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Around 1980, the trend toward racial wage convergence essentially stopped. I ask whether this break in the convergence trend can be explained by school quality. Department of Education surveys provide earnings data for the high school Class of 1972 in 1979 and the Class of 1980 in 1986, both linked to data from the respondents' high schools. By several measures, differences between schools attended by blacks and whites were already rather small in the 1970s. Furthermore, I find that measurable school inputs generally have little effect on wages. Thus school quality explains little of the recent black/white wage trend.

I. Introduction

Between 1940 and 1980, the black/white wage differential narrowed at an average rate of 0.71 percentage points per year (Smith and Welch 1986, table 2). In the last decade of that period, moreover, wage convergence accelerated. Juhn, Murphy, and Pierce (1991) reported that wages converged at the annual rate of 1.38 percentage points during the 1970s.

Around 1980, however, the trend broke. The ratio of median black male wages to median white male wages was 0.76 in 1979; in 1988 the ratio was 0.75 (U.S. Bureau of Labor Statistics 1991, table 41). The black/white wage gap actually increased slightly, rather than decreasing as it had for 40 years. In this article I test whether educational quality can explain the break in the trend toward black/white wage equality.

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This question is motivated by the work of Juhn et al. (1991), who recently analyzed the slowdown in black/white wage convergence during the 1980s. They estimated that about one-half of the recent convergence slowdown could be attributed to lower educational attainment among blacks, coupled with the rapid increase in the returns to education that took place during the 1980s (Bound and Johnson 1992; Katz and Murphy 1992; Murphy and Welch 1992; Grogger and Eide 1995). They then attempted to determine how much of the remaining convergence slowdown could be attributed to changes in racial differences in the quality of education.

Since their data lacked any direct measures of school quality, they constructed a measure of the racial quality differential as the difference in educational attainment between whites and blacks who earned the same wage and had otherwise identical observable characteristics. They then assumed that the returns to educational quality rose in the same manner as the returns to educational attainment. They reported that differential school quality explained almost all of the slowdown in racial wage convergence not already explained by differential attainment. From this they concluded that the slowdown stems largely from market forces that have increased the returns to skill generally, rather than from factors that are "black-specific," such as increases in wage discrimination.

The main problem with Juhn et al.'s (1991) approach is that it is indirect. Indeed, they themselves argue that "what is needed is further direct evidence concerning the size of the school quality gap and the way returns to schooling quality have changed within racial groups" (p. 143). The need for such direct evidence is further highlighted by the conflicting evidence provided by Boozer, Krueger, and Wolkon (1992). They note that, during the 1980s, racial wage differentials among men born in the 1950s grew by more than those among men born in the 1930s. Since racial education differences were likely greater among the older cohort than the younger, Boozer et al. conclude that school quality is unlikely to explain the recent break in the black/white wage convergence trend.

The purpose of this article is to resolve these contradictory conclusions, each of which is based on indirect evidence, by providing direct estimates of the role of school quality in determining wages, wage differentials, and the recent break in the black/white wage trend. I employ data from two longitudinal studies commissioned by the U.S. Department of Education that follow different cohorts of recent high school graduates. I observe earnings from the Class of 1972 in 1979, and from the Class of 1980 in 1986. The benefit of these surveys is that they let me link data on the respondents' earnings with characteristics of the high schools they attended. I thus have direct measures of the quality of high schools attended by blacks and whites. Moreover, the surveys provide information not only on such traditional school quality measures as the pupil/teacher ratio, the length of the school year, and teacher education, but also on other measures such as school size and racial segregation. Hence the data permit a relatively

detailed analysis of the effects of school quality on wages and wage differentials.

Another virtue of these surveys is that they contain several measures of the respondent's ability and family background. These are valuable because studies of educational achievement have shown that omitting family background can exaggerate the importance of school characteristics on student performance (Hanushek 1986). It seems likely that such biases may arise in the study of students' postschooling earnings as well.

The findings of this article are easy to summarize. In general, my results regarding the racial wage trend contrast sharply with Juhn et al. (1991). I find that educational quality explains very little of the recent black/white wage trend. The reason is simple: by many measures, the average quality of schools attended by blacks and whites was nearly equal by the early 1970s. Moreover, the returns to school quality were generally low and changed little over time.

Indeed the finding of low returns to measured school quality is an important substantive result in itself. It also is at odds with the recent results of Card and Krueger (1992*b*), who found class sizes and the length of the school year to be important determinants of earnings. After presenting my main findings on the black/white wage trend, I discuss why their results may differ from mine.

II. Data

I analyze two longitudinal surveys supported by the U.S. Department of Education. The National Longitudinal Study of the High School Class of 1972 (NLS72) is a longitudinal survey of about 21,000 high school seniors who graduated in 1972 (National Center for Education Statistics 1981). Information about respondents' family backgrounds was collected in the 1972 base year survey. Subsequent waves of interviews collected data on respondents' work and educational experiences since leaving high school. I use wage data from the 1979 interview because it is the first wave that includes earnings data for college graduates.

The High School and Beyond (HSB) survey is a similar panel of roughly 12,000 members of the high school Class of 1980 and was intended as a follow-up to the NLS72 (Center for Education Statistics 1987). Similar background data were collected in the base year survey, and similar work and earnings measures were collected in later rounds of interviews. I use wage data from the 1986 interview in estimation.

