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Source: *The Journal of Political Economy*, Vol. 104, No. 5 (Oct., 1996), pp. 869-895
Published by: The University of Chicago Press
Stable URL: http://www.jstor.org/stable/2138945
Accessed: 17/08/2009 11:58

The Role of Premarket Factors in Black-White Wage Differences

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Many attempts to measure the wage effects of current labor market discrimination against minorities include controls for worker productivity that (1) could themselves be affected by market discrimination and (2) are very imprecise measures of worker skill. The resulting estimates of residual wage gaps may be biased. Our approach is a parsimoniously specified wage equation that controls for skill with the score of a test administered as teenagers prepared to leave high school and embark on work careers or postsecondary education. Independent evidence shows that this test score is a racially unbiased measure of the skills and abilities these teenagers were about to bring to the labor market. We find that this one test score explains all of the black-white wage gap for young women and much of the gap for young men. For today's young adults, the black-white wage gap primarily reflects a skill gap, which in turn we can trace, at least in part, to observable differences in the family backgrounds

We appreciate the assistance of Jane Arabian of the Department of Defense. We also acknowledge very useful comments by Joe Altonji, Rebecca Blank, Francine Blau, Steve Cameron, Arthur Goldberger, James Heckman, Bo Honore, Joe Hotz, Larry Kahn, Alan Krueger, Bruce Meyer, Ed Olsen, Randy Olsen, Sherwin Rosen, Jon Skinner, James Smith, Steve Stern, Robert Topel, Finis Welch, and workshop participants at McMaster, NBER, Northwestern, and the Universities of Chicago, Illinois, Toronto, Virginia, and Wisconsin and anonymous referees. Neal gratefully acknowledges support by the John M. Olin Foundation both from its grant to the Center for the Study of the Economy and the State and from the Olin Faculty Fellowship. Johnson thanks the Bankard Fund for support. A substantial portion of this research was completed while Johnson was visiting the Department of Economics at the University of Chicago. Opinions and remaining errors are ours alone.

[*Journal of Political Economy*, 1996, vol. 104, no. 5]

and school environments of black and white children. While our results do provide some evidence of current labor market discrimination, skill gaps play such a large role that we believe future research should focus on the obstacles black children face in acquiring productive skill.

The analysis of the black-white wage gap typically assigns some responsibility to the observable productive characteristics each group of workers brings to the labor market and treats the remaining residual of unexplained wage differences as a measure of current labor market discrimination. Most studies conclude that although differences in worker characteristics are important sources of black-white wage differentials, current labor market discrimination accounts for at least one-third to one-half of the overall gap.

In this paper, we attempt to address two well-known problems that have plagued numerous previous empirical studies of black-white wage gaps. First, some do not account for the fact that many productive characteristics are endogenous and can be affected by labor market discrimination.[1] In empirical studies of black-white wage differences, researchers have included controls for characteristics such as occupation, postsecondary schooling, part-time work, marital status, geographical location, and actual labor market experience (see Corcoran and Duncan 1979; Reimers 1983; Smith and Welch 1986; O'Neill 1990; Blau and Beller 1992; Oaxaca and Ransom 1994). Since all these variables are subject to worker choice and could be contaminated by current labor market discrimination, controlling for them in wage regressions may misstate the wage effects of current discrimination.

At the same time, most studies do not adequately address the fact that, on average, blacks and whites enter the labor market with different levels of skill. Although years of school is typically used as a measure of worker skill, this variable is less than satisfactory. To begin, years of schooling is an inherently noisy measure of worker skill because it measures an input, not an outcome.[2] Moreover, years of school may systematically overstate the relative skill of blacks. Evidence from standardized tests indicates that black children exhibit

[1] Blinder (1973) was the first to distinguish between and to estimate structural and reduced-form wage equations in the context of discrimination. Cain's (1986) survey of the literature on estimating wage discrimination also discusses this issue.

[2] Because blacks receive less formal schooling than whites, it is straightforward to show that this source of measurement error creates a bias toward overstating the magnitude of the black-white wage gap.

lower levels of achievement than white children in the same grade.[3] As a consequence, analyses that rely on schooling as a measure of skill will likely overstate the effect of current labor market discrimination on wages and confuse the barriers that black children face in acquiring human capital with the obstacles that black adults face when they enter the labor market.

We use the National Longitudinal Survey of Youth to examine the black-white wage gap among workers in their late twenties. In our regressions, we control for a single measure of skill, the Armed Forces Qualification Test (AFQT). We argue that our approach improves on previous work in this area because the test is taken by our sample before market entry and is therefore less likely to be contaminated by worker choices or labor market discrimination. Further, as we show later on, independent studies verify that the AFQT is a racially unbiased measure of basic skills that helps predict actual job performance.

Our results can be interpreted as estimates of the portion of the overall racial wage gap attributable to human capital formation before the age of 16–18. Even though we do not observe every aspect of skill, our estimates will not overstate this portion unless blacks surpass whites in unobserved productive characteristics.

The first half of the paper presents the basic wage regressions and shows their robustness to alternative specifications or interpretations of the data. The second half of the paper explores some of the reasons black youths acquire less skill than whites. Family background variables that affect the cost or difficulty parents face in investing in their children's skill explain roughly one-third of the racial test score differential. Measures of school environment account for part of the remaining gap in test scores.

While we find some evidence of labor market discrimination, we conclude that the disadvantages young black workers now face in the labor market arise mostly from the obstacles they faced as children in acquiring productive human capital. Our analysis suggests that public policy should focus on the plight of black children in acquiring skills valued by the labor market.

I. The Basic Result

The model underlying our empirical results views the amount of human capital youths have attained by their late teens as a predeter-

[3] Data from the High School and Beyond Survey of 1980 (by the National Center for Educational Statistics) show that among high school students in the same grade, mean scores for black children fall well below the means for whites on not only tests of math and verbal achievement but also tests of writing, science, and civics achievement.

mined initial condition that constrains the future path of human capital and, hence, future wages. After the late teens, further investments in human capital, work experience, and occupation are endogenous choices that affect wages but are constrained by the initial level of human capital. Therefore, using postsecondary education, experience, and occupation as regressors in a wage equation would bias our estimate of the effect of race on wages if discrimination against blacks causes them to choose jobs and training opportunities different from those chosen by whites. Instead, we look at reduced-form wage equations that include only variables that are exogenous or determined before labor market entry: ethnicity, gender, age, and test score. These reduced-form wage equations are appropriate because we are primarily interested in the total effect of race on wages after age 18, not the partial effect conditioning on endogenous covariates. We can then estimate the share of the total racial wage gap determined by the time a young person is in his or her late teens.

