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PREFACE

This document consists of comments by the author on two papers presented to the American Economic Association Meeting, December 1979. The two papers were "Impacts of Discrimination on Black-White Earning Differentials, 1960-1970" by Doug K. Jeong, and "Changes in Black-White Inequality, 1968-1978: A Decade of Progress" by William Darity and Samuel Meyers. These comments were published in the Summer 1980 issue of the Review of Black Political Economy.

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COMMENTS ON PAPERS BY PROFESSORS JEONG, DARITY & MEYERS

Professor Jeong's Paper

Dong Jeong's paper is an empirical investigation of black-white male earnings differentials using the 1960 and 1970 United States Censuses. The main characteristic distinguishing it from numerous other studies of race differences in earnings is that, in addition to the standard earnings function, two additional equations are estimated. The first deals with the level of schooling acquired and the second relates to labor supply (annual hours worked). The three structural equations are estimated separately by race using two-stage-least-squares. Finally, decomposition of the earnings function is performed in an attempt to attribute racial wage differentials to different levels of characteristics that produce earnings and to different estimated returns to a given set of characteristics (the latter are (mis)labeled market discrimination). Given the empirical nature of the paper, the most straightforward method of commenting on it is to discuss, in turn, the three empirical equations.

The most important of the three is the structural earnings function. While, in large part, Jeong's earnings function contains the standard list of variable we are accustomed to expect, it deviates from conventional practice in two respects. Unfortunately, I believe that both exceptions get Jeong into trouble and invite considerable confusion.

The first departure from convention is that in place of the log-income-schooling functional form popularized by Mincer, Jeong estimates arithmetic income as a linear function of schooling and other variables. While there is nothing sacrosanct about Mincer's functional form and there may well be alternatives that are superior to it, Jeong's estimates should establish once and for all that this is not a viable alternative. If there is anything we have learned from empirical research on the schooling-income nexus, it is that the effect of schooling on income is highly nonlinear. Larger absolute differences in income per year of schooling appear at higher

levels of schooling. The consequences for the researcher who ignores this truth is aptly demonstrated by Jeong's empirical results. His education coefficients can best be viewed as a linear approximation to the slope of a nonlinear function evaluated at the mean education level in his sample. If the true income-schooling relationship were stable across demographic groups and years, then we would expect, using Jeong's specification, to estimate larger slopes and smaller intercepts the greater the average schooling level of the sample. And this apparently is precisely what happened. Schooling coefficients are estimated as higher for whites than blacks and larger, for both races, in 1970 than they were in 1960. Similarly, intercepts are negative (a good indication of how seriously this function is misspecified), larger in absolute value for whites than blacks, and more negative for both races in 1970. This misspecification leads to serious problems later on in the decomposition of the earnings functions into factors labeled discrimination and those attributed to different levels of characteristics.

The second empirical truth we have learned from research on earning functions is that the effect of experience on earnings is also nonlinear (and not linear as assumed by Jeong). This leads to the second departure from convention which causes some confusion in the interpretation of the age and experience variables. If taken literally, the structural estimates imply that the young get paid more (holding job experience constant--experience itself increases earnings). Exactly the opposite conclusion emerges from the reduced form where youth detracts from earnings (but now job experience reduces market earnings). While there is the obvious potential for collinearity with schooling, age, and experience (i.e., age-schooling) included as regressors, the answer to these puzzling empirical findings is that the age dummies in essence allow the effect of experience to be nonlinear. Take, for example, the 1970 white male estimates. If we combine the age and experience coefficients, we trace out the following increments in earnings per year of additional experience as we move between successive intervals of the age dummies--\$254, \$187, \$60, \$15, -\$198. Thus, the data is forcing the combined impact of age and experience to trace out the familiar quadratic in experience. A similar exercise on the other structural equations or the reduced forms produce

essentially the same pattern. Thus, we should not be puzzled, as Professor Jeong apparently is, by the signs of the age or experience variables or the manner in which they switch signs between the structural and reduced form systems. It is only the combined effect of age and experience that have any meaning. Clearly, a simpler specification that does not lend itself to the possible confusion in interpretation of the age and experience variables would be the more familiar experience and experience squared.

