4. Triple Differences for Proportion of Each Group That Left the Labor Force Conditional on a Change in Participation.

Group	*93–94*	*94–95*	*95–96*	*96–97*	*97–98*	*98–99*
White Men	—	—	—	—	—	—
White Women	0.015	0.167*	0.155	0.017	–0.007	0.038
Black Men	–0.039	0.477***	0.251	0.010	0.019	–0.013
Black Women	–0.156*	0.289**	0.318**	–0.061	0.052	0.075
Hispanic Men	–0.017	0.177	–0.072	–0.018	–0.041	–0.033
Hispanic Women	–0.036	0.186*	0.235**	–0.009	–0.010	–0.040
Other Men	0.015	0.068	0.100	0.046	–0.127	0.006
Other Women	–0.054	0.155	–0.276**	0.048	–0.022	–0.032
All Minorities	–0.038	0.157*	0.116	–0.008	–0.034	–0.022

*Statistically significant at the .10 level; **at the .05 level.

and citizenship. The remaining variables are used in the triple difference estimates. In this case, YEAR is a dummy for the later year in the regression (for example, 1999 if we are comparing 1999 to 1995), EXPER is a dummy indicating that the individual resides in California, the experimental state, and TREAT indicates that the individual is a member of the minority treatment group.

A quick examination of the various differences of interest illustrates that, as is standard, the coefficient β_7 represents the triple difference estimate of the impact of Proposition 209 on outcome y for the treatment group. The double difference estimate of the change in outcome for the treatment group in California relative to white men (WHITEMEN) can be calculated as follows:

$$(2)\qquad \Delta_{\text{TREAT,EXPER}} = y_{\text{TREAT,EXPER,99}} - y_{\text{TREAT,EXPER,95}} = \beta_1 + \beta_4 + \beta_5 + \beta_7$$

$$(3)\qquad \Delta_{\text{WHITEMEN,EXPER}} = y_{\text{WHITEMEN,EXPER,99}} - y_{\text{WHITEMEN,EXPER,95}} = \beta_1 + \beta_4$$

$$(4)\qquad \Delta^2_{\text{EXPER}} = \Delta_{\text{TREAT,EXPER}} - \Delta_{\text{WHITEMEN,EXPER}} = \beta_5 + \beta_7.$$

Similarly, the double difference for the control states is

$$(5)\qquad \Delta^2_{\text{NATION}} = \Delta_{\text{TREAT,NATION}} - \Delta_{\text{WHITEMEN,NATION}} = \beta_5.$$

Hence, the triple difference estimate of the impact of Proposition 209 is

$$(6)\qquad \Delta^3 = \Delta^2_{\text{EXPER}} - \Delta^2_{\text{NATION}} = \beta_7.$$

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

The remaining three outcomes of interest—employment, unemployment, and non-participation—are binary variables and are commonly estimated with probit or logit models. As Ai and Norton (2003) pointed out, the marginal effect of the interacted variables in a nonlinear model is not the same thing as the marginal effect of the interaction term.[8] In other words, simply calculating the marginal effect of the triple difference term ignores the fact that one cannot "turn off" β_7 without affecting the other related interaction variables.

Some authors (for example, Borjas 2003) have chosen to use a linear probability model to avoid the complications that arise from a nonlinear model with interaction terms. In this paper, I use a probit model and estimate

[8]The authors note that most studies with interaction terms in a nonlinear model have reported the marginal effect of the interaction term even though there is little intuitive explanation for what this means. In the differences-in-differences literature, Gruber (1994), for example, reported the marginal effect of the difference-in-difference coefficient.