Ideal data for estimating the effect of labor market discrimination on black-white wage gaps could be generated by a social experiment that observes a group of identically skilled teenagers both toward the end of secondary school and later during their labor market careers. Everything relevant for wages that happens to them after secondary school could be affected by discrimination: postsecondary schooling, marriage, occupation, on-the-job learning, and so on. Under the assumption that there are no racial differences in discount rates or willingness to supply labor, the wage gaps observed during their careers would then represent the cumulative effects of labor market discrimination.

Instead of ideal experimental data, we use a sample of individuals for whom we have a good measure of skill that is not directly affected by career choices or labor market discrimination because the measurement is taken just before these workers enter the labor market or make important choices about schooling. Such a sample can be found in the National Longitudinal Surveys of Youth (NLSY), a panel data set of 12,686 young people born between 1957 and 1964.[4] The NLSY consists of both a nationally representative cross-section sample and a supplemental sample designed to oversample blacks, Hispanics, and low-income whites. The oversamples of blacks and Hispanics represent random samples for the black and Hispanic populations. Our analysis combines the cross-section sample and the supplemental samples of blacks and Hispanics. The resulting sample contains ran-

[4] The data are described in more detail in App. table A1. A data file is available from the authors on request.

dom samples within racial or ethnic groups, although as groups blacks and Hispanics are overrepresented.

In 1980, the Armed Services Vocational Aptitude Battery (ASVAB) was administered to over 90 percent of the members of the NLSY panel. This is a set of 10 tests, of which a subset of four constitutes the AFQT.[5] The military services use the AFQT for enlistment screening and scores on various parts of the entire ASVAB for job assignment within the military. When the AFQT was administered in 1980, the NLSY panel members, born between 1957 and 1964, ranged from 15 to 23 years old. The older youths in this group had already entered the labor force as full-time workers or proceeded to postsecondary education. Job experience and postsecondary education surely enhance human capital and will therefore increase test scores. If discrimination limits access to these human capital investments, then postentry discrimination contaminates the test scores. To reduce this possibility, we restrict the sample to those younger members whose schooling choices were constrained by compulsory schooling laws until at least 1978 and who likely would not have entered the labor market full-time by 1980. We analyze respondents born after 1961 who would have been 18 or younger when they took the AFQT. Most of this group had neither entered the labor market full-time nor started postsecondary schooling when they were tested.[6] As a consequence, discrimination either in the labor market or in postsecondary education could not directly affect the test performance of blacks in this young cohort.

The results presented in the paper pertain to this younger subset of the NLSY panel, which we feel provides the cleanest estimates of residual wage gaps. However, when we estimate every specification using the full sample, we find that the implied black-white differences in the means of the conditional wage offer distributions are slightly smaller. This result is expected if discrimination adversely affects access to learning opportunities in the labor market or postsecondary

[5] There are two different scoring systems for the AFQT. The 1980 version employs ASVAB scores from the paragraph comprehension, arithmetic reasoning, word knowledge, and numerical operations tests. The 1989 version employs the mathematics knowledge score instead of the numerical operations score. Here, we use the 1989 scoring system. In an earlier version of this paper, we reported results based on the 1980 version. In almost every specification, black-white wage gaps are slightly smaller when the 1980 version is used as a control for premarket skill. Further, the correlations between wages and the 1980 version are a little stronger for all racial groups. Nonetheless, we employ the 1989 version because the military validation studies relating to racial fairness are more exhaustive for this later version.

[6] No respondent in this sample had completed a year of schooling beyond high school by May 1980 and less than 1 percent had even enrolled in college by this date. The AFQT was administered in the summer of 1980.

education. Then the AFQT scores of older blacks will in part reflect the consequences of discrimination.

Columns 1 and 4 of table 1 show simple regressions of log wage rates in 1990 and 1991 (when this sample was aged 26–29) on age and ethnic or racial group dummies for men and women, respectively.[7] The coefficients on black, −.244 and −.185, are measures of the unadjusted log wage gaps between blacks and whites. We seek to explain these gaps.

Using the AFQT score as the measure of skill in the log wage regressions produces our central results, shown in columns 3 and 6 of table 1.[8] Since panel members took the AFQT at different ages and scores clearly rise with age, we adjusted the raw AFQT score for age at the test date and also normalized the score so that the sample mean is zero and the standard deviation is one. Our normalized AFQT variable is highly significant in the wage regression and reduces the magnitude of the coefficient on black to −.072 for men and .035 for women. This test score explains nearly three-quarters of the racial wage gap for young men and all of the gap for young women. Moreover, when wage rather than log(wage) is used as the dependent variable, unreported results show small statistically insignificant racial differences in wages for either sex when AFQT is included.[9]

The wage regressions in columns 3 and 6 show that the average marginal effect of a standard deviation of test score on log wages is roughly .2 for both men and women. Since the black mean test score for each sex is about a standard deviation lower than the corresponding white mean, the test score gaps account for large portions of the black-white log wage gaps of −.18 and −.24 found for women and men, respectively.

Our estimates show that, when AFQT is held constant, black and Hispanic women earn more than white women. In fact, Hispanic women earn about 15 percent more, and the estimated differential is clearly statistically significant. We do not have a good explanation for this result, but we do offer two observations. First, Murnane, Willett, and Levy (1995) report a similar result when they examine

[7] The wage variable is the log of the mean of real wages in 1990 and 1991 for workers who worked in both years and the log of the real wage in the year of employment for workers who worked in only one year. Those who worked in neither year have no wage data and are excluded from these regressions.

[8] The square of AFQT, intended to capture deviations from log-linearity, is not significant here but is included to preserve comparability with later specifications. In a few instances, the deviation from linearity is significant.

[9] In these regressions, similar to those in table 1, black men earn $0.27 per hour less than white men and black women earn $0.03 per hour more than white women, but neither difference is statistically significant.

TABLE 1

LOG WAGE REGRESSIONS BY SEX

	MEN (N = 1,593)			WOMEN (N = 1,446)		
	(1)	(2)	(3)	(4)	(5)	(6)
Black	−.244	−.196	−.072	−.185	−.155	.035
	(.026)	(.025)	(.027)	(.029)	(.027)	(.031)
Hispanic	−.113	−.045	.005	−.028	.057	.145
	(.030)	(.029)	(.030)	(.033)	(.031)	(.032)
Age	.048	.046	.040	.010	.009	.023
	(.014)	(.013)	(.013)	(.015)	(.014)	(.015)
AFQT	...	...	.172	...	...	.228
			(.012)			(.015)
$AFQT^2$	...	...	−.013	...	...	.013
			(.011)			(.013)
High grade by 1991	...	.061	...	...	.088	...
		(.005)			(.005)	
R^2	.059	.155	.168	.029	.191	.165

NOTE.—The dependent variable is the log of hourly wages. The wage observations come from 1990 and 1991. All wages are measured in 1991 dollars. If a person works in both years, the wage is measured as the average of the two wage observations. Wage observations below $1.00 per hour or above $75 are eliminated from the data. The sample consists of the NLSY cross-section sample plus the supplemental samples of blacks and Hispanics. Respondents who did not take the ASVAB test are eliminated from the sample. Further, 163 respondents are eliminated because the records document a problem with their test. All respondents were born after 1961. Standard errors are in parentheses.

skill-adjusted gaps among 24-year-old women.[10] Second, it is possible that selection effects contaminate the estimates of racial wage gaps for women. For all women, the mean of observed wages likely overstates the mean of the wage offer distribution. If this selection effect is most acute in the minority samples, the results in table 1 will understate the wage costs of racial discrimination suffered by women. Such a result seems likely if highly skilled minority women have less nonearned income than their white counterparts.

