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BLACK ECONOMIC PROGRESS: A CASE AGAINST THE DRAMATIC IMPROVEMENT HYPOTHESIS

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Black Economic Progress: A Case against the Dramatic Improvement Hypothesis

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ABSTRACT

A systematic reexamination is undertaken, using data from the Current Population Survey for 1968 and 1978, of the view that black Americans have achieved dramatic progress in recent years. Black and white earnings are each treated as determined by a simultaneous equation structural model that accounts for the effects of nonlabor sources of income on labor force participation and unemployment. Whatever validity the "dramatic progress" case might have is shown to hang on the content of sample selection. The problem of sample selection bias pervades the early work on this topic of Richard Freeman and James Smith and Finis Welch. The problem is at the core of the critique of their studies advanced by Richard Butler and James Heckman. The results reached here demonstrate clearly the importance of the sample chosen, because the model is estimated both with a sample drawn from the population of positive earners and a sample drawn from the population of all persons (the "potential labor force"). The differences in the point estimates are quite striking. They even pose limitations to Heckman's specific argument concerning sample selection bias. This paper ultimately provides the basis for a substantial reinterpretation of the pattern of change in relative incomes for blacks over the decade 1968-1978.

I. INTRODUCTION

The view has become widely accepted that the 1960s was a period of substantial progress for blacks in the United States--a period when blacks made rapid strides toward income parity with white Americans. The improvement is alleged to have been sustained throughout the 1970s, despite continuous inflation and intermittent recession in that decade. The greatest progress has been proclaimed for black women vis-à-vis white women. The former, it is said, have attained equality in mean earned incomes with their white female counterparts in the labor force.

There have been several major attempts to explain the gains blacks have supposedly achieved over the past fifteen years, especially in the work of Richard Freeman, James P. Smith, and Finis Welch.[1] The explanations have ranged from an appeal to the decline in labor market discrimination coupled with the advent of affirmative action to the belief that young black and white cohorts have become more similar in human capital characteristics. The former is Freeman's favorable demand shift claim for black labor; the latter is the Smith-Welch "vintage effect" claim.

Other researchers have questioned the extent of the "dramatic progress." Richard Butler and James Heckman take the stance that in general there has been no change in the gap in average productivity between blacks and whites.[2] They suggest that the apparent improvement in the status of blacks results from those blacks with the lowest levels of human capital having removed themselves from the labor market, while the "most productive" blacks have remained.

Edward Lazear, in contrast, accepts the assessment of the effectiveness of affirmative action but contends that it has a perverse effect on the potential lifetime earnings of black youths.[3] According to Lazear, employers will have an incentive, when compelled to pay equal wages for equal work, to reduce the on-the-job training they provide young black workers. This means that the present parity in earnings will erode over time, since black youths will accumulate less human capital over their lifetimes.

Robert Hill has demonstrated that the apparent convergence in individual black and white incomes is not reflected in a comparable convergence in family incomes.[4] Jerome Culp and Glenn Loury have shown that black families who achieve higher incomes are less likely to stay permanently in an upper-income bracket than whites.[5] William Darity has shown that exclusion of zero-earners from an analysis of black-white income differentials over time obscures the relative stability that racial income inequality has maintained during the seventies.[6]

It is the purpose of this paper, then, to reestimate black and white wage and salary equations using data on all individuals rather than those who were in the labor force or who had positive earnings. We construct a simultaneous equation model that explicitly incorporates the effects of labor force participation on earnings and the effect of wage and nonwage income on work decisions. The identical equations are estimated using comparable data on individuals with positive incomes. It is shown that certain conclusions--such as the effect that younger cohorts have on narrowing the wage gap--are essentially the product of sample selection bias.