In their senior years, students in both cohorts were administered a set of achievement tests designed to measure basic mathematical, vocabulary, and visual perception skills. Tests taken by those in the later cohort explicitly were designed to be comparable to the tests taken by those in the earlier cohort, and indeed the two test batteries share many identical questions (Hilton 1992). Self-reports of the students' grades also are included

on both base year surveys. I use these test scores and grades as measures of student ability.

For my purposes, the key feature of both surveys is that each one collected information on the respondents' high schools directly from the schools' administrative offices. These data on educational inputs can be linked with respondent records via a common identification code. I construct measures of educational quality from these data on school characteristics.

My sample is limited to men who participated in the base year survey and the follow-up wave from which the data were drawn and who were working full-time and not enrolled in school at the time their wages were observed. I further restricted the sample to persons whose hourly wage was between $1 and $100. All monetary values are expressed in 1986 dollars, deflated by the consumer price index.

My dependent variable is the logarithm of the hourly wage.[1] Besides the family background, ability, and school quality measures, my independent variables include educational attainment, labor market experience, and dummies for race, region, and urban or rural residence. Educational attainment categories are coded as binary indicator variables for high school graduates, college graduates, and postgraduate degree recipients.[2] One limitation of these surveys is that there are no high school dropouts in the sample. Since the base year interviews were conducted late in the students' senior year in high school, almost all either graduated or obtained an equivalency diploma. The few who did not were either nonrespondents in later follow-up interviews or did not meet my sample selection criteria.

Table 1 displays sample means by race for several of the variables used in the analysis. To compare the education surveys with a more familiar data source, I also present summary statistics for two comparison samples drawn from the annual earnings files of the Current Population Survey (CPS). I drew a sample of men aged 24–25 in 1979 to match the ages of the NLS72 respondents and another of men aged 23–24 in 1986 to match the HSB respondents. I exclude from the CPS samples persons with less than 12 years of education since there are no high school dropouts in my NLS72 or HSB samples. Also excluded are part-time workers. All means are weighted since all samples, particularly the HSB, are stratified.

The first row of Table 1 compares mean wages across cohorts and data sources. Current Population Survey wages are a bit lower than either NLS72 or HSB wages, for both blacks and whites. The mean wage of the early CPS sample is about 93% of the NLS72 mean; in the later cohort, the CPS mean wage is about 96% of the HSB average.

[1] Specific definitions of all variables are given in the data appendix.

[2] I count as a high school graduate anyone who finished high school but did not receive a bachelor's degree. College graduates include all respondents whose highest degree was a bachelor's. Anyone who received a higher degree is recorded as having a postgraduate degree.

Table 1
Summary Statistics

Variable	A. 1979 Samples						B. 1986 Samples					
	Class of 1972			CPS Men, 24–25			Class of 1980			CPS Men, 23–24		
	Total (1)	White (2)	Black (3)	Total (4)	White (5)	Black (6)	Total (1)	White (2)	Black (3)	Total (4)	White (5)	Black (6)
Hourly wage	10.35	10.48	9.21	9.61	9.77	8.43	8.20	8.32	7.16	7.90	8.11	6.83
	(4.93)	(5.00)	(4.10)	(3.80)	(3.84)	(3.40)	(3.51)	(3.51)	(3.43)	(3.35)	(3.36)	(2.94)
College or higher education	.24	.26	.15	.19	.20	.10	.18	.20	.09	.18	.20	.10
Experience	4.47	4.41	4.42	4.03	3.93	4.49	3.87	3.77	4.19	3.25	3.17	3.53
	(2.47)	(2.45)	(2.65)	(1.86)	(1.89)	(1.59)	(1.93)	(1.95)	(1.88)	(1.69)	(1.73)	(1.46)
Black	.07	...	...	.08	...	...	.10	...	...	.10	...	...
No. of observations	4,685	3,796	452	3,942	3,334	258	2,396	1,133	545	3,536	2,941	247

NOTE.—Based on weighted data. CPS = Current Population Survey. Standard deviations are in parentheses.

The black/white wage ratio is about the same across data sources, however. The ratio is 0.88 in the NLS72 and 0.86 in the corresponding CPS sample. The corresponding ratios for the younger cohort are 0.86 and 0.84. Both sources show a 2-percentage-point increase in the black/white wage differential between 1979 and 1986.

The next row of table 1 compares educational attainment by race across cohorts and data sources. In the CPS, the category "college or higher" includes anyone with 16 or more years of education; in the education surveys, it denotes anyone with a bachelor's degree or higher. The differences in graduation rates between the education surveys and the CPS vary by cohort. Graduation rates for the NLS72 are higher than the corresponding CPS figures, whereas the HSB gives estimates that are more similar to its corresponding CPS cohort. As a result, the CPS shows little change in graduation rates across cohorts, whereas the education surveys show a decrease.

Because of this disagreement in trends across sources, I tabulated a third set of graduation rates by dividing the number of male college graduates in 1975 and 1987 by the number of 22-year-old men in the population in those years.[3] This yielded graduation rates for the older cohort of 26% overall, 27% for whites, and 12% for blacks. For the younger cohort, the corresponding figures are 22%, 23%, and 9%. Thus the aggregate data show a decrease in graduation rates that is similar to the trend in the NLS72 and HSB. It appears that the CPS, rather than the education surveys, is aberrant on this point.

Table 2 presents means of several characteristics of the high schools attended by black and white students.[4] Although both studies collected data on many school characteristics, relatively few were common to both surveys. The first two rows report data on the pupil/teacher ratio and the length of the school term, both of which Card and Krueger (1992*a*) found to be important determinants of pre-1980 wage convergence. Mean pupil/teacher ratios were nearly equal for blacks and whites already by 1972, and little change occurred between cohorts. There was little variation in term lengths, either between or within race.