However, since we have no direct evidence concerning the extent of selection bias in the three samples of women, we focus most of our attention on men. We present parallel results for women, but a complete analysis of the racial wage gaps observed among women remains a topic for further research.

The usual approach is to control for skill with a schooling variable. When years of schooling (in 1991 when wages are observed) is used instead of AFQT as the measure of skill (as shown in cols. 2 and 5 of table 1), it reduces the unadjusted wage gap by only one-fifth for men and only one-sixth for women.

[10] Murnane et al. report a Hispanic-white wage gap of .105 among women. Their approach differs from ours not only because they look at younger workers but also because they include numerous controls for work history and family background.

Some have argued that our specification should include controls for both AFQT and either years of total schooling or years of schooling following the AFQT. We prefer the AFQT only specification for several reasons. Given AFQT, schooling measures serve as proxies for skills that either are not captured by AFQT or are acquired after the test date. In either case, schooling is an indirect measure of these skills, and it is straightforward to show that given the other controls in our specification, this source of measurement error introduces a bias toward overstating the black-white wage gap. Further, as we noted previously, this bias will be magnified if years of schooling is not only a noisy measure but also one that systematically overstates the relative skill of blacks.

Finally, in our sample, schooling completed after the AFQT is primarily schooling completed beyond the age of compulsory attendance and is therefore endogenous. Postsecondary schooling decisions are based in part on expected pecuniary returns from further educational investments, which will, in turn, be affected by patterns of discrimination in the labor market. Our goal here is not to document all the ways that discrimination might affect career paths, but instead to provide a summary measure of the effect of current labor market discrimination on wages.[11]

Nonetheless, for completeness, we provide an Appendix table with results from three different specifications that include controls for both AFQT and measures of either total schooling or schooling completed after the AFQT.[12] Because the estimated returns to schooling conditional on AFQT are significantly greater for blacks than for whites, we estimated each of the specifications separately for blacks and whites. Then, for each specification, we formed two estimates of the black-white wage gap, one based on the sample means of observed characteristics in each sample. Our six estimates of the conditional log wage gap range from −.054 to −.093. The median of these estimates is −.075. Among women, the estimated black-white gaps are small conditional on these measures of education, and in five of six cases, they are statistically insignificant. So, while we prefer the specification without any schooling variables, results from the specifications that include them support our main conclusions.

It is useful to compare these results with those of other studies, many of which use different data sets and a wider range of ages than

[11] Separate analyses of the black and white samples show that among students with identical age-adjusted AFQT scores, blacks earn higher returns to additional schooling and, in fact, complete about two quarters more of additional post-AFQT schooling.

[12] In App. table A2, we employ (i) total grades completed by 1991, (ii) grades completed after taking the AFQT, and (iii) dummies for high school and college graduation.

we do. O'Neill's (1990) study employs the 1980 version of the AFQT as a measure of skill in wage equations on NLSY data. O'Neill derives black-white wage gaps for men between 22 and 29 years of age. Her regressions of log wages on total years of schooling, potential experience, region, and AFQT imply estimates of the residual black-white gap that range from −.046 to −.101. When she includes in her regressions additional controls for industry, occupation, and actual work experience, the black-white wage gap disappears.

All of O'Neill's specifications include controls that may be affected by current labor market discrimination.[13] In addition, it appears that her analysis included the NLSY supplementary sample of economically disadvantaged whites. For these reasons, O'Neill's results may understate the effects of current labor market discrimination.[14]

Oaxaca and Ransom (1994) used Current Population Survey data on men over 25 and found a log wage gap between blacks and whites of −.221, which fell to −.125 with controls for observable characteristics. Reimers (1983) found an unadjusted log wage difference of −.233 in the Survey of Income and Education data on men of all ages, with an adjusted gap of .132. Corcoran and Duncan (1979) estimated the residual black-white wage gap for men of all ages in 1975 using an extensive list of variables from the Panel Study of Income Dynamics but could explain only 53 percent of it. Even though all these studies use many independent variables as controls, we can account for a greater portion of the unadjusted wage gap with a single measure of skill.

Cutright's (1973) study relating AFQTs for Korean War draftees to their earnings in 1964 yields results roughly similar to ours for whites, but a much lower payoff to skill for blacks. As a result, he finds that AFQT explains only a quarter of the black-white wage gap, which is a much smaller fraction than our results in table 1 suggest. The contrast between our results for 1990–91 wages and Cutright's results for 1964 is consistent with the well-documented advance in the relative wages of blacks that occurred after the civil rights legislation of the mid-1960s (see Freeman 1981; Donohue and Heckman 1991).

[13] In O'Neill's sample, AFQT is endogenous because she includes people who were aged 19–23 when they took the test and therefore may have started postsecondary schooling or full-time work.

[14] Further, O'Neill includes in her wage regressions workers who are 22–25 years of age, ages at which wage differences are likely to understate lifetime differences in earning capacity. The hypothesis that O'Neill's results understate the black-white wage gap is supported by the fact that the *unadjusted* wage gap is smaller in her sample than in our study and others similar to it. In related work on returns to educational quality, Maxwell (1994) also notes that, among men, controls for AFQT reduce black-white wage gaps substantially.

We now discuss possible objections to our interpretation of the results in table 1.

Is the AFQT Racially Biased?

An obvious objection to our interpretation of table 1 is that the AFQT is a racially biased test in the sense that its scores underpredict productivity or job performance for blacks compared to whites. For many tests, it would be impossible to judge the validity of such an assertion because we typically have no way of directly measuring job performance and relating it to the test scores received. However, in 1991 the National Academy of Sciences (NAS) completed an exhaustive study with the Department of Defense of the validity of the AFQT with special emphasis on the racial fairness of the test. The unique aspect of the NAS study is that job performance was measured without using either supervisor evaluations or written tests, two methods that could be seen as introducing racial bias. Instead, for several military occupational specialties, direct measures of performance on the tasks constituting the job were undertaken. As an example, the job of infantry rifleman in the Marine Corps was broken into 15 tasks and each task further divided into subtasks. Subtasks were small enough that performance could be evaluated by a (1, 0) yes-no scoring system, which ensured a high degree of consistency across evaluators. Military job experts designed a weighting system that translates the subtask scores into a composite job performance measure.[15] Then these "hands-on" measures of job performance were regressed on the AFQT score of the individual at the time he or she enlisted in the military.