II. THE MODEL

There are K sources of total income, Y. Wage and salary income or earnings, Y_1, for the j^{th} individual of the racial group R is given by

$$Y_1^{Rj} = G^R\ (LFPR^{Rj},\ \underset{\sim}{X}^{Rj}), \tag{1}$$

where $LFPR^{Rj}$ is the person's labor force participation rate and $\underset{\sim}{X}^{Rj}$ is a vector of mean values of personal characteristics and characteristics of local labor markets. The labor force participation rate, $LFPR^{Rj}$, is defined as

$$LFPR^{Rj} \equiv \frac{E^{Rj} + U^{Rj}}{52}, \tag{2}$$

where E^{Rj} is the number of weeks worked by the individual and U^{Rj} is the number of weeks the individual is unemployed. Therefore, $LFPR^{Rj}$ is the fraction of the year the person was either working or looking for work.

Weeks employed, E^{Rj}, and weeks unemployed, U^{Rj}, are specified as follows:

$$E^{Rj} = g^R\ ({Y_1}^{Rj},\ {Y_2}^{Rj},\ .\ .\ .,{Y_k}^{Rj},\ X^{Rj},\ \underset{\sim}{Z}^{Rj}) \tag{3}$$

$$U^{Rj} = h^R\ ({Y_1}^{Rj},\ {Y_2}^{Rj},\ .\ .\ .,{Y_k}^{Rj},\ X^{Rj},\ \underset{\sim}{Z}^{Rj}). \tag{4}$$

X^{Rj} is a vector of the elements of $\underset{\sim}{X}^{Rj}$ that enter the labor force participation equations, and $\underset{\sim}{Z}^{Rj}$ are the exogenous factors influencing the j^{th} person in racial group R's employment but not directly influencing his or her earnings. The (K - 1) predetermined sources of income are assumed to directly affect weeks working and weeks unemployed.

Conventional arguments from a human capital self-investment or a job search perspective could be invoked to justify the inclusion of other income in equations (3) and (4). For example, access to nonlabor sources of income reduces the opportunity cost of leisure. Correspondingly, access to nonlabor sources of income should also lead a labor-averse individual to raise his or her "reservation wage," the lowest wage at which a person will accept employment. The inheritance of wealth or a family business *a priori* has ambiguous effects. It could reduce time devoted to acquisition of wage and salary income, thereby lowering labor supply. It also could make it easier for an individual to engage in the self-investments that eventually should generate higher earnings.

The elements of $\underset{\sim}{X}^{Rj}$ are education, age, sex, marital status, region, veteran status, and mobility. The first six of these seven variables will enter into the labor force participation equations. They constitute the vector $\hat{X}^{Rj}$.

$\underset{\sim}{Z}$ is the vector of exogenous factors that influence employment. It includes real family income, household size, and number of earners in the household.

III. METHOD AND RESULTS

We created a "pseudo" panel from the annual CPS public use tapes. For 1968 and 1978 the means for the Y_i's, $\underset{\sim}{X}$'s, and $\underset{\sim}{Z}$'s were computed for each age, race, sex, and regional cohort. Because of small cohort sizes for older individuals and blacks in the West in 1968, all persons over 60 were grouped into one cohort for each region, race, and sex. Moreover, the Southwest and West cohorts were combined.

Since the analysis that follows relates to the years 1968 and 1978 only, the relevant comparison is between cohorts of age (a) in 1968 with age (a + 10) in 1978. Two caveats: (1) Cohort sizes differ across the two sample years. The size of a given age cohort will change over the years because of migration and death. (2) Different age cohorts also have varying sizes within a sample year, because during some years (e.g., immediately after World War II) the birth rate was high, whereas in others it was relatively low.

The regressions do not weight the different cohorts; each counts equally. This is desirable since we want to be able to examine whether particular cohorts have demonstrated relative improvement, regardless of whether the larger group of which the cohort is a part has improved. This is important because blacks can <u>on average</u> appear to be becoming better off when in fact no one age, sex, or regional cohort is improving.