I next constructed a measure of average teacher quality by creating a dummy variable equal to one if at least 30% of the teachers at the student's high school had master's degrees or higher. I chose this particular measure because, among the several measures that I analyzed, it had the strongest

[3] Data on college graduates are from National Center for Education Statistics (1989); population data are from U.S. Bureau of Labor Statistics (1991). Graduation data for the spring of 1976 and spring of 1984, the years in which the NLS72 and HSB respondents would have graduated if they had completed college on time, were unavailable.

[4] As above and throughout this article, the student rather than the school is the unit of observation.

Table 2
School Characteristics by Race

Variable	Class of 1972			Class of 1980			Change in Black/White Ratio
	White	Black	Black/White Ratio	White	Black	Black/White Ratio	
Pupils/teacher	20.1	20.7	1.03	18.9	18.6	.98	−.05
	(4.4)	(4.7)		(4.9)	(4.1)		
Term length (weeks)	36.9	36.8	1.00	36.0	36.1	1.00	.00
	(1.8)	(1.6)		(1.0)	(1.0)		
30% or more teachers have advanced degrees	.67	.61	.91	.77	.81	1.05	−.11
	(.47)	(.47)		(.42)	(.39)		
School size (enrollment)	1,341	1,549	1.16	1,176	1,454	1.24	.08
	(869)	(967)		(705)	(818)		
% black	5.9	47.7	8.08	6.0	49.2	8.20	.12
	(11.5)	(31.7)		(11.1)	(32.9)		

NOTE.—Based on weighted data. Standard deviations are in parentheses.

effect on wages. On average, blacks and whites of both cohorts attended schools with similarly educated teachers.

The next row of the table gives average school size, measured as the number of pupils enrolled in the high school. This variable generally has a positive effect on educational achievement, presumably resulting from economies of scale (Hanushek 1986). Blacks begin at a higher, possibly more advantageous level, and relative school size increases slightly across cohorts.

The final variable measures the extent of school segregation. High levels of segregation might indicate that students are culturally isolated from the white mainstream. Moreover, Boozer et al. (1992) found school segregation to be an important determinant of wages among black men who were 25–65 years old in 1980. In both of my relatively recent cohorts, blacks attended schools with a much higher proportion of black students than did whites.

In order for these school quality measures to explain the recent black/white wage trend, however, either (1) relative school quality must have deteriorated across cohorts, or (2) quality initially must have differed by race, and the labor market value of school quality must have risen. Table 2 shows clearly that traditional measures of school quality such as pupil/teacher ratios and the length of the school year cannot explain the trend break. These inputs already were nearly identical across blacks and whites by the early 1970s and did not deteriorate over time. Moreover, since these quality measures were equal, increases in their returns would not work to the disadvantage of blacks.

On the basis of some other less common indicators of quality, however, schools of blacks differ from schools of whites. Blacks generally attend larger schools than whites, so if the advantage associated with larger schools fell over time, then blacks could end up at a greater relative disadvantage. Furthermore, the substantial level of school segregation could work to blacks' disadvantage if the effects of attending a segregated school worsened over time. In the next section, I discuss how I estimate the labor market value of the various school characteristics.

III. Empirical Approach

I base my analysis on the following regression model:

$$y_{it} = \alpha_t \, \mathrm{BLACK}_{it} + X_{it} \, \beta_t + F_{it} \, \theta_{Ft} + A_{it} \, \theta_{At} + Q_{it} \, \theta_{Qt} + \varepsilon_{it}, \qquad (1)$$
$$i = 1, \ldots, n_t; \; t = 1979, 1986,$$

where y_{it} is the logarithm of the hourly wage of individual i in year t, and BLACK_{it} is equal to one if individual i is black and zero otherwise.[5] The

[5] Employment rates also vary by race, and a similar framework could be used to assess the importance of school quality for racial employment differentials. Such an analysis is beyond the scope of this article, however.

vector of baseline regressors, X_{it}, includes the college and postgraduate degree indicators, labor market experience and its square, and dummy variables for region of residence, residence in an urban or rural area, and for Hispanic and other racial/ethnic background. Other vectors include F_{it}, a vector of family background characteristics; A_{it}, a vector of ability measures; and Q_{it}, a vector of school quality measures. The disturbance term ε_{it} is random noise. For the cross-cohort analyses that are the main focus of the next section, there is one observation per cohort: the Class of 1972 is observed in 1979, and the Class of 1980 in 1986. It is therefore equivalent to speak of changes across cohorts and changes over time. The parameters α_t, β_t, θ_{Ft}, θ_{At}, and θ_{Qt} are to be estimated from the data. In particular, α_t is my estimate of the proportionate black/white wage differential in year t, and θ_{Qt} is a vector of returns to the educational quality measures.