How well does AFQT predict military job performance? For the 23 military occupations studied, the correlations between AFQT scores and job performance ranged from .13 to .49, with a median correlation of .38.[16] The more important question, however, concerns racial bias, a key issue for the NAS panel. It concluded that AFQT does not systematically underpredict black job performance relative to white performance: "for practical purposes the same regression lines predicted performance about as well for both groups" (Wigdor

[15] Examples of tasks tested are land navigation, squad automatic weapons, first aid, night vision device, rifle, live fire, etc. (see Wigdor and Green 1991, vol. 1).

[16] These correlations are likely to understate the correlation between AFQT and a general skill or capacity to learn a specific task because selection into military occupational specialties is accomplished in part with test scores. Hence the range of test scores for any particular job is truncated. Since AFQT is also used to select individuals into the military, any observations about racial differences in the power of AFQT to predict military job performance apply only to the individuals joining the military.

and Green 1991, p. 179).[17] If anything, test scores slightly overpredict job performance by blacks. We view the NAS findings as strong independent verification that the AFQT can be considered a racially unbiased predictor of success in acquiring new skills in the military, and we have no reason to believe that the AFQT would be a racially biased predictor of success in acquiring civilian job skills.

Do Blacks Underinvest in Skill Because the Return Is Lower?

Models of discrimination developed by Arrow (1973) and Lundberg and Startz (1983) yield discriminatory equilibria from black-white differences in the return to acquiring skill. In both models, blacks with more skill have more difficulty distinguishing themselves to employers than high-skill whites, and therefore the payoff to acquiring skill is lower for blacks. Our results in table 1 indicate that blacks and whites earn different wages in large part because they typically begin their careers with different levels of human capital. These models of discrimination highlight the possibility that black youths enter the labor market with relatively few skills simply because they anticipate that the returns from acquiring skills will be low.

We investigate this possibility in tables 2 and 3. While we have no direct evidence about the expectations of these youths, we can look for differences among blacks, whites, and Hispanics in the realized effects of AFQT scores on civilian wages. The regression equations reported in column 1 of both tables 2 and 3 include an interaction between black and AFQT. For men, there is some indication that black men fare relatively better at the high end of the AFQT distribution. For women, the opposite is true. However, for both sexes, the estimated coefficients on the interaction terms are jointly insignificant.[18] The remaining results in tables 2 and 3 show the marginal effect of AFQT on log wages for each racial group. There are small, statistically insignificant black-white differences for men in table 2, and columns 2 and 3 of table 3 show that AFQT exerts an almost identical effect on the wages of black and white women. For both black and white men and women, the law of one price roughly holds for skill as measured by AFQT. Nonetheless, since the Cutright (1973) study found that the return to skill investment was lower for

[17] At the mean level of black test scores, the average overprediction of black performance, in standardized units, is .15 when the job includes at least 75 blacks tested (Wigdor and Green 1991, p. 178). Overprediction also occurs on average for jobs with smaller samples of blacks.

[18] Under the null hypothesis that the coefficients on both interaction terms are zero, the F-statistics for the male and female regressions are 2.20 and 2.17, respectively.

TABLE 2

TESTING FOR RACIAL DIFFERENCES IN THE RETURN TO AFQT: MEN

	All Races (N = 1,593) (1)	White (N = 825) (2)	Black (N = 466) (3)	Hispanic (N = 302) (4)
Black	−.107	...	...	...
	(.033)			
Hispanic	.003	...	...	...
	(.029)			
Age	.038	.052	.047	−.014
	(.013)	(.017)	(.025)	(.035)
AFQT	.172	.183	.208	.124
	(.015)	(.017)	(.031)	(.031)
$AFQT^2$	−.023	−.018	.031	−.066
	(.013)	(.015)	(.025)	(.031)
Black × AFQT	.037	...	...	...
	(.031)			
Black × $AFQT^2$	.056	...	...	...
	(.028)			
R^2	.170	.155	.129	.074

NOTE.—The "all races" sample includes all men from the sample described in table 1. All respondents were born after 1961. Standard errors are in parentheses.

TABLE 3

TESTING FOR RACIAL DIFFERENCES IN THE RETURN TO AFQT: WOMEN

	All Races (N = 1,446) (1)	White (N = 726) (2)	Black (N = 428) (3)	Hispanic (N = 292) (4)
Black	.079	...	...	...
	(.037)			
Hispanic	.137	...	...	...
	(.034)			
Age	.023	.017	.015	.055
	(.015)	(.022)	(.024)	(.030)
AFQT	.212	.189	.223	.202
	(.019)	(.030)	(.029)	(.030)
$AFQT^2$	.031	.059	−.039	−.025
	(.016)	(.025)	(.030)	(.029)
Black × AFQT	−.011	...	...	...
	(.038)			
Black × $AFQT^2$	−.071	...	...	...
	(.037)			
R^2	.168	.137	.166	.154

NOTE.—The "all races" sample includes all women from the sample described in table 1. All respondents were born after 1961. Standard errors are in parentheses.

blacks in 1964, we cannot rule out the possibility that the young black adults in the NLSY or their parents expected lower returns to skill when they chose levels of investment. Our data cannot address this issue.

Although Hispanic women earn returns to AFQT that resemble the returns for black and white men and women, it is puzzling that Hispanic men earn substantially lower returns. We do not have an explanation for this result, but we can report that our estimate of the conditional black-white wage gap changes little when we drop Hispanics from the sample.

What about the Labor Market Dropouts?

The work of Butler and Heckman (1977) and Brown (1984) has alerted labor economists to the importance of considering differences in labor force participation by race when estimating wage differences. Since market wages for nonparticipants are not observed, they are typically dropped from standard wage equations (as they were from the regressions reported in tables 1, 2, and 3). In the male sample, labor market dropouts are disproportionately black and are likely to have relatively low wage offers. Figure 1 shows that, at most levels of AFQT, labor force participation rates for black men are lower than the rates for white men.[19] The exclusion of nonparticipants could understate the effect of race on the mean of the male wage *offer* distribution. One way to address the selection problem is to model the labor force participation decision explicitly and estimate a structural model of wage offers and participation. However, the difficulty of identifying such a model led us to consider other approaches.[20]

We cannot make inferences about the wage offer distribution without some assumption concerning the wage offers of nonparticipants. Suppose that all nonparticipants have wage offers below the median offer made to workers with comparable skills. In this case, we can estimate medians of conditional log wage offer distributions by assigning nonparticipants an arbitrarily low wage.[21] Under the addi-

[19] A participant is defined as someone who reported in either 1990 or 1991 that he or she had worked at some time since the last NLSY interview. Interviews are about a year apart. This definition is not the same as the Current Population Survey definition, which is whether one worked last week. Our measure will, of course, yield higher rates of participation.