the triple difference marginal effects through repeated differencing of the normal CDF. As is discussed in the results section, the estimates produced by the two methods are quite similar. Using a probit model, I find that the double differences for the sample of individuals in the treatment group are

$$(7) \qquad \Delta^2_{i,\mathrm{EXPER}} = [\Phi(\boldsymbol{x_i}\gamma + \beta_1 + \beta_2 + \beta_3 + \beta_4 + \beta_5 + \beta_6 + \beta_7) - \Phi(\boldsymbol{x_i}\gamma + \beta_2 + \beta_3 + \beta_6)] - [\Phi(\boldsymbol{x_i}\gamma + \beta_1 + \beta_2 + \beta_4) - \Phi(\boldsymbol{x_i}\gamma + \beta_2)]$$

and

$$(8) \qquad \Delta^2_{i,\mathrm{NATION}} = [\Phi(\boldsymbol{x_i}\gamma + \beta_1 + \beta_2 + \beta_5) - \Phi(\boldsymbol{x_i}\gamma + \beta_3)] - [\Phi(\boldsymbol{x_i}\gamma + \beta_1) - \Phi(\boldsymbol{x_i}\gamma)],$$

and the triple difference is

$$(9) \qquad \Delta^3_i = \Delta^2_{i,\mathrm{EXPER}} - \Delta^2_{i,\mathrm{NATION}}.$$

I average the triple difference marginal effects across individuals in the treatment group to get the average marginal effect.

The delta method is used to compute standard errors. Again, in previous work, authors have generally reported only the standard error for β_7, the triple difference probit coefficient. But simply because the estimated probit coefficient on the triple difference term is statistically significant does not mean that the marginal effect is. Let $h(\beta, z_i)$ be the nonlinear function composed of the eight-fold differences of normal CDFs as expressed in equation (9) and let V_β be the variance-covariance matrix of the coefficients. Then the variance for the average triple difference marginal effect is

$$(10) \qquad V = E(\nabla_\beta h(\beta,x_i))\, V_\beta E(\nabla_\beta h(\beta,x_i)').$$

This can be estimated by

$$(11) \quad \hat{V} = \Big(\frac{1}{n}\sum_i \nabla_\beta h(\hat{\beta},x_i)\Big)\, \hat{V}_\beta \Big(\frac{1}{n}\sum_i \nabla_\beta h(\hat{\beta},x_i)\Big)'.$$

Note that this method calculates the standard errors for the average triple difference marginal effect rather than the other commonly used option in evaluating marginal effects: the marginal effect for the average member of a group.[9]

[9]Ai and Norton (2003) provided an estimator for the variance of the marginal effect for the average individual when using interaction terms, but not for the average marginal effect. Thanks to Stephen Donald for his help with obtaining the correct estimator for the case used here.

Empirical Analysis of Proposition 209

Labor Force Status

Table 5 reports the triple difference estimates of the average marginal effects of Proposition 209 on employment, unemployment, and non-participation.[10] In reality, the changes in employment, unemployment, and non-participation must sum to zero. Although that added restriction is not placed on the marginal effects reported here, the sum of the unrestricted effects is in fact fairly close to zero, for the most part. In an attempt to identify possible short-term and longer-term effects of the legislation, I examine three pairs of years: 1995 and 1999, 1995 and 2000, and 1995 and 2001.[11] Moreover, the results are presented for each of the seven treatment groups as well as for all of the minorities together.

As shown in Table 5, between 1995 and 1999 the relative employment of minorities fell by 2.8 percentage points while non-participation rose by 2.9 percentage points. Similarly, between 1995 and 2000 relative employment fell by 1.8 percentage points and non-participation rose by 2.2 percentage points, and between 1995 and 2001 relative employment fell by 2.2 percentage points while non-participation rose by 2.0 percentage points. Breaking this down by group, between 1995 and 1999, the percentage point rise in relative non-participation was 2.9 for

[10]In addition to race and sex, other variables controlled for in all regressions were age, marital status, interview month, education, region, urban status, citizenship, and nativity. Wage regressions also included indicators for employment sector (public, private, or federal), occupation, and industry.

[11]In all cases 1995 is used as the base year to which post-legislation years are compared. The results are similar if 1993, 1994, or 1996 is used as the base instead. In addition, 1998 and 2002–2003 were also examined as post-legislation years. The triple difference coefficients are smaller in 1998, similar in 2002, and become statistically insignificant in 2003. How to interpret these results is not clear, however, since extending the time frame also increases the chance of unobserved events biasing the results.

Table 5. Triple Difference Marginal Effects for Employment, Unemployment, and Non-Participation Probits.