Our 1968 and 1978 "pseudo" panels are based upon the complete CPS sample for blacks and whites 14 and over or what we call the "potential" labor force. A major reason for using <u>all</u>

individuals in the CPS samples is that by doing so we overcome Heckman's valid criticism of the early Freeman and Smith-Welch studies. Heckman pointed out that the early studies were beset by sample selection bias, since they excluded persons who had withdrawn from the labor force or had been continuously unemployed.

The exclusion of persons with only self-employment income from the existing studies also has biased them in favor of the "dramatic progress" case. There is evidence of increasing racial inequality in nonfarm and farm self-employment income--the latter probably associated with the ongoing decline in black ownership of rural land.

Our ultimate objective is to investigate the determinants of black-white cohort income changes and to test whether the data <u>for cohorts</u> are consistent with the convergence hypothesis.

The transition from the model in Section 2 to the model we finally estimate is straightforward. Instead of observation R_j representing the j^{th} <u>individual</u> in racial group R, it will represent the j^{th} <u>cohort</u> in racial group R. Since each of our cohorts is determined by sex, age, race, and region, we arrive at a total of 768 observations in 1968 and 928 observations in 1978.

We estimate separate structural equations for blacks and whites in 1968 and 1978 for the log of the real wage, for weeks worked, and for weeks unemployed. We use an instrumental variables procedure to overcome the problem of simultaneous equation bias. Our instruments are all the predetermined variables in the model. We also accommodate potential nonlinearities in the structural relations by including several interaction terms with the age variable. The results appear in Tables 1 and 2.

Table 1

Instrumental Variable Estimates of Coefficients of Black-White Employment Equations, 1968 and 1978
(t-statistics in parentheses)

