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7 | COMMENTS

Orley Ashenfelter

Princeton University

The Smith-Welch paper represents an enormous amount of data collection and analysis. The results are clearly and faithfully reported, even when they do not seem to support the (perhaps vested) interests (of at least one) of the authors. I cannot imagine a different organization of the data that would be likely to change the results appreciably. In consequence, those who are interested in the subject of racial differences in economic success owe a considerable debt to the authors of this paper. Nevertheless, there is an important sense in which I believe the paper is largely a failure.

Surely the most important part of the paper is the attempt in the second section to provide an accounting, or explanation, of why the ratio of black to white male earnings increased over the period 1959 to 1969. In principle, this could be done very successfully if we could first verify empirically that the effect

of a unit of a characteristic (such as schooling) on earnings was the same for both black and white groups in both 1960 and 1970. Changes in the average characteristics of the black and white populations could then provide an unambiguous accounting of the sources of relative earnings growth, with any residual decrease in discrimination affecting the constant terms in the regressions. As it turns out, the effects of the characteristics of workers on their earnings are neither the same by race group or over time. This introduces considerable ambiguity into the accounting process. As can be seen from Table 6, for example, the accounting process based on observed characteristics produces predictions of declines in the relative incomes of blacks in the three highest experience classes, but increases in the three lower experience classes. Since there were actually increases in the relative earnings of blacks in all experience classes, objective characteristics are supposed to have helped boost the relative earnings of younger blacks but to have kept relative earnings from going up even faster among older blacks. And this is true even though the single most important objective characteristic, the schooling gap, was narrowing in each experience class. The upshot is that the "residual," or component of growth in the relative earnings of blacks not accounted for by objective characteristics, is large and almost identical in every experience class. Thus, we have failed to "explain" the cause of the increase in the relative earnings of black males.

It is natural to attribute this large residual to a general decline in labor market discrimination during the 1960s. Before doing so, however, it would be useful to have at least some direct evidence that nondiscrimination was a factor of importance. One way to do this is to search for larger increases in the relative earnings of black workers in the sectors of the economy where it might plausibly be argued that antidiscrimination forces would be greater. The authors have tried to do just this, though, as they admit, the Census of Population provides only very crude data for this purpose. In particular, they have examined the change in the relative earnings of black male workers in sectors which they estimate to have been affected directly or indirectly by the government, on the presumption that government may have been an independent force in the reduction of labor market discrimination. There are now several studies using microeconomic data of a different kind that find, as do the authors, little impact that can be attributed directly to government action.¹

Where does this leave us? In my view, it leaves us with a very considerable puzzle. To amplify this, Table 1 contains annual data on the relative wage and salary earnings of black men and women through the most recent year available. These data from the Current Population Survey reports are for year-round, full-time workers and thus provide some control, though not as much as would be desirable, on cyclical changes in annual working hours. These aggregate data are not entirely comparable to the aggregates that Smith and Welch use, but they do suggest to me an increase in the relative earnings of black males sometime during the 1960s comparable to the magnitude that Smith and Welch observe. These data also suggest to me that the relative earnings of black males were very stable until around the mid-1960s and that most of the increase that did take place may have been completed by the early 1970s, though it will take

TABLE 1 Ratios of Nonwhite to White Median Wage and Salary Earnings for Year-Round, Full-Time Workers,^a 1955-73

	Men	Women
1955	.64	.57
1956	.62	.55
1957	.63	.60
1958	.65	.62
1959	.61	.66
1960	.67	.70
1961	.66	.67
1962	.63	.63
1963	.65	.64
1964	.66	.69
1965	.64	.71
1966	.63	.71
1967	.68	.77
1968	.69	.78
1969	.69	.82
1970	.70	.85
1971	.71	.90
1972	.70	.87
1973	.72	.88

SOURCE: Various reports from the *Current Population Surveys*. For details see Ashenfelter [1].