	1995–1999		*1995–2000*		*1995–2001*	
Group	*Coeff.*	*S.E.*	*Coeff.*	*S.E.*	*Coeff.*	*S.E.*
Employment						
White Women	–0.034***	0.011	–0.018	0.011	–0.026**	0.011
Black Men	–0.035	0.027	–0.012	0.027	0.027	0.027
Black Women	–0.029	0.024	–0.054**	0.024	–0.051**	0.024
Hispanic Men	–0.008	0.016	–0.030*	0.016	–0.034**	0.016
Hispanic Women	–0.048***	0.017	–0.045***	0.017	–0.034**	0.017
Other Men	–0.071***	0.020	–0.047**	0.020	–0.068***	0.001
Other Women	–0.020	0.020	–0.022	0.020	–0.028	0.020
All Minorities	–0.028***	0.010	–0.018*	0.010	–0.022**	0.010
Unemployment						
White Women	0.004	0.004	–0.001	0.004	0.003	0.004
Black Men	0.009	0.014	0.011	0.014	0.002	0.014
Black Women	–0.016	0.011	–0.022**	0.010	0.002	0.012
Hispanic Men	–0.003	0.008	0.004	0.008	0.006	0.008
Hispanic Women	–0.003	0.007	0.005	0.007	< 0.001	0.008
Other Men	0.005	0.009	–0.009	0.008	0.010	0.009
Other Women	< 0.001	0.007	< 0.001	0.006	0.008	0.007
All Minorities	< 0.001	0.004	–0.003	0.004	0.003	0.005
Non-Participation						
White Women	0.029***	0.010	0.018	0.010	0.022**	0.011
Black Men	0.029	0.026	0.002	0.026	–0.024	0.026
Black Women	0.046**	0.023	0.079***	0.024	0.050**	0.024
Hispanic Men	0.014	0.015	0.029*	0.015	0.032**	0.015
Hispanic Women	0.052***	0.017	0.041**	0.016	0.036**	0.016
Other Men	0.068***	0.020	0.059***	0.019	0.059***	0.019
Other Women	0.020	0.020	0.021	0.019	0.019	0.019
All Minorities	0.029***	0.009	0.022**	0.009	0.020**	0.010

Notes: Marginal effects are averages across the relevant treatment group. Marginal effects and standard errors are calculated as outlined in the text.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

white women, 4.6 for black women, 5.2 for Hispanic women, and 6.8 for other men. This increase in non-participation accounts for nearly all of the decline in employment for all groups except black women, who also saw a drop in unemployment. Black and Hispanic men and "other" women do not exhibit statistically significant changes in labor force status between 1995 and 1999. The general trend continues through 2001, at which point there appears to have been a rise in non-participation for all minority groups save for "other" women, for whom the point estimates are similar to other groups but not significant, and black men, for whom non-participation fell. That there is little evidence of a negative impact on black men is in keeping with previous findings (for example, Holzer and Neumark 2000b) that in later years affirmative action had a greater impact on women, but it should also be noted that black men comprise the smallest of the six minority groups in the sample.

Interestingly, these estimates are nearly identical to those obtained using a linear probability model with robust standard errors, which avoids the complications inherent in estimating the marginal effects and their standard errors and makes it simple to restrict them to sum to zero. The linear probability estimates, however, yielded predicted probabilities that were outside the range of [0,1] for 5% to 9% of the observations, depending on the dependent variable. Turning to another common method of estimating marginal effects in nonlinear differencing

Table 6. Triple Difference Coefficients for Log(Wage) Regressions.

Group	*1995–1999* Coeff.	S.E.	*1995–2000* Coeff.	S.E.	*1995–2001* Coeff.	S.E.
White Women	0.001	0.018	–0.014	0.019	–0.003	0.020
Black Men	–0.039	0.039	–0.092**	0.040	–0.005	0.040
Black Women	–0.029	0.040	–0.029	0.037	–0.100**	0.041
Hispanic Men	0.007	0.021	0.005	0.021	–0.019	0.021
Hispanic Women	0.052**	0.024	0.044*	0.023	0.037	0.025
Other Men	–0.016	0.033	0.044	0.031	0.009	0.033
Other Women	0.041	0.030	0.087***	0.029	0.019	0.032
All Minorities	0.010	0.015	0.008	0.015	–0.002	0.016