More precisely, θ_{Qt} shows the average change in wages that would result from a unit change in Q_{it}, conditional on educational attainment, which is included in X_{it}. Therefore, θ_{Qt} measures the direct effect of school quality on wages but does not capture any indirect effect arising from the effect of school quality on educational attainment. In general, of course, both effects are of interest. Here I focus only on the direct effect because I am interested in whether school quality explains the portion of the recent black/white wage trend not explained by educational attainment. Therefore, all statements below regarding the magnitude of the effect of a specific educational input refer only to the direct effect.[6]

From these regression estimates, I calculate the change in the black/white wage differential across cohorts, conditional on the explanatory variables, as $\hat{\delta} = \hat{\alpha}_{86} - \hat{\alpha}_{79}$, where the hat notation denotes statistical estimates. Positive values of $\hat{\delta}$ indicate convergence in the black/white wage differential, while negative values indicate divergence. Note that $\hat{\delta}$ generally will reflect both changes in quantities such as Q_{it} and changes in their returns θ_{Qt}, since no cross-cohort restrictions are imposed on θ_{Qt} in estimation.[7]

[6] Strictly speaking, even these direct estimates pertain only to workers who at least finish high school since the sample contains no dropouts. It is possible that the inclusion of dropouts, whose numbers vary by race, could alter the conclusions. Furthermore, to the extent that student achievement reflects school quality, the coefficients provide estimates of the effect of school quality net of any effects on both attainment and achievement. It is worth noting that none of the results regarding the effect of school quality on either racial wage differences or changes in racial wage differentials is affected by the inclusion of the achievement measures.

[7] An alternative approach would be to estimate separate regressions by race and use a Oaxaca (1973) decomposition to assess the importance of school quality on the racial wage gap. In practice, the results from this alternative approach yielded conclusions quite similar to those reported below.

Finally, I can estimate the effect of some characteristic Z on the change in the wage differential. Let $\hat{\delta}$ give the change in the differential when Z is excluded from the models used to estimate α_{79} and α_{86}, and let $\hat{\delta}_Z$ give the change when Z is included. The effect of Z on the change in the differential is then $\hat{\delta}_Z - \hat{\delta}$.

IV. Results

A. The Effect of School Quality on Wages, the Black/White Wage Gap, and Cross-Cohort Changes in the Racial Wage Differential

Table 3 presents estimates of black/white wage differentials and the returns to measured school inputs. Each column reports the results from a separate regression. In addition to the variables shown, each regression included college and post-college-degree dummies, potential experience and its square, region dummies, urban and rural dummies, a Hispanic dummy, family income, standardized test scores, and high school grades. All models were estimated by ordinary least squares (OLS).[8]

The first column establishes a baseline wage differential from a model that excludes all school quality variables. Columns 2–6 present estimates from specifications to which the school quality variables are entered individually. I chose this strategy due to the substantial correlation that exists among the inputs that may degrade individual coefficient estimates in a model containing several measures.

Columns 2–4 show the effects of three traditional quality measures. The pupil/teacher ratio has a surprising positive (albeit insignificant) coefficient in both samples. The coefficients of the term length variable have different signs across cohorts. The third measure performs a bit better. Teacher education has the expected sign, although it is significant in only one of the samples.

The next two columns present the effects of school size and segregation. Column 5 shows that school size contributes positively to wages in both cohorts, though its effect is significant only in the HSB sample. This finding is generally consistent with results from studies of educational achievement as measured by standardized test scores (Hanushek 1986). The magnitude of this effect is rather small, however. The coefficients indicate that an increase in high school enrollment of 1,000 students (a very large increase, based on the figures in table 2) would increase wages of the earlier cohort by 1.4% on average, and of the latter cohort by 3.6%.

Column 6 shows the effect of school segregation on future wages. The coefficients are negative in both samples, and larger and significant in the

[8] In principle, a generalized least square (GLS) estimator should be used to account for possible dependence in ϵ_{it} among students in the same school. In fact, the estimated within-school correlation was generally quite small, on the order of 0.04. As a result, the GLS estimates and standard errors were nearly identical to their OLS counterparts. For simplicity, therefore, I present OLS results.

Table 3
Black/White Differentials and Returns to School Quality

Variable	Ordinary Least Squares							School Fixed Effects
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Class of 1972 in 1979:								
Black	−.048	−.050	−.049	−.051	−.052	−.031	−.037	−.040
	(.023)	(.023)	(.023)	(.023)	(.024)	(.027)	(.027)	(.031)
Pupils/teacher (× 100)		.292						
		(.149)						
Term length			.005					
			(.004)					
Advanced degrees				.038			.034	
				(.014)			(.014)	
School size (enrollment × 1,000)					.014		.011	
					(.008)		(.008)	
% black (× 10)						−.005	−.004	
						(.004)	(.005)	
B. Class of 1980 in 1986:								
Black	−.065	−.066	−.064	−.066	−.070	−.036	−.039	−.090
	(.025)	(.025)	(.025)	(.025)	(.025)	(.029)	(.029)	(.035)
Pupils/teacher (× 100)		.312						
		(.188)						
Term length			−.001					
			(.008)					
30% or more teachers have advanced degrees				.006			−.005	
				(.020)			(.020)	
School size (enrollment × 1,000)					.036		.038	
					(.013)		(.013)	
% black (× 10)						−.008	−.009	
						(.004)	(.004)	
C. Changes in black/white differentials:								
Black	−.017	−.016	−.015	−.015	−.018	−.005	−.002	−.050
	(.034)	(.034)	(.034)	(.034)	(.035)	(.040)	(.040)	(.047)

NOTE.—Standard errors are in parentheses. All regressions include college and postgraduate degree dummies, experience and experience^2, region and urban and rural dummies, family income, test scores, and high school grades.

HSB. Taken at face value, the estimates indicate that moving from an all-white school to an all-black school would result in a wage decrease of 5% among the earlier cohort, and 8% among the latter. School segregation thus appears to have important effects on wages. Boozer et al. (1992) report similar results. They also report that segregation has increased in recent years, particularly in the Northeast.