Turn next to the second equation in the system, the demand for schooling. Jeong's argument for including this equation is that schooling is an endogenous variable. It is as difficult to argue with this assertion as it is to come up with a reasonable set of exogenous variables that enable one to identify the separate earnings and schooling structural equations. Jeong is so severely limited by his data set that his solution to this empirical problem is unconvincing, at least to me. For example, in his specification, two of the principal determinants of past schooling decisions are current income and current occupational status. One can surely conjure up some ingenuous arguments for including current income and occupation in the schooling equation (for example, as an index of unobserved motivation of the individual). But the reverse causation from schooling to income and occupation is so obvious, that I find it difficult to determine what this equation really means. This problem is especially acute since current occupation is treated as exogenous. Schooling certainly is an endogenous variable, and Jeong's criticism of previous work that ignores this endogeneity has some merit. But census styled data is so limited in the information it provides on individual backgrounds, that it is not the appropriate data set to use if one insists on opening up the issue of the endogeneity of schooling. There are other data files which do contain information on a respondent's background (i.e., the economic and demographic characteristics of his parents) that are more appropriate for this problem. If one insists on using Census data, I would, at the minimum, suggest taking out the income and occupation variables and substituting information available in the Census on the characteristics of the area in which an individual was born.

The final equation in the system relates to labor supply. The main problem encountered here is empirical. The three excluded exogenous variables used to identify the supply function are experience (which is closely approximated by the included age dummies), veteran status, and property income. That these last two variables have difficulty carrying the identification load is apparent by glancing at the 1960 and 1970 white male estimates (Appendix Table 5). The 1960 coefficients often are thirty times larger than those observed in 1970. While the world might have changed a great deal over the decade, the more likely explanation is that veteran status and property income cannot, by themselves, allow for a robust identification of a structural labor supply equation.

The final section of the paper attempts to separate black-white wage differences into two components--the first due to different levels of characteristics and the second due to market discrimination. The latter is deduced from different coefficients for blacks and whites for the same characteristics. While the practice has become a standard exercise among economists, I think it is time that we stop kidding ourselves that this is a meaningful way of measuring discrimination. That is not to say that this statistical exercise is not useful. It does tell us the relative importance of different variables in "accounting" for black-white differences. This is a useful and important first step in our search for why blacks and whites differ in market outcomes. For example, if differences in returns to schooling account for a large fraction of observed wage differences (relative to other variables), it is a signal to direct our thinking and research efforts towards understanding why this coefficient in particular differs by race. But to think measuring discrimination is so simple a task as finding coefficient discrepancies in an earning function avoids the real and complex issues of why market outcomes differ. With only the earnings function as our guide, we are at best measuring our ignorance and not the market's.

Professors Darity & Meyers Paper

The basic argument presented in this paper is that, in contrast to the experience of the 1960s and in spite of the evidence presented by other researchers, the decade of the seventies was a period of little or no reduction in the wage differences between blacks and whites. In the process, they also attempt to refute a particular explanation for the improving economic status of blacks, the vintage hypothesis. According to that hypothesis, which Finis Welch and I have advocated very strongly in our joint work, the key to understanding the narrowing of income differences by race is that more recent cohorts of blacks are more similar in the characteristics producing market earnings than their predecessors were. The convergence in characteristics is not difficult to document. It shows up most dramatically in years of schooling and the quality of the educational environment. This evidence in favor of converging characteristics is not disputed by Darity and Meyers. Where they differ from Welch and me is their claim that in spite of convergence in nominal characteristics, the black-white income gap has not narrowed in recent years. Since Welch and I have analyzed the same data set used in their empirical work (The Current Population Survey), and found a decided narrowing in wage differences by race, something obviously is amiss.