Notes: Standard errors are robust. All monetary values are 1995 dollars.
*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

models, the estimates presented here are of much smaller magnitude than those obtained if the marginal effect of the interaction term is used instead, as many previous studies have done. For instance, the average marginal effect of the triple difference coefficient—that is, the difference between the normal CDF with β_7 turned "on" and "off"—suggests that between 1995 and 1999, employment for all minorities fell by 10 percentage points, unemployment rose by 2.4 percentage points, and non-participation rose by 11.2 percentage points. That these effects are so much larger than those obtained using the correct method is somewhat alarming given the frequency with which marginal effects are calculated in this way.

As a whole, the results suggest that the impact of Proposition 209 was to move women and minorities from employment to out of the labor force. If, as the results indicate, the removal of affirmative action made it more difficult for women and minorities to find work, then this exit from the labor force is not surprising. Previous work has tended to indicate that women have more elastic labor supplies than men and that they tend to be more responsive along the extensive participation margin (Blau and Kahn 2005; Meyer 2002; Kimmel and Kniesner 1997). In addition, when looking at the impact of minimum wage legislation, Mincer (1976) found that affected groups tend to leave the labor force and, moreover, that women and minorities have relatively high participation elasticities. Furthermore, these estimates look at the impact of Proposition 209 a year and more after its implementation. It may be that these groups did initially move from employment to unemployment but that by 1999 they were increasingly becoming discouraged and leaving the labor force.

Wages

As mentioned previously, there is no clear prediction of how the removal of affirmative action will affect wages. Relative wage changes will depend on the nature of pre-existing discrimination, the effectiveness of affirmative action, and the relative skill levels of the groups affected by its removal. It is thus not surprising that the patterns shown by the results in Table 6 are less clear than those shown by the labor force status results. No statistically significant changes are observed between 1995 and 1999, with the exception of the relative wages of employed Hispanic women, which rose by 5%. In 2000 we see a rise for "other" and Hispanic women, but the wages for black men have fallen by 9.2%. Between 1995 and 2001, only black women show a statistically significant change in relative wages. As a whole, the results do not show a consistent effect for any of the groups. This could indicate that affirmative action had little effect on wages. Affirmative action laws, after all, did not directly address wage equality, which was covered by equal employment law. It could also be the result of skill selection among those leaving employment. Since the wage regressions are conditional on employment, the wages of those who remain employed could rise because they are more

Table 7. Triple Difference Marginal Effects by Education and Age for Non-Participation Probit, 1995–1999.

	Education			*Age*		
Group	*< High School*	*High School*	*> High School*	*< 30*	*30–50*	*> 50*
White Women	0.032***	0.030***	0.027***	0.036***	0.026**	0.028***
Black Men	0.032	0.031	0.027	0.035	0.026	0.030
Black Women	0.049**	0.048*	0.045*	0.056**	0.041*	0.046**
Hispanic Men	0.016	0.014	0.012	0.016	0.014	0.010
Hispanic Women	0.057**	0.050***	0.045***	0.058***	0.052***	0.043***
Other Men	0.083***	0.072***	0.062***	0.080***	0.060***	0.067***
Other Women	0.025	0.021	0.017	0.023	0.016	0.022
All Minorities	0.035***	0.028***	0.025***	0.035***	0.028***	0.025***

Note: Marginal effects are averages across the relevant treatment group and are calculated as outlined in the text.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

skilled or fall because they are less skilled than those who left.

Participation Effects by Age and Education Level

The wage findings do not provide consistent evidence of skill bias among those who remained employed, but neither do they prove the contrary. Previous studies have suggested that affirmative action helps advance minorities into more skilled occupations (for example, Goldstein and Smith 1976; Leonard 1984b). However, it is not clear how the removal of affirmative action would affect workers across skill groups. It could be the case that low-skilled workers become discouraged with the diminishment of opportunities for advancement and leave the labor force; alternatively, the removal of affirmative action might directly affect those high-skilled workers who had previously advanced. To try to determine if a particular group was disproportionately affected by the legislation, I estimated the non-participation marginal effects for separate segments of the sample.