Column 7 reports the results from a less restrictive specification that includes all three school characteristics whose coefficients had the expected sign in both samples and were significant in at least one. The three school inputs are jointly significant; the F-value for the joint test is 4.13 in the NLS72 sample, and 4.10 in the HSB sample. For the most part, the coefficients from this model are similar to those from the more restrictive specifications. The one exception is the teacher education variable, which has a negative, though insignificant, coefficient in the younger cohort.

The effect of each of the school quality measures on changes in the black/white wage differential can be taken from the bottom panel of table 3 by comparing the baseline change given in column 1 with the changes given in each of the other columns. For the most part, the school characteristics have little effect. This probably is to be expected since many of the quality measures are similar by race, most of the quality coefficients are small, and there is little change in the return to school quality over time.

There is one potential exception. The results in column 6 indicate that when school segregation is accounted for, the black/white wage differential falls 1.7 percentage points in the earlier cohort, and 2.9 percentage points in the latter. As a result, the corresponding change in the wage differential falls by 1.2 percentage points. This may indicate that recent increases in school segregation have put black students at a greater disadvantage.

Alternatively, at least part of the decrease in the racial wage differential may stem from collinearity between the black dummy and the school segregation variables. These variables are highly correlated since blacks attend schools that are disproportionately black. The standard errors of the black coefficients rise when the school segregation variable is added to the model, adding weight to the collinearity argument.

Fortunately, this issue can be resolved by considering school-fixed-effects estimates of the wage equation. Since there are multiple observations per school, I can include a dummy variable for each school in equation (1). This specification allows each school to have its own effect on the post-graduation earnings of its students, subsuming all measurable characteristics, such as the level of segregation, as well as all unmeasurable characteristics. If school segregation, or any other aspect of school quality, has contributed to increases in the black/white wage differential, then the fixed-effects estimates should show a small, or even positive, change in the racial wage gap across cohorts.

Although not reported in table 3, adding the school dummies significantly increased the explanatory power of the model. The R^2 increased by 23 percentage points in the NLS72 sample, and by 37 percentage points in the HSB sample. This specification also showed that schools have important effects on students' postgraduation wages. In the NLS72 sample, the interquartile range of the school coefficients was 0.285, indicating that on average, students attending the 75th percentile school had wages roughly 29% higher than the wages of students attending the 25th percentile school. In the HSB, the interquartile range was 0.354.

Estimates of the racial wage differentials from the fixed-effects regressions are reported in column 8 of table 3. For the earlier cohort, this estimate is slightly smaller than the baseline estimate. For the latter cohort, however, the fixed-effects estimate of the racial wage gap is substantially larger than the baseline estimate. Thus the change in the racial wage gap from the fixed-effects model is substantially larger than the change in the baseline differential. Although the changes in the racial wage gap are not significantly different across specifications, the coefficients suggest that school quality had little to do with the deteriorating relative labor market position of blacks.

B. School Quality and Within-Cohort Changes in the Black/White Wage Gap

One potential criticism of the results presented so far is that they are based on samples of recent labor market entrants, for whom the returns to school quality may be particularly low. In Farber and Gibbon's (1991) model, for example, worker characteristics not directly observable to employers have little effect early in the life cycle but gain importance as the worker ages and the employer is able to observe the employee's performance. In many labor markets, high school quality probably is not readily observed by employers. School quality therefore might be unimportant at labor market entry but increase in importance over time. In other words, the weak results I have presented thus far regarding the importance of school quality may be merely an artifact of the youthful samples I have analyzed.

Juhn et al. (1991) have shown that the recent convergence slowdown affected essentially all blacks, however, and that the slowdown was similar for cohorts of different ages. Indeed, they found that both educational attainment and educational quality contributed similarly to the slowdown, independently of age. Thus the narrow age range of my samples may be less of a limitation than it initially appears.

Moreover, the design of the NLS72 allows me to test this hypothesis directly. In 1986 approximately two-thirds of the original NLS72 respondents were resurveyed. At that time the Class of 1972 was roughly 32 years old and had accumulated 10–15 years of labor market experience. Thus I

can estimate wage regressions with the 1986 data and compare the results with those from the 1979 data. The results are presented in table 4.

The first column of the table shows that between 1979 and 1986, the black/white wage differential increased within cohort by about 3 percentage points. Comparing the estimates from table 4 with those from table 3, one sees in general only small changes in the school quality coefficients, most of which are insignificant. In addition, the race coefficient from the school-fixed-effects model is almost identical to that of the younger cohort. I conclude that the weak results above regarding the importance of the school quality measures are not merely an artifact resulting from my use of youthful samples.

C. The Importance of Measurable School Characteristics

My primary conclusion from the preceding analysis is that school quality fails to explain the break in the trend toward racial wage equality.[9] A second set of findings is of comparable importance, however. My fixed-effects regressions showed that high schools significantly and substantially affect students' postgraduation wages, in excess of any effect they may have on educational attainment. The other regressions, however, show that the specific school inputs that I can measure explain little of this effect. In particular, the regressions reported in tables 3 and 4 show that class sizes and the length of the school year have no significant effect on earnings in excess of any effect they may have on educational attainment. Because these findings contrast with the recent results of Card and Krueger (1992*b*), I consider the factors that may explain our differing conclusions.

It is important to note that Card and Krueger and I analyze samples of men that are quite different in vintage. Their sample consisted of three cohorts of men born in the 1920s, 1930s, and 1940s, whereas the men in my sample were born in the mid-1950s and the early 1960s. Thus the workers in Card and Krueger's samples were educated largely during the 1930s, 1940s, and 1950s, whereas mine received their education during the 1960s and 1970s.