^aA year-round, full-time worker is defined as a worker who worked 50-52 weeks per year and 35 or more hours per week.

considerably more experience before this can be confirmed. Perhaps more important, the relative earnings data for black women suggest that there will be no easy resolution of the puzzle. For these data show continuous upward progress in the relative earnings of black women until the early 1970s. Thus, attributing the increase in the relative earnings of black males to post-1964 Civil Rights activities requires an explanation for the considerable steady progress of the relative earnings of black women in the period before 1964. On the other hand, attributing the steady increase in the relative earnings of black women to the gradual relative increase in their skills (as measured, say, by schooling) requires an explanation of why the gradual relative increase in the skills (as measured again, say, by schooling) of black men had so little effect on their relative earnings before the mid-1960s.

In sum, this subject clearly will require considerably more effort before we have a clear empirical picture based on a convincing causal foundation. Smith and Welch have helped to provide a part of this foundation.

NOTE

1. See Ashenfelter and Heckman [2] and Goldstein and Smith [3].

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Alvin Mickens

New York University

The paper by Finis Welch and James P. Smith represents an impressive effort in manipulating and analyzing data from the latest decennial Census. The study will surely be of great value in providing new angles of vision for analyzing the relative progress of blacks and will add fresh material to the growing controversy over the extent and permanence of the apparent improvement in the relative economic status of blacks by experience cohorts.

The Smith and Welch findings of black experience-cohort gains relative to whites' parallel Richard Freeman's results of black progress (Brookings Papers on Economic Activity, 1: 1973) analyzed from the age-cohort approach. Both find that there have been remarkable gains for black male and female workers, when measured by the more recent age/experience cohorts. Both the Freeman study and the Smith-Welch paper reach conclusions which challenge the conventional view that there has been little improvement in black economic status relative to whites in terms of relative incomes and life-cycle labor market activity, especially when cyclical factors are carefully accounted for. I do not so much want to carry a brief for the standard view as to cite some critical limitations to which, I believe, the Smith-Welch paper fails to give adequate consideration. These factors, in my view, mainly center around black interregional migration, southern black progress, and overall economic activity.

First, recent studies, including a paper by Wayne Vroman, have shown that the gains in relative income for black males, even in the prosperous decade of the 1960s, were pretty much confined to the South and were barely evident in the Northeast and North Central regions. Smith and Welch, too, note that the third most significant argument "explaining" a major part of the relative earnings gain is black migration out of the South and the improvement in earnings of resident southern blacks. Since the major strides in educational improvements occurred

mainly among resident southern blacks, I suspect that a certain amount of intercorrelation here captured some of the importance of the southern-resident and regional variables the authors employ. In fact, Smith and Welch do show that estimated gains by blacks were reinforced by interregional and metropolitan migration of the black population.

When Smith and Welch consider the black/white cohort earnings ratios shown in Tables 3A and 3B, they do not, of course, adequately account for the peculiar effects of the southern region. Nor do they properly caution that 1969 was an incomparably better year than 1959 for overall economic activity. Moreover, the prolonged expansion from 1961 to 1969 exceeded any experienced in the postwar period and contrasts dramatically to the stop-and-go, generally stagnant 1950s. Thus, 1960s labor market entrants could surely be expected to make spectacular gains relative to 1950s labor market entrants.

It has been repeatedly documented that (1) the status of black workers improves relative to whites during expansions, and declines relative to whites during contractions, and (2) recent evidence suggests that occupational mobility and earnings/educational differentials, however measured, are highly elastic with respect to cyclical activity. In this connection, one might also mention that the speed and duration of expansion also affect black employment opportunities.

In spite of the strong emphasis attributed to education and schooling quality as accounting for the narrowing of the black-white differentials for the 1960s labor market entrants, Smith and Welch were not able to isolate fully the pervasive effects of the prolonged labor market tightness experienced in that decade from the operation of other determinants purporting to measure separately cohort life-cycle effects, although their employment variables do adjust for some of the expansionist impact.

The specter that haunts these cross-sectional approaches, which are "snapshots" of cohort gains at points in time, and which are in sharp contrast to the minimal relative progress of blacks documented in time series, is a reconciliation problem perhaps similar to that of time series versus cross-section surveys in aggregate consumption studies.