[20] Identification is achieved in such models with either exclusion restrictions or assumptions about the functional form of the error term. Exclusion restrictions are problematic in the case of male workers because it is difficult to conceive of a variable that affects participation but does not affect the market wage.

[21] We do not pursue a similar strategy with the female sample, in part because this assumption seems implausible for women. The women in our sample are in their late

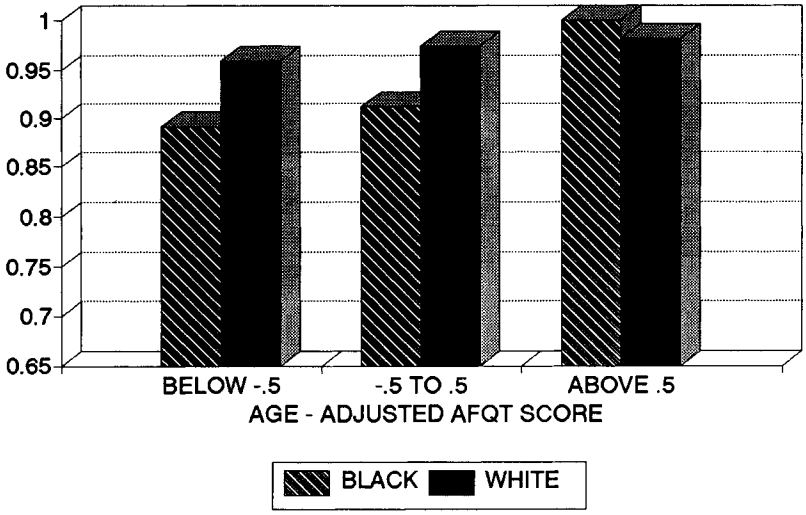

FIG. 1.—Male participation rates, 1990–91

tional assumption that the means and medians of the conditional log wage offer distributions are equal, this approach yields a consistent estimate of the black-white gap in mean log wage offers.

To illustrate, suppose that the best wage offer for worker i is a log-linear function of characteristics:

$$\ln(\text{wage offer}_i) = \beta_w \text{white}_i + \beta_b \text{black}_i + \beta_h \text{Hispanic}_i + \alpha \text{AFQT}_i + \epsilon_i, \tag{1}$$

where ϵ_i is an independent draw from a distribution $G(\epsilon_i)$ whose mean and median are both zero. The effect of race on the mean of the log wage offer distribution is the same as the effect of race on the median of the log wage offer distribution, namely $\beta_b - \beta_w$. Consider a group of individuals with identical characteristics. If all the nonparticipants in that group have wage offers less than the median wage offer for the group and if more than half participate, then the median of the log wage offer distribution is the same as the median of the distribution formed by adding the nonparticipants to the bottom of the observed market log wage distribution.[22] Therefore, we

twenties. For this group, child care demands may be an important factor in participation decisions. Therefore, the link between wage offers and participation may be weakened.

[22] Even at very low AFQT scores, our data show participation rates of men well above 50 percent.

TABLE 4

MEDIAN LOG WAGE REGRESSIONS: MEN
(N = 1,674)

	(1)	(2)
Black	−.352	−.134
	(.029)	(.035)
Hispanic	−.180	−.007
	(.034)	(.038)
Age	.067	.055
	(.015)	(.017)
AFQT	...	.206
		(.015)
$AFQT^2$	...	−.010
		(.014)

NOTE.—The dependent variable is log hourly wages. The sample is the sample described in table 1 plus the sample of nonparticipants. Nonparticipants include workers who report not working between their 1989 and 1991 interviews. Nonparticipants also include workers who did not work between their 1989 and 1990 interviews and were not interviewed in 1991. Some respondents are excluded from the previous regression analyses solely because their wage observations are invalid. These respondents are also excluded from this analysis. All respondents were born after 1961. Standard errors are in parentheses.

construct our sample of log wage offers by assigning log wages of zero (hourly wages of one cent) to all male nonparticipants. This strategy ensures that our imputed offers for nonparticipants always fall below the relevant conditional medians. Table 4 presents median regression results based on this sample.

The racial wage gap at the median moves from −.352 to −.134 when AFQT is added to the regression. Whether we condition on AFQT or not, these median regressions show a larger negative effect of being black than the regressions on participants in table 1, where the adjusted gap for men was −.072. The contrast between the results at the mean and at the median supports the view that looking only at participants masks some discrimination. Nonetheless, over 60 percent of the difference in medians is explained by our one measure of skill.

Smith and Welch (1986) use a different method to estimate the racial difference in the conditional means of the wage offer distributions. They observe that the mean of the wage offer distribution, $E(w)$, is a weighted average of the mean wage offers for participants and nonparticipants:

$$E(w) = \text{LFPR}\, E(w \mid \text{participate}) + (1 - \text{LFPR}) E(w \mid \text{don't participate}). \tag{2}$$

The ratio of the means of the wage offer distributions facing two groups, i and j, can therefore be written as

$$\frac{E(w_i)}{E(w_j)} = \left[\frac{E(w_i|\text{participate})}{E(w_j|\text{participate})}\right]B, \tag{3}$$

where B, the selection bias, is equal to

$$B = \frac{(1 - k_i)\text{LFPR}_i + k_i}{(1 - k_j)\text{LFPR}_j + k_j}, \tag{4}$$

and

$$k_i = \frac{E(w_i|\text{don't participate})}{E(w_i|\text{participate})}.$$

Conditional on the sample labor force participation rates of each group, we can derive B for various values of k, the ratio of the means of nonparticipant wages to participant wages.[23] If $k_w = k_b$, then k must be .1 or less in order to generate the selection bias implied by the difference between our mean and median regression results. To see this, note that the mean gap of −.072 log point implies a black/white wage ratio of .931, and the median gap of −.134 yields a selection corrected ratio of .875. The ratio of these two is .94. If we assume that $k_b = k_w = .1$ and use the sample labor force participation rates (.91 for black men and .975 for white men) as proxies for the true probabilities of participation, the Smith-Welch bias formula gives $B = .94$.

The difference between the race effects in the mean and median regressions shows that selection bias may contaminate our ordinary least squares estimates of black-white wage gaps. However, using the Smith-Welch method, we cannot generate such a large correction for selection bias unless we are willing to assume that the mean wage offer of nonparticipants is only one-tenth of the mean offer among observationally similar participants. Since equation (4) follows directly from basic statements about conditional expectations, we feel comfortable viewing the −.134 gap as an upper bound on the absolute value of the black-white gap in mean wage offers.