Given this difference in vintage, there are at least two hypotheses that may reconcile the difference in our results. One concerns statistical power. If schools have become more homogeneous over time, so that variation in quality has lessened, then the apparent insignificance of the traditional quality measures may stem from the decrease in precision with which it is possible to measure their effects. The second hypothesis involves diminishing returns. Between 1930 and 1954, the pupil-teacher ratio fell

[9] One qualification to these conclusions may be warranted. I observe in the NLS72 and HSB only the quality of the student's high school. If primary school quality has important wage effects, then my conclusions could be overstated. This problem will be small, however, if most students obtain their education in a single district and expenditures across school levels are highly correlated within districts.

Table 4
Black/White Differentials and Returns to School Quality: Class of 1972 in 1986

Variable	Ordinary Least Squares							School Fixed Effects
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Black	−.079	−.078	−.080	−.075	−.088	−.056	−.061	−.089
	(.033)	(.033)	(.034)	(.034)	(.033)	(.039)	(.039)	(.049)
Pupils/teacher (× 100)		.328						
		(.193)						
Term length			.002					
			(.005)					
30% or more teachers have advanced degrees				.042			.028	
				(.017)			(.018)	
School size (enrollment × 1,000)					.039		.037	
					(.009)		(.010)	
% black (× 10)						−.006	−.006	
						(.005)	(.005)	

NOTE.—Standard errors are in parentheses. All regressions include college and postgraduate degree dummies, experience and experience^2, region and urban and rural dummies, family income, test scores, and high school grades.

from 30.1 to 27.7 (U.S. Bureau of the Census 1957). Between 1955 and 1980, average class sizes fell further from 27.4 to 18.6 students (National Center for Education Statistics 1992*a*). If educational production functions exhibit diminishing marginal productivity, then reductions in class sizes today would have less effect than similar reductions in the past. I consider these explanations in turn.

Data from table 2 suggest that the low power hypothesis could explain my results regarding term length since its standard deviation is only one. The argument that low power explains my results regarding the pupil/teacher ratio is less compelling, however. The basic unit at which my school quality data are observed is the school. Calculated on a school basis, the standard deviation of the pupil/teacher ratio is 5.8 in the NLS72. Card and Krueger's basic unit of observation is the state, for which they constructed quality measures specific to each of their three cohorts. Calculated on this basis, the standard deviation of their pupil/teacher ratio is 4.2. The greater variation in my data thus suggests that low power is not to blame for the difference in our results.

Next, data from Card and Krueger (1992*b*) provide what may be evidence on the importance of diminishing returns in educational production. In one of their preferred specifications, Card and Krueger regress state- and cohort-specific estimates of the returns to education on the average pupil/teacher ratio, cohort dummies, and a full set of state dummies, pooling data from all three cohorts. They obtain an estimate ($\times$ 100) of -9.52 (2.81), indicating that a decrease in the pupil/teacher ratio of 10 would increase students' returns to education by 0.95%.

Based on data in their tables 1 and 2, I am able to replicate their results fairly closely, obtaining an estimate of -9.63 (2.70). When I restrict the sample to the first two cohorts, however, I obtain an estimate of -12.73 (3.75). From the last two cohorts, I obtain -6.06 (5.07).[10] The estimated coefficient falls by one-half. Although the change is insignificant, so too is the estimate from the more recent cohorts. This suggests that the pupil/teacher ratio may have been less important for men educated during the 1950s than it was for men who received their schooling during the 1930s.

A final hypothesis that may explain the difference in our results, however, concerns measurement error. Card and Krueger (1994) have argued that state-level school quality measures may contain less measurement error than school-level measures. If so, then my estimates of the effect of school quality may be biased downward.

Fortunately, this hypothesis may be tested directly. If measurement error is driving my results, then instrumental variables (IV) regressions, using state-level quality measures as instruments, should provide consistent estimates of the effect of school quality. Because published state-level data

[10] It is necessary to use two cohorts in order to include the state dummies, which are highly significant. The 1930s cohort is common to both subsamples.

Table 5
Instrumental Variable Estimates of Black/White Differentials and Returns to School Quality: Class of 1980 in 1986

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Black	−.065	−.064	−.063	−.067	−.066	−.059	−.061
	(.025)	(.025)	(.025)	(.025)	(.025)	(.025)	(.026)
Pupils/teacher (× 100)		−.276					
		(.387)					
Term length			−.005				
			(.005)				
30% or more teachers have advanced degrees				.022			.028
				(.082)			(.116)
School size (enrollment × 1,000)					.008		.013
					(.036)		(.047)
% black (× 10)						−.006	−.008
						(.008)	(.009)

NOTE.—Standard errors are in parentheses. All regressions include college and postgraduate degree dummies, experience and experience^2, region and urban and rural dummies, family income, test scores, and high school grades. Instruments are state-level measures of school size, teacher education, percent of black students, pupils/teacher, and term length.

on teacher education are available only beginning in the mid-1980s, a set of instruments for all of my school quality variables is available only for the HSB cohort. Results are reported in table 5 and should be compared with the estimates in the second panel of table 3. In all cases, instruments for the school-level variables are published state-level aggregates.[11]

None of the IV estimates are significant, either jointly or individually. The class size variable has a negative coefficient, but it is smaller than its standard error. Indeed nearly all the IV estimates have large standard errors, even though the R^2's for the first-stage regressions used to form the instruments ranged from 15% to 25%.