Table 7 reports these results. In columns (1)–(3), non-participation marginal effects are reported for three education levels: less than high school, a high school diploma, and education beyond high school.[12] On average, non-participation rose more for minorities with lower levels of education than for other groups. For those with less than a high school degree, non-participation rose by 3.5 percentage points; for those with a high school degree or education beyond high school, the corresponding rises were 2.8 and 2.5 percentage points, respectively. This trend is evident for the disaggregated treatment groups as well, although, as in the earlier results, the rise tends to be statistically significant only for white women, black women, Hispanic women, and "other" men. Turning to age, columns (4)–(6) report results for three age brackets: 30 years old or younger, 30 to 50 years old, and older than 50. On average, non-participation rose more for the youngest group of workers than for older groups. When disaggregated by treatment group, for all groups except Hispanics it seems that non-participation rose more for individuals who were under 30 years old or over 50. This is not surprising given the expectation that very young and very old workers will be less attached to the labor force than middle-aged workers. If education and age truly proxy for skill, then these findings suggest that the removal of affirmative action disproportionately affected low-skilled workers.

[12]These estimates are based on the NILF probit for the entire sample, but the marginal effects are calculated for the relevant level of education or age.

Sector of Employment

Previously I suggested that because Proposition 209 does not supersede federal affirmative action laws, workers in California's public

Table 8. Triple Difference Marginal Effects for Sector of Employment: Probits for all Minorities, 1995–1999.

Sector	*Coeff.*	*S.E.*	*P-Value*
P(Private|Emp)	–0.0011	0.0006	0.0805
P(Public|Emp)	0.0012	0.0008	0.1423
P(Federal|Emp)	–0.0001	< 0.0001	0.1206

Note: Marginal effects are averages across the relevant treatment group and are calculated as outlined in the text.

sector, who were covered by California policy but not by federal policy, might see the largest effects from the measure. However, it is difficult to use CPS data to compare inter-sector differences. The results for the economy as a whole suggest that Proposition 209 did not affect the unemployment rate but did decrease participation. But if an individual is not in the labor force, we cannot identify what sector he or she may have worked in previously. I attempt to circumvent this problem by using a probit model to estimate the impact of Proposition 209 on the probability that an individual works in the private, public (state or local), or federal sector given that he or she is employed. Because some of the cell sample sizes become very small when disaggregated by both treatment group and sector, the estimation is performed only for all minorities as a single treatment group.

Table 8 presents these results. The point estimates suggest that minorities were actually slightly more likely to work in the public sector and less likely to work in the private sector after the removal of affirmative action. This is counter to the expectation that the negative effects of Proposition 209 would be strongest for state and local workers in California.

It may be the case that private employers did respond to the removal of state-sponsored affirmative action in California and to the general anti-affirmative action climate of the period. While many private-sector firms in California were likely to be federal contractors, federal affirmative action policy had also been under intense legal scrutiny during the 1990s, and private employers in California may have felt more bound by state policy than federal policy. However, the coefficients here are not statistically significant and are of small magnitude, thus providing no strong evidence of a sizeable shift between sectors of employment.

Alternative Explanations

The estimates presented thus far suggest that between 1995 and 1999 relative employment for most minority groups in California fell as non-participation rates rose. While I have attributed this exodus from the labor force to the 1996–97 removal of affirmative action in state employment and contracting, other concurrent events could instead be responsible. I now consider six alternative explanations: changes in school enrollment patterns, immigration policy, child care policy, child support policy, incarceration rates, and welfare reform.