This section began with a discussion of median regression. We have also examined the black-white wage gap at the seventy-fifth and nine-

[23] It is obviously difficult to measure this quantity directly. Smith and Welch compare the wages of individuals who participate intermittently with those who participate all the time, but their measure is probably upwardly biased because those who do not participate at all, whose wages are never observed, likely have the lowest wage offers. Another approach is to make distributional assumptions about the wage offer distribution and then infer the unobserved lower tail from the observed accepted wages.

tieth percentiles of the wage distribution. For men, both the conditional and unconditional black-white log wage gaps are smaller at these percentiles than at the median. In fact, the estimated conditional gap at the ninetieth percentile is only −.042 and is statistically insignificant. Thus there is no evidence that the black-white gap in mean offers is driven by the systematic exclusion of blacks from the best jobs available to workers of a given skill level. Conditional on AFQT, the gap in median log wages between blacks and whites is much greater than the gap at the ninetieth percentile of the distributions.

How Well Does the Wage Gap at Age 30 Represent the Lifetime Gap?

Our data restrict us to looking at labor market outcomes for workers in a fairly narrow age range (ages 26–29). One might object that evidence on wages of young adults cannot be used to make inferences about the wage gap for the rest of the life cycle. If the lifetime trajectories of log wages for whites and blacks were parallel, one could extrapolate the results here to say something about lifetime earnings. However, if discrimination in the labor market prevented blacks from investing heavily in on-the-job experience, then blacks may have flatter log wage trajectories and the log wage gap may widen with age. Unfortunately, there is contradictory evidence about the black-white log wage gap over the life cycle. Smith and Welch (1986) generally find narrowing of the unadjusted gap in decennial census data through 1980 as a cohort ages. However, Boozer, Krueger, and Walkon (1992, p. 317) include 1990 data and show that for older cohorts (born before 1940) the gap has narrowed over the life cycle; for younger cohorts, the reverse is true. Whether the pattern of these younger cohorts will be maintained over time is an open question. Further, for a given cohort, trends in the overall black-white wage gap may be different from trends in the gap conditional on premarket skill.

II. The Determinants of AFQT Scores

Now that we have established the importance of the AFQT score as a measure of the skills young workers bring to the labor market and as an explanation for lower wage rates among blacks, the following natural question arises: Why do blacks score lower on this test? Figures 2 and 3 show the sample distributions of test scores by race for men and women, respectively. Over 35 percent of black men score below −1.0 but less than 10 percent of white men do. Again, we

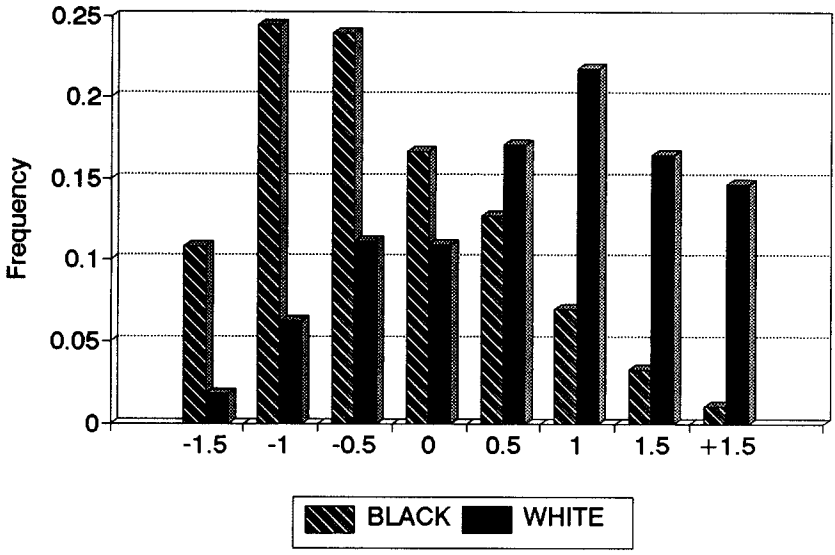

FIG. 2.—Age-adjusted AFQT scores: men

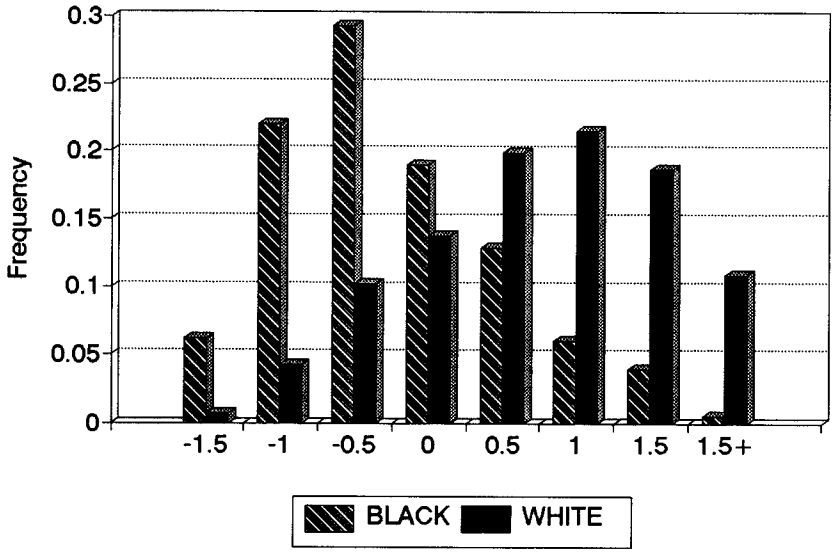

FIG. 3.—Age-adjusted AFQT scores: women

stress that we view the test as a test of achievement and learned skill, not of innate ability, so we seek reasons that black youths have acquired less skill than white youths. Since the payoff to acquiring skill is roughly the same for black and white women and men, we conclude that the investment differential between the races is likely to be driven by differences in the costs of acquiring skill. Why do black youths (and their parents) find it more costly to invest in skill than white youths? Obviously, past and current discrimination against black families affects the constraints black parents face in investing in their children. For example, if education and high income make it easier to invest in children's human capital, then part of the racial difference in AFQT scores can be attributed to racial differences in parental education and income.

Table 5 documents the extent to which observed aspects of family background account for the observed black-white gap in AFQT scores for men. Column 1 of table 5 sets the stage by estimating the unexplained score differences for men in the sample we have used for tables 1–4, those born after 1961. As column 1 shows, the mean black score is one standard deviation below the mean white score, with Hispanics about .7 of a standard deviation below. The corresponding result for women, in table 6, shows only a slightly smaller black-white score differential.