In general, the IV estimates make no more compelling a case for the importance of these particular school characteristics than their OLS counterparts. I conclude that measurement error does not explain the differences between my results and those of Card and Krueger. This conclusion reinforces the notion that the effect of school inputs on wages has varied across cohorts. Given the importance of this issue for educational policy, a more detailed analysis of the differences across cohorts would be a useful endeavor for future research.

V. Conclusions

The point of this article is to ask how much of the recent break in the trend toward convergence between black and white wages can be explained

[11] The states in which the HSB schools are located are not directly identified on the public use tapes. They can be inferred, however, from data on the postsecondary institutions attended by students. The data appendix provides details, as well as sources for the state-level quality measures.

by measures of educational quality. My direct approach gives answers quite different from the indirect approach of Juhn et al. (1991). Whereas they attribute much of the convergence slowdown to changes in the returns to educational quality, I find that such changes account for little or none of the recent widening of the black/white wage differential. There are two main reasons for this: (1) By many measures, the quality of schools attended by blacks in the 1970s was nearly equal to the quality of schools attended by whites; and (2) measurable aspects of school quality generally were not highly valued in the labor market.

My analysis also yields a second set of findings that are of comparable importance. School fixed-effects regressions show that high schools have significant and important effects on students' wages, well in excess of any effect they may have on students' educational attainment. Yet characteristics of schools typically thought to measure educational quality—such as class size and the length of the school year—do not explain these effects.

It would be desirable to resolve the differences between my conclusions and those of Card and Krueger, who reported that class size in particular is an important determinant of wages. One way to reconcile our findings would be to test the diminishing returns hypothesis directly by using a sample that included both older and newer vintages of workers. An even more important task for future research, however, is to identify which educational inputs matter today.

Data Appendix

This appendix details how I constructed the variables used in the analysis. Capital letters denote variable names from the NLS72 and HSB documentation. See National Center for Education Statistics (1981) and Center for Education Statistics (1987).

Wages and Employment

Earnings data were reported differently in the two surveys, and so construction of the hourly wage variables differed for each cohort. In the NLS72, I constructed hourly wages from earnings and hours worked on jobs held over the year prior to the October 1979 interview. The hourly wage was defined as the average weekly wage (FT24) divided by the average hours worked per week (FT22). In the HSB, earnings and hours data pertain to the job held on the February 1, 1986, interview date. I constructed employment spells for all jobs held between the 1984 and 1986 interviews from job beginning and ending dates: TE8E1BM–TE8E4BM, TE9E1BM–TE9E4BM, TE10E1BM–TE10E4BM, TE11E1BM–TE11E4BM, TE8E1BY–TE8E4BY, TE9E1BY–TE9E4BY, TE10E1BY–TE10E4BY, TE11E1BY–TE11E4BY, TE8E1EM–TE8E4EM, TE9E1EM–TE9E4EM, TE10E1EM–TE10E4EM, TE11E1EM–TE11E4EM, TE8E1EY–TE8E4EY, TE9E1EY–TE9E4EY, TE10E1EY–TE10E4EY, TE11E1EY–TE11E4EY. If the respondent was employed on the 1986

interview date, I determined his weekly hours (TE8I, TE9I, TE10I, and TE11I) and recorded him as either a full-time (hours > 30) or part-time worker. His hourly wage on his February 1986 job was then calculated from information on his pay rate and schedule (e.g., $4/hour, or $1,200/month from TE8HA–TE11HA, TE8HB–TE11HB, and TE8I–TE11I) and his usual weekly hours, if necessary (e.g., for workers paid monthly, I calculated hourly wages as monthly pay/monthly hours, where monthly hours was set at 4.4 × usual weekly hours). If the respondent had a full-time and a part-time job at that date, I used wage data from the full-time job. If the respondent held two full-time jobs, I used wage data from the job most recently begun.

Educational Attainment

The NLS72 composite variable EDATT was used to obtain the respondent's educational attainment as of the 1979 interview. A value of EDATT = 7 denotes that the respondent completed a 4- or 5-year college degree. When EDATT = 8 the respondent was coded as having completed an advanced degree. The HSB composite variable EDATTAIN was used to construct educational attainment dummies for that cohort. A respondent is coded as a college graduate for EDATTAIN = 5, and coded as having a postgraduate degree for EDATTAIN = 6 or 7.

School Quality

The school input measures were taken from the school files of the respective surveys. The variable SCHCODE identifies schools in the NLS72 and allows student and school records to be linked; SCHLID plays the same role in the HSB. Most of the school quality variables were either taken directly from the school file or involved only obvious transformations of a single data item. Enrollment was contained in SCHQ020 in the NLS72 and SB002A in the HSB. The corresponding data items for term length were SCHQ05 and AB005; for teacher education, SCHQ30 and SB042; and for percent black, SCHQ18A and SB0094S. The pupil/teacher ratio was computed from enrollment and the number of teachers (SCHQ27 and SB039C). The HSB-specific per-pupil expenditure items and base teacher salary came from SB053A, SB053B, and SB047.

Student Ability

The achievement tests administered to each cohort explicitly were designed to be similar across surveys to facilitate cross-cohort comparisons; in fact, large subsets of the achievement tests are identical (Hilton 1992). I used the subsets of the tests that give the closest comparability across cohorts. Descriptions from the NLS72 and HSB documentation are as follows:

Math (FORSC-MT in NLS72 and EBMTH1FS in HSB).—Students were asked to determine which of two quantities were greater, whether

they were equal, or whether there were insufficient data to answer the question.