Examining the first three alternative explanations—changes in enrollment patterns, immigration law, and family leave policy during this period—suggests that, if anything, the magnitude of the estimated impact of affirmative action might be biased downward. After the passage of Proposition 209, which directly affected admissions at public universities, minority enrollment dropped sharply in California state schools (Card and Krueger 2005). This contraction in educational opportunities should have caused a rise in minority labor force participation rather than the observed fall. Furthermore, in 1994 California voters passed Proposition 187, which denied illegal immigrants access to social services, health care, and public education. Although the initiative was overturned by a federal court and never went into effect, its passage is perhaps indicative of a change in attitudes toward public provision of services to illegal immigrants. However, even if this is the case, it seems likely that a decrease in the provision of public services again would tend to increase minority labor force participation rather than decrease it. Finally, if child-care-related policy in California decreased the costs of parental leave during this period, non-participation may have risen for women. However, relative to the rest of the country, parental leave policy in California actually became less generous. The federal Family and Medical Leave Act (FMLA), enacted in 1994, established a twelve-week minimum leave duration that was less generous than the sixteen weeks already mandated by California state law. Thus, while women in other states may have found it easier to take leaves of absence for childbearing, there was little change in California. Hence, if anything, labor force participation in California should have risen relative to the rest of the country, again moving opposite to the direction of the observed effect.

Turning to the fourth consideration, changes in child support law or enforcement seem unlikely to have biased the results. In 1996, California did establish a child support commissioner and family law facilitator system to assist with the processing of child support cases in the courts. However, if this program helped to increase child support enforcement, it seems unlikely to have caused a decrease in the labor force participation of California minority men relative to white men. Furthermore, Hu (1999) provided evidence that increases in child support payments tend to have a small effect on female labor supply and that, moreover, this effect is a positive one because increases in child support payments tend to reduce welfare eligibility.

To the extent that minorities are over-represented among prison populations, changes in California incarceration rates during this time might also be confounded with the effects of affirmative action. However, Bureau of Justice Statistics show that the increase in incarceration rates in California between 1995 and 1999 was close to the average increase across the nation (Bureau of Justice Statistics 1996, 2000). Hence, California was not experiencing a larger-than-average rise in prison populations that might have affected relative participation rates.

The final policy consideration is welfare reform. The federal government enacted major welfare reform legislation in 1996 with the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA), which imposed stricter limits on the cutoffs for federally funded benefits while also giving states greater autonomy in program design. Many state governments, in response, adjusted their benefit reduction rates in an attempt to increase labor force participation and hours. While California had been adjusting its welfare laws in an attempt to increase labor force participation prior to 1996, Los Angeles in particular has been accused of being slow to implement further welfare reform in the wake of the federal legislation (Klerman et al. 2000). If it is indeed the case that welfare reform was enacted more slowly in California than elsewhere, this, rather than the removal of affirmative action, could have generated the fall in participation rates relative to the rest of the country.

To address this concern, I would first point out that the breakdown of the change in minority participation by education level presented in Table 7 showed that, while the fall in participation was larger for those with low levels of education, the overall decrease for groups with education beyond high school remained quite similar to the marginal effects for the population as a whole. That relative minority participation declined by 2.5 percentage points for even the relatively well-educated, whose participation rates were less likely to be affected by welfare reform, suggests that welfare reform is unlikely to explain the overall decline.

To pursue this issue further, I change the control group in the triple difference estimation from the rest of the nation to states that had little change in their benefit reduction rates during this period. Blank (2002) presented calculations of estimated welfare recipient who worked 30- and 40-hour workweeks in 1996 and in 2000 and had two children. Estimated cumulative cash welfare benefits for a two-year period in California for a recipient earning \$6 per hour increased from \$4,198 to \$8,724 after reform for a 30-hour workweek and from \$788 to \$5,700 for a 40-hour workweek. According to Blank's calculations, eight other states (Arizona, Arkansas, Idaho, Indiana, Maryland, Nebraska, Oklahoma, and West Virginia) offered no cash benefits to such a worker either before or after welfare reform. Hence, even if welfare reform was enacted slowly in California, the effect on participation should have been to increase it relative to these states that were not increasing the

Table 9. Triple Difference Marginal Effects for Employment, Unemployment, and Non-Participation Probits by Control Group, 1995–2000.