Since the relative gains for the black earnings profiles in 1970 seemed to have been registered so widely for practically all the experience cohorts, one might seek out an argument more compelling than schooling quality or "vintage": and that is the very distinct possibility that the 1960s expansion, like the 1940-48 period, achieved a dramatic one-time gain in black-white earnings ratios, and overall probably had a more equalizing impact than previous expansions.

This contention also applies to the occupational-industrial analysis in the paper. While the 1960s black entrants to the labor force may be approaching greater occupational congruency with whites, available data from another angle of vision using an occupational dissimilarity index approach show that the concentration of black workers in just three occupations (operatives, laborers, and private households) still accounts for over 40 percent of the black labor force, with little overall change since 1957. Less than 21 percent of white workers are concentrated in these jobs.

I suggest, moreover, that the pattern of gains in earnings for various experience cohorts may not square uniformly with the Welch-Smith interpretation of major gains for the latest experience cohorts when we compare cohort gains within occupations.

For example, in professional groups for both sexes, the gains in relative incomes are shown to be at least as large for many of the 1950s entrants as they were for the 1960s entrants.

If our major argument is that education improvement, in isolation from cyclical factors, accounts for the gain in relative incomes for more recent black experience cohorts, we might look at black progress in occupations ranging from ones considered more sensitive to cyclical factors to those considered less sensitive. A category I regard as "less sensitive" is the professional-technical group. My own estimates indicate that the black proportion of total employment in this group increased more during 1957-63 than during 1963-69, and the same was nearly true for managers and proprietors.

Finally, I want to note some areas to which the cross-sectional approach employed by Smith and Welch gives inadequate attention. One is the much higher unemployment of black high school graduates relative to white high school dropouts. In every year for which data are available, some 12 to 18 black graduates are jobless for every 10 white school dropouts. No such pattern exists between white graduates and dropouts. Second, labor force participation rates for black males have been experiencing a dramatic decline, a trend that shows up even for men under 45. If anything, this trend accelerated in the latter 1960s (the reference point where the most recent experience cohort of blacks show dramatic relative gains). Also, if we look at those occupations in which the bulk of the black labor force is crowded—operatives, laborers, household workers, and sales-clerical—we find that real earnings have held constant since 1967.

How are we to reconcile the hopeful "light at the end of the tunnel" emerging from cross-sectional studies of this type with the persistent gloomy results revealed in the dark tunnel of time-series studies? Perhaps longitudinal approaches made possible from Continuous Work History Samples of the Social Security Administration—once this file overcomes problems of incomplete coverage and can account for utilization continuity—may offer the way for overcoming the dissonance of opposite signs portrayed in relative black economic progress emerging from time-series and cross-sectional data.

In the meantime, I feel that more promising avenues for future research in these areas point in the direction of analysis of labor market segmentation and barriers to mobility among submarkets.

By using an index of occupational congruency Welch and Smith manage to bias upward the estimated improvement in the distribution of blacks, since they compare unweighted flows of recent black job market entrants to "stocks" of the distribution of occupationally classified whites. Such an index could be expected to exhibit more "congruency" if the (most recent) distributional flows of blacks into occupations are considered better educated and trained than previous whites—or blacks—already classified in existing jobs in 1960.

If they had compared the flows (1960-70) of new black occupational entrants to flows of new white occupational entrants for the same period and then asked:

How long will it take for blacks in a given occupation to approach parity with whites? I surmise that the answer that would emerge would put the estimate far enough into the distant long run so that, as Keynes might say, we might all be dead!

A more appropriate index for gauging this kind of progress would be one of occupational dissimilarity. Ron Oaxaca's paper, using such an index, showed no improvements when occupational comparisons of black-white, male-female were made over the years 1958-71.

I trust that we do not have to wait for 1980 or 1984, to determine whether or not the gains that seem to impress the authors, are real.

I am convinced that if the 1970s repeat the 1950s pattern of "stagflation" and low overall economic growth, many of the touted gains registered for the 1970 cohorts will probably erode and the black-white earnings differential can be expected to remain relatively constant.