The NLSY data include many variables describing the household in which the respondent was raised. In column 2, we present results from a specification that includes controls for the parents' capacity to provide human capital for their children. The results show that the achievement of children on the AFQT varies positively with the education and professional status of their parents. Further, given these controls, the black-white gap in scores falls to −.70 for men and −.72 for women.

These measures of family background serve only as rough measures of parental resources, and although it would be difficult to construct precise measures, we do know that parental resources affect optimal choices of family size and child quality. Column 3 introduces additional controls for family size and parental investment. Both number of siblings and two measures of family reading materials are strongly correlated with test scores. Further, the black-white gaps in scores fall to −.62 for women and −.57 for men when they are included.

Schools differ in many observed dimensions, and residential segregation by race may also affect parents' capacity to invest in their children. Column 4 in tables 5 and 6 reports an AFQT regression with several school characteristics included: student/teacher ratio, dis-

TABLE 5

DETERMINANTS OF AFQT: MEN

	Full Sample (N = 1,873)			Valid Response to School Survey (N = 954)
	(1)	(2)	(3)	(4)
Black	−1.03	−.70	−.57	−.42
	(.05)	(.05)	(.05)	(.07)
Hispanic	−.70	−.31	−.22	−.02
	(.06)	(.05)	(.05)	(.08)
Mother high school graduate	...	.36	.26	.18
		(.04)	(.04)	(.06)
Mother college graduate	...	.21	.16	.09
		(.08)	(.08)	(.11)
Father high school graduate	...	.32	.25	.22
		(.05)	(.05)	(.06)
Father college graduate	...	.32	.30	.31
		(.07)	(.07)	(.09)
Mother professional	...	.20	.17	.08
		(.07)	(.07)	(.10)
Father professional	...	.26	.23	.21
		(.06)	(.06)	(.08)
Number of siblings	...	...	−.05	−.05
			(.01)	(.01)
No reading materials	...	...	−.19	−.31
			(.06)	(.09)
Numerous reading materials	...	...	.25	.27
			(.04)	(.06)
Student/teacher ratio	...	...	...	−.017
				(.006)
Disadvantaged student ratio	...	...	...	−.002
				(.001)
Dropout rate	...	...	...	−.004
				(.001)
Teacher turnover rate	...	...	...	−.005
				(.003)
R^2	.219	.382	.415	.392

NOTE.—The dependent variable is the age-adjusted AFQT score. In all specifications, the sample excludes respondents with invalid AFQT scores. In specification 4, the sample also excludes respondents with invalid responses to the school survey items employed in col. 4. Specifications 3 and 4 also include dummies for whether or not the respondent has knowledge of the educational background of his or her mother or father. Specification 4 also includes a private school dummy. The estimated coefficient is positive but not statistically significant. All background information comes from the 1979 wave of the NLSY. The dummy variables for reading materials are constructed from information about magazines, newspapers, and library cards in the home. "Numerous" means all of the above. "No" means none of the above. All respondents were born after 1961. Standard errors are in parentheses.

TABLE 6

DETERMINANTS OF AFQT: WOMEN

	Full Sample (N = 1,791)			Valid Response to School Survey (N = 926)
	(1)	(2)	(3)	(4)
Black	−.99	−.72	−.62	−.59
	(.04)	(.04)	(.04)	(.06)
Hispanic	−.77	−.45	−.37	−.30
	(.05)	(.05)	(.05)	(.07)
Mother high school graduate	...	.29	.20	.20
		(.04)	(.04)	(.06)
Mother college graduate	...	.33	.32	.24
		(.08)	(.08)	(.11)
Father high school graduate	...	.24	.18	.12
		(.04)	(.04)	(.06)
Father college graduate	...	.32	.29	.31
		(.07)	(.07)	(.09)
Mother professional	...	.15	.09	.16
		(.07)	(.07)	(.09)
Father professional	...	.15	.13	.07
		(.05)	(.05)	(.07)
Number of siblings	...	...	−.027	−.026
			(.007)	(.010)
No reading materials	...	...	−.29	−.21
			(.06)	(.08)
Numerous reading materials	...	...	.23	.23
			(.04)	(.05)
Student/teacher ratio	...	...	...	−.0043
				(.0025)
Disadvantaged student ratio	...	...	...	−.002
				(.001)
Dropout rate	...	...	...	−.003
				(.001)
Teacher turnover rate	...	...	...	−.003
				(.003)
R^2	.244	.390	.419	.431

NOTE.—See table 5.

advantaged student ratio, student dropout rate, and teacher turnover rate.[24] Each works in the expected direction, and together they further reduce the unexplained AFQT gap between blacks and whites. The residual gap falls to −.42 for men and −.58 for women.[25]

[24] The NLSY school survey obtained information directly from the high school the respondent last attended. Unfortunately, the sample sizes are smaller for this analysis because many schools did not respond. Only .45 of the black students have valid responses for the items used here. The corresponding figure for whites is .57.

[25] Boozer et al. (1992) argue that black students suffer from racial isolation in school and less access to computers, both of which act to reduce their wages as adults. In contrast, Grogger (1996) finds little direct effect of school characteristics on the racial

Even with our controls for both family and school environment, sizable black-white gaps in AFQT remain. However, we can account for a significant fraction of the overall gap using only a few measures of family background and secondary school environment. For example, elementary school environments may also be important, but we have no measure of them.

Although we believe that the black-white gap in AFQT scores reflects differences in acquired skills, Herrnstein and Murray (1994) have generated significant controversy recently by using AFQT as a measure of inherent ability. Specifically, they claim that AFQT is a nearly exogenous measure of cognitive ability that is not greatly affected by additional schooling or other human capital investments. However, our investigation generated two sets of results that are inconsistent with the claims made by Herrnstein and Murray.

Appendix table A3 presents four regressions of standard AFQT scores on dummies for race and year of birth. The regressions provide estimates of the black-white gaps in standard scores not only for the sample of respondents who took the test at age 18 or younger but also for those who were between 19 and 23 at the time of the test. To the extent that AFQT scores measure immutable individual traits, racial gaps in these scores should be constant across age groups. However, in both the male and female samples, the estimated racial gaps in scores are larger in the sample of older respondents.[26] Since differences between blacks and whites in both work experience and years of schooling grow with the age of the respondents, our results are consistent with the hypothesis that differential investment contributes to the black-white gap in scores.

Appendix table A3 provides indirect evidence that human capital investments affect AFQT scores. We also provide more direct evidence on the link between schooling and AFQT scores. We ran instrumental variables regressions of standard AFQT scores on dummies for year of birth, dummies for race, and grades of school completed by May of 1980 (the test was administered during the summer of 1980). Again, we use only respondents born after 1961 and run separate regressions for males and females. Following Angrist and Krueger (1991), we use quarter of birth as an instrument for grades completed.[27]

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wage gap. However, his analysis includes controls for variables that are outcomes of school quality, such as test scores and postsecondary schooling.