	Control Group			
	Rest of Nation		*Limited Welfare Reform States*	
Group	*Coeff.*	*S.E.*	*Coeff.*	*S.E.*
Employment				
White Women	–0.018*	0.011	–0.032***	0.012
Black Men	–0.012	0.027	0.001	0.033
Black Women	–0.054**	0.024	–0.022	0.029
Hispanic Men	–0.030	0.016	–0.044*	0.027
Hispanic Women	–0.045***	0.017	–0.024	0.029
Other Men	–0.047**	0.020	–0.062*	0.033
Other Women	–0.022	0.020	–0.037	0.033
All Minorities	–0.018**	0.010	–0.024**	0.012
Unemployment				
White Women	–0.001	0.004	0.008	0.005
Black Men	0.011	0.014	0.007	0.018
Black Women	–0.022**	0.010	–0.021	0.014
Hispanic Men	0.004	0.008	0.029*	0.016
Hispanic Women	0.005	0.007	0.017	0.013
Other Men	–0.009	0.008	0.010	0.017
Other Women	<0.001	0.006	0.020	0.013
All Minorities	–0.003	0.004	0.005	0.006
Non-Participation				
White Women	0.018*	0.010	0.023*	0.012
Black Men	0.002	0.026	–0.006	0.031
Black Women	0.079***	0.023	0.043	0.028
Hispanic Men	0.029*	0.015	0.009	0.024
Hispanic Women	0.041**	0.017	0.009	0.029
Other Men	0.059***	0.020	0.055*	0.031
Other Women	0.021	0.020	0.015	0.032
All Minorities	0.022**	0.009	0.019*	0.011

Notes: Marginal effects are averages across the relevant treatment group. Marginal effects and standard errors are calculated as outlined in the text.

*Statistically significant at the .10 level; **at the .05 level; ***at the .01 level.

participation incentive through a reduction in benefits.

The first column of Table 9 presents triple difference marginal effects for a model that is identical to that in Table 5. The second column of Table 9 presents these marginal effects for a model in which the control group is now reduced to states that did not show a change in estimated benefit levels. The employment estimates in the first column of the table might be negatively biased if the effects of the removal of affirmative action are confounded with a slow implementation of welfare reform. The second set of estimates, on the other hand, might be positively biased by the comparison to states that continued to deny benefits to low-wage women who worked 30 or 40 hours per week. While precision decreases with the limited sample size in the second set of estimates, the general trend for all minorities remains. Relative employment appears to have dropped in 2000, with a corresponding small increase in unemployment and a larger increase in non-participation. Looking at individual minority groups, as a wholethe coefficients show similar trends for most groups, with a few notable exceptions. The drop in employment for Hispanic men and women now seems to be accounted for by a rise in unemployment rather than non-participation. "Other" men and women show a larger rise in unemployment. Finally, black men exhibit little relative change in labor force status. While the results are not entirely consistent across subgroups of minorities, the general trend remains. Relative employment for minorities in California declined between 1995 and 2000, and differences in the implementation of welfare reform do not appear to account for all of the change.

Conclusion

The enactment of Proposition 209 in California created a unique opportunity to study the labor market effects of the removal of affirmative action programs. Changes in outcomes for minorities in California relative to those for white men are compared to the same differences for the rest of the nation in order to separate the effects of Proposition 209 from general trends in inequality. Because the possibility of confounding policy changes is of concern, attention has also been paid to alternative explanations for the observed effects, the most likely of which is state variation in the implementation of welfare reform during this period.

The results suggest that there was a sharp drop in employment after the passage of Proposition 209, which resulted in minorities leaving the labor force. Between 1995 and 1999, relative employment for minorities in California fell by 2.8 percentage points as these groups left the labor force, causing non-participation rates to climb by a corresponding 2.9 percentage points. Disaggregated, non-participation rates in California rose by 2.9 percentage points for white women, 4.6 percentage points for black women, 5.2 percentage points for Hispanic women, 1.4 percentage points for Hispanic men, and 6.8 percentage points for "other" men. There appears to have been little corresponding change in wages rates, but this may reflect skill bias in the workers who remained employed in later years.

To the extent that this decline in minority participation resulted from the removal of affirmative action and not other concurrent policy changes, it raises doubts about affirmative action programs. It is consistent with one of three hypotheses: that affirmative action was inefficient and created reverse discrimination; that affirmative action was efficient, but ineffective at engendering permanent change in prejudices; or that the sources of inequality were not prejudice-based. A final possibility is that California's affirmative action programs had been working effectively while in place, but had not been in existence long enough to engender permanent alterations in inequality.

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