There are essentially three pieces of evidence that Darity-Meyers put forth to support their assault on the vintage hypothesis. The first is contained in Table 1, which lists black-white earnings ratios from 1968 through 1978. Although they are presented for three different samples, the critical distinction is between those with positive earnings and ratios defined for the potential labor force. If one looks at those with positive earnings, as most other researchers, including Welch and I, have, the pattern of improving economic status of blacks is clear. For males, black-white earnings ratios increased from .59 to .69 over this period. For women, the narrowing is even more dramatic, with the ratio rising from .76 to 1.00. Darity-Meyers argue, however, that a more appropriate comparison involves their concept of the potential labor force. This sample includes all individuals, not simply those with positive earnings in the labor force.

If the potential labor force comparison is made, the narrowing of the incomes gap is almost completely eliminated. For males, the ratio rises only slightly from .60 to .62. For females, the difference between the earlier comparison is truly astonishing: the earnings ratio now rises only from .92 to 1.00. The story that can be told clearly differs drastically between these samples. Which, then, is the appropriate one to use and why do the results differ so much between them?

The reason the two samples differ is clear and argues strongly against using the potential labor force sample. The difference between the samples is that the potential labor force includes those who are not workers or members of the labor force and hence have zero market earnings. Since labor force participation rates have fallen for black males (relative to white males) over this period, we are simply including a larger fraction of black males with zero income in 1978 than in 1968. This clearly will depress the rate of growth of black male earnings relative to those of whites. Similarly, participation rates of white women have risen relative to those of black women over this period. Since this makes it relatively less likely that white women will have zero income in 1978 compared to 1968, the relative growth in black female earnings is attenuated in the potential labor force sample.

Why is the potential labor force sample inappropriate? It would only be correct if individuals out of the labor force could be viewed as worthless. Darity-Meyers are assigning non-workers a zero value of time and a zero potential wage. The female comparison makes it especially obvious how inappropriate the potential labor force sample is. No one would take a study of male-female wage differences seriously if one argued that female wages were rising relative to male wages simply because female market participation is rising (so that there are fewer women with zero income). But this is exactly the same procedure used by Darity-Meyers to eliminate the relative growth in black wages. There might very well be a sample selection problem, as Heckman and Butler have argued, because of changing participation patterns by race. There are reasonable statistical procedures for dealing with problems, but assigning zero wages to non-workers is not one of them.

While the rest of the evidence used to refute the vintage hypothesis is suspect because of the sample used, I will briefly address it. The second piece of evidence is contained in Tables 3 and 4, which show between- and within-cohort changes in black-white earnings ratios. These tables more directly relate to the vintage hypothesis. I find it difficult to extract anything from these tables because of the large discrepancies between the story told using annual earnings ratios and weekly wages. But I cannot resist mentioning the following evidence from the weekly wage ratios. In 9 out of 12 cases contained in this table, the weekly wage ratio of the 1978 cohort in the same age interval as the 1968 cohort is larger in 1978. In 9 out of 10 cases, if we follow an age cohort from 1968 to 1978, black-white wages for that cohort are larger in 1978 than they were in 1968. If this is evidence refuting the vintage hypothesis, I, with a vested interest in the vintage hypothesis, would welcome more like it. To be sure, the story is different with annual earnings, but until the authors demonstrate why annual and weekly wages are so different, the claim that these tables refute the vintage hypothesis should be viewed with much skepticism.

The last piece of evidence, the wage regressions in Table 7, is the most difficult to comment on. In fairness to Darity-Meyers, they admit that these regressions should be viewed as preliminary. The regressions as they now stand do not invite much confidence. The estimated coefficients are drastically unstable across years and their patterns defy belief. For example, the 1968 black regression implies the following life-cycle returns to schooling for the six years considered: .72, -.02, .04, .02, .32. I don't believe that the authors want to have a test of the vintage hypothesis if it depends on such estimates. Finally, I must confess, I don't understand at all the discussion at the end of the paper on how these regressions are used to test the vintage hypothesis. First, there is a tendency to compare the size of differences in coefficients for variables measured in different units. Such comparisons are meaningless. Second, when interaction variables are included in the regression, as they are here, one must combine the main and interacted effects, appropriately weighted to

predict future wage ratios. Darity and Meyers do not do this, and hence are left with a confusing array of some variables predicting less inequality and some more.

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