[26] In a sample that includes both cohorts, the standard deviation of AFQT scores is 38.01 for men and 35.27 for women. The four estimated age differences in racial gaps range from −3.5 to −7.03 and are statistically significant at the 10 percent level.

[27] In our sample of teenagers, the effect of birth quarter on schooling arises primarily from restrictions on the age at which students may enter school. Most localities have

These results cast more doubt on Herrnstein and Murray's claims that AFQT measures an inherent trait. For both sexes, the estimated coefficients on grades completed are large and statistically significant. The coefficient estimates imply that an additional year of schooling raises AFQT scores for men and women by .22 and .25 standard deviations, respectively.[28] Thus the black-white gap in mean scores is roughly equivalent to the skill-building effect of just over four years of secondary schooling.[29]

III. Conclusion

Our results echo a common theme in much of the recent literature on wage determination. Recent studies indicate that the return to measured skills is large in today's labor market. Although earlier research often failed to detect a strong relationship between wages and test score measures of achievement or aptitude, recent work by Bishop (1991) and by Murnane et al. (1995) finds that, during the 1980s, the labor market return to skills as measured by test scores rose dramatically.

After decades of narrowing, the unadjusted black-white wage gap has either widened or failed to shrink further since 1980.[30] Considerable disagreement exists about the causes of this recent pattern, but several studies emphasize the interaction between black-white skill gaps and the rising value of skill in the 1980s. Our results cannot directly address the question of changes in the racial wage gap over time because the limited span of birth years in the data limits our ability to observe changes in the relationship between test scores and wages for workers in their late twenties. Nonetheless our results are

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rules or guidelines concerning the age a child must be to enter school, and children born in the last quarter of the year often start school a year later than students born earlier in the same calendar year. In our data, the average of grades completed at the time of the AFQT declines slightly over the first three quarters of a given birth year and falls substantially between the third and fourth quarters.

[28] By contrast, Herrnstein and Murray claim that an additional year of schooling raises scores by only .07 standard deviation (1994, p. 591). Ceci (1991) reviews studies of the effect of schooling on measured IQ and concludes that there is an important causal link between exposure to formal schooling and measured intelligence.

[29] Bound, Jaeger, and Baker (1995) demonstrate that even in large samples, instrumental variables estimates may be seriously biased if the instruments are weakly correlated with the potentially endogenous variable. Our partial R^2's for the quarter of birth dummies in the first-stage regressions are .074 for men and .046 for women. Our F-statistics on the significance of the quarter of birth dummies in the first-stage regressions are 46.6 and 29.3, respectively. According to the criteria set forth by Bound et al., the implied bias is quite small for both males and females.

[30] See Bishop (1991), Juhn, Murphy, and Pierce (1991), Bound and Freeman (1992), Card and Krueger (1992), Ferguson (1993), and Smith (1993) for treatments of this issue.

consistent with the view that blacks have suffered relative to whites from recent increases in the market price of skill.

While our results do provide some evidence of current labor market discrimination, our primary finding is that large skill gaps between blacks and whites are an important determinant of the black-white wage differences. Future research on the determinants of the black-white wage gap should focus on the obstacles black children face in acquiring productive skill.

Appendix

TABLE A1

DESCRIPTIVE STATISTICS

	Men			Women		
	Black	Hispanic	White	Black	Hispanic	White
Age-adjusted AFQT score	−.621	−.284	.422	−.524	−.298	.465
	(.815)	(.893)	(.895)	(.743)	(.825)	(.779)
High grade completed by 1991	12.458	12.156	13.248	12.873	12.328	13.347
	(1.954)	(2.238)	(2.511)	(1.984)	(2.239)	(2.388)
Mother high school graduate	.490	.336	.757	.457	.280	.714
Father high school graduate	.493	.369	.717	.474	.372	.717
Mother college graduate	.065	.041	.112	.063	.032	.110
Father college graduate	.062	.074	.210	.071	.067	.187
Mother professional	.076	.061	.106	.103	.064	.104
Father professional	.042	.090	.287	.066	.106	.270

NOTE.—These sample means pertain to persons who were born between 1962 and 1964 and have valid responses to the relevant questionnaire items. Blacks account for approximately 30 percent of the total observations. Hispanics account for 20 percent. The total sample size is roughly 3,400, but the total number of observations varies across survey items. Standard deviations are in parentheses.

TABLE A2

A. Log Wage Regressions with Schooling and AFQT: Men

	Blacks (N = 466)			Whites (N = 825)		
	(1)	(2)	(3)	(1)	(2)	(3)
Constant	4.566	4.086	5.011	4.926	4.743	5.333
	(.689)	(.752)	(.679)	(.464)	(.504)	(.452)
Age	.051	.091	.059	.054	.075	.053
	(.025)	(.027)	(.025)	(.017)	(.018)	(.017)
AFQT	.122	.157	.139	.125	.154	.131
	(.033)	(.031)	(.031)	(.020)	(.019)	(.019)
$AFQT^2$	−.024	.022	−.012	−.030	−.030	−.036
	(.025)	(.025)	(.025)	(.015)	(.015)	(.016)
High grade in 1991	.059			.035		
	(.012)			(.007)		
School years since AFQT		.050			.024	
		(.012)			(.008)	
High school graduate			.094			.074
			(.044)			(.036)
College graduate			.270			.187
			(.067)			(.038)
R^2	.175	.159	.170	.178	.165	.186

B. Implied Black-White Gap

Specification	X = Black Sample Mean	X = White Sample Mean
1	−.093	−.073
	(.029)	(.038)
2	−.077	−.057
	(.030)	(.038)
3	−.080	−.054
	(.029)	(.038)

Note.—The dependent variable is log hourly wages. All respondents were born after 1961. Standard errors are in parentheses.

TABLE A3

Racial Gaps in Standard AFQT Scores by Sex and Cohort

	Males		Females	
	Born 1962–64 (N = 1,882)	Born 1957–61 (N = 2,579)	Born 1962–64 (N = 1,806)	Born 1957–61 (N = 2,807)
Black	−39.25	−46.28	−37.52	−40.92
	(1.76)	(1.57)	(1.64)	(1.38)
Hispanic	−27.26	−31.82	−28.85	−35.85
	(2.10)	(1.84)	(1.87)	(1.63)
R^2	.23	.27	.25	.28

Note.—The dependent variable is the standard AFQT score. Scores range from 95 to 258. In the cross-section subsample of the NLSY, the mean score is 196.5 and the standard deviation is 36.65. Each regression includes dummies for year of birth.

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