Vocabulary (VOCABSC in NLS72 and EBVOC1FS in HSB).—Questions consisted of one word followed by a series of possible synonyms. The test taker selected one word or phrase whose meaning was closest to that of the stem.

Perception (MOSCOM-1 in NLS72 and EBMOS1FS and EBMOS2FS in HSB).—This test was used as a highly speeded measure of perceptual speed and accuracy. The respondent compared a series of tilelike shapes and patterns to detect the location of small differences in the designs.

I also constructed a variable to account for respondents whose test scores were missing. This variable equaled one if a test score was missing and zero if not missing. High school grades were taken from a self-reported question about the course grades the respondent usually received (BQ5 in NLS72 and HSGRADES in HSB).

Family Background

Family income takes the form of categorical responses to questions about the yearly family income of the students' parents in the base year (BQ93 in the NLS72, and FAMINC in the HSB). From these I created dummy variables. The income categories differ between the cohorts. The NLS72 family income dummies correspond to the following income levels in \$1972: (1) less than 3,000 a year; (2) 3,000–5,999; (3) 6,000–7,499; (4) 7,500–8,999; (5) 9,000–10,499; (6) 10,500–11,999; (7) 12,000–13,499; (8) 13,500–14,999; (9) 15,000–18,000; (10) over 18,000; (11) income missing. The HSB family income dummies in 1980 dollars are (1) less than 7,000; (2) 7,000–11,000; (3) 12,000–15,999; (4) 16,000–19,999; (5) 20,000–24,999; (6) 25,000–37,999; (7) over 38,000; (8) income missing.

Parental education was taken from CFAED and CMOED in the NLS72, and BB039 and BB042 in the HSB. From these I created two dummy variables, equal to one if the father or mother, respectively, was a college graduate.

Another set of dummies was created for the father's occupation. The categories differ slightly across cohorts. The categories common to both surveys are "clerical," "craftsman," "farmer," "laborer," "manager," "military," "operative," "professional," "protective service," "sales," "school teacher," "service," and "information missing." The HSB has the additional categories of "proprietor," "technical," "none," and "don't know." The variables used to create the occupation indicators are CFAOCP in the NLS72 and BB038 in the HSB.

Demographic Indicators

Separate dummy variables for race were created for the categories of black, Hispanic, and other race (not including white). These categories are mutually exclusive, with white as the base group. Each survey provides a race variable, CRACE in the NLS72 and RACE in the HSB, which I used to construct the individual race dummies.

Enrollment Status

School enrollment status was determined in a manner similar to the employment status. For the NLS72, part-time and full-time enrollment were determined from an activity state variable (ACT379) that gives enrollment status as of October 1979. In the HSB, enrollment status is determined according to whether the respondent was attending school on the February 1, 1986, interview date. I constructed enrollment spells for the schools attended most recently prior to the 1986 interview from school beginning and ending dates (TE21C1BM–TE21C4BM, TE22C1BM–TE22C4BM, TE21C1BY–TE21C4BY, TE22C1BY–TE22C4BY, TE21C1EM–TE21C4EM, TE22C1EM–TE22C4EM, TE21C1EY–TE21C4EY, TE22C1EY–TE22C4EY). For those enrolled on the 1986 interview date, I determined enrollment status by using a variable that records whether the students were enrolled full-time or part-time (TE21E or TE22E).

Labor Market Experience

Potential experience was calculated by determining the number of years between the interview date and the date the student last attended school full-time. In the NLS72, the last enrollment date was found using a series of activity state variables that indicate whether the respondent was enrolled full-time during October for the years 1972–79 (ACT372, ACT373, ACT374, ACT375, ACT376, ACT377, ACT378, ACT379). The student was assumed to finish school during the spring following the last October that he was enrolled full-time. For those whose education stopped at high school, the finish date was set at June 1, 1972. Potential experience was then calculated as the number of years between the October 1979 interview date and the date the respondent finished full-time school.

To calculate potential experience in the HSB, I first computed ending dates for all enrollment spells since high school completion (FE33D1M–FE33D5M, FE33D1Y–FE33D5Y, SE18DM–SE20DM, SE18DY–SE20DY, TE21C1EM–TE21C4EM, TE22C1EM–TE22C4EM, FE33E1–FE33E5, SE18F–SE20F, TE21E, and TE22E). I then used the ending date from the most recent spell of full-time enrollment. Individuals who never attended school after high school graduation were assigned June 1, 1980, as the full-time school completion date. Potential experience was then taken to be the number of years between the February 1, 1986, interview data and the date the respondent finished school.

Instruments

Sources.—State-level data on school segregation and the number of secondary schools are from National Center for Education Statistics (1982). Term length data are from National Center for Education Statistics (1984), data on pupils/teacher and the number of secondary schools are from National Center for Education Statistics (1992*b*), and teacher education data are from National Center for Education Statistics (1992*a*). Except for term lengths and teacher education, all variables

pertain to the 1979–80 school year. Term length data are from 1980–81, and teacher education are from 1987–88. In both cases, these represent the best temporal matches available.

Identifying states in the HSB.—The HSB does not directly provide data on the state in which each high school is located, but it does provide the names and locations of all postsecondary educational institutions attended by each respondent. Within each high school I tabulated across all respondents the states in which each postsecondary school was located, using data from all waves of both the senior and sophomore cohorts of the HSB. I then assigned to each high school the modal state among all postsecondary schools. In almost all cases, there was a single and distinct mode. Hanushek and Taylor (1990) and Ganderton (1992) use similar procedures.

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