Independent Variable	Weeks Worked 1968 Black	Weeks Worked 1968 White	Weeks Worked 1978 Black	Weeks Worked 1978 White	Weeks Unemployed 1968 Black	Weeks Unemployed 1968 White	Weeks Unemployed 1978 Black	Weeks Unemployed 1978 White
Constant	16.880	19.739	14.339	17.124	.909	.291	1.221	3.359
	(4.679)	(3.191)	(4.195)	(5.120)	(.670)	(.230)	(.859)	(3.267)
Real Income								
Wages and salary	.009	.002	.005	.003	-.001	-.000	.000	-.000
	(8.609)	(6.110)	(6.621)	(9.586)	(-1.265)	(-.309)	(1.255)	(-.503)
Nonfarm self-employed	.003	0.002	.005	.001	.000	.000	.001	-.000
	(4.342)	(3.064)	(4.376)	(3.254)	(.683)	(1.234)	(1.049)	(-.121)
Farm self-employed	.020	.003	.011	.001	-.001	-.001	.002	-.000
	(1.889)	(2.536)	(1.414)	(.994)	(-.326)	(-2.119)	(.710)	(-1.391)
Social Security	-.006	-.014	-.005	-.008	-.001	-.001	-.000	-.001
	(-2.129)	(-6.417)	(-3.144)	(-11.506)	(-.643)	(-1.315)	(-.155)	(-4.727)
Rent, dividends, and interest	-.003	-.001	-.000	.000	.001	.000	-.001	.000
	(-.805)	(-.726)	(-.018)	(.525)	(.572)	(.970)	(-1.082)	(.130)
Welfare and public assistance	.001	-.008	-.005	-.012	-.000	-.001	.002	.000
	(.592)	(-1.212)	(-2.892)	(-2.494)	(-.143)	(-.584)	(3.051)	(.064)
Other transfers	.003	.003	-.002	.000	.001	.000	.002	.000
	(.817)	(1.509)	(-1.454)	(.362)	(.437)	(.702)	(4.263)	(.834)
Real family income	-.002	-.008	-.001	-.001	-.000	-.001	-.001	-.000
	(-4.569)	(-3.165)	(-3.418)	(-4.317)	(-.136)	(-2.183)	(3.042)	(-2.520)
Household size	-1.176	-1.894	-1.317	-2.114	.048	.007	-.290	-.099
	(-2.692)	(-5.791)	(-2.957)	(-6.142)	(.298)	(.098)	(-1.469)	(-.929)
Number of earners	5.538	6.847	9.134	7.086	.882	.463	2.146	.654
	(4.804)	(8.088)	(9.491)	(11.000)	(2.060)	(2.685)	(5.018)	(3.275)
Education	.417	.371	.208	.340	.150	.194	-.023	.033
	(.915)	(.654)	(.768)	(1.142)	(.889)	(1.680)	(-.189)	(.901)
Veteran	-7.626	2.281	-.689	-3.728	-.666	-.767	-1.252	-.837
	(-2.811)	(1.052)	(-.326)	(-1.897)	(-.661)	(-1.736)	(-1.334)	(1.374)
Sex, female	.070	-7.934	-1.496	-5.859	-1.752	-.907	-.025	-1.606
	(.033)	(-5.629)	(-1.037)	(-6.502)	(-2.237)	(-3.156)	(-.038)	(-5.749)
Married, spouse present	-5.075	6.034	-.380	6.405	.225	-.754	-.619	-1.967
	(-2.236)	(4.471)	(-.266)	(5.979)	(.266)	(-2.739)	(-.976)	(-5.921)
Region								
Northeast	-2.106	.959	.239	-.020	.111	-.191	.155	.189
	(-2.740)	(3.263)	(.371)	(-.124)	(.387)	(-3.192)	(.542)	(2.566)
North Central	-2.527	1.228	-.091	.541	-.007	-.272	.474	-.103
	(-3.436)	(4.467)	(-.161)	(2.407)	(-.026)	(-4.867)	(1.882)	(-1.432)
Southeast	2.585	.399	.870	-.240	-.708	-.349	-.144	-.141
	(-2.270)	(1.275)	(1.478)	(-1.029)	(-2.385)	(-5.478)	(-.552)	(-1.958)
Age-education interaction								
44-53	-1.187	-.890	-.501	-1.607	.032	-.169	-.305	-.248
	(-2.270)	(-1.350)	(-1.175)	(-3.076)	(.166)	(-1.257)	(-1.610)	(-1.528)
34-43	-.948	-.309	-1.296	.799	-.154	-.289	.052	.127
	(-1.602)	(-.393)	(-2.433)	(1.322)	(-.704)	(-1.808)	(.221)	(.677)
24-33	-.875	-1.224	.289	-.346	-.253	-.275	.646	-.305
	(-1.287)	(-1.418)	(.341)	(-.502)	(-1.003)	(-1.562)	(1.712)	(-1.428)
14-23	1.608	1.900	2.043	2.800	.462	.097	.738	.698
	(2.725)	(3.198)	(4.668)	(8.644)	(2.109)	(.802)	(3.796)	(6.949)
Age-sex interaction								
44-53	4.948	1.803	2.997	2.070	-1.319	-.088	-.408	.283
	(3.504)	(2.166)	(2.480)	(3.047)	(-2.193)	(-.953)	(-.760)	(1.345)
34-43	3.040	-.613	6.946	3.803	.114	-.196	-.385	.387
	(1.781)	(-.607)	(5.274)	(3.795)	(.179)	(-.952)	(-.658)	(1.245)
24-33	4.355	-3.855	6.939	.874	.379	.071	-.557	.170
	(2.723)	(-4.305)	(4.605)	(1.008)	(.638)	(.391)	(-.972)	(.633)
14-23	.013	3.636	2.241	3.657	.718	.369	-1.116	.637
	(.007)	(2.805)	(1.368)	(3.940)	(1.046)	(1.399)	(-1.533)	(2.213)
Age								
14-23	-22.686	-27.010	-31.841	-33.998	-5.991	-1.84	-4.993	-8.773
	(-3.471)	(-4.036)	(-5.606)	(-8.330)	(-2.469)	(-1.346)	(-1.978)	(-6.931)
24-33	5.126	16.905	-5.961	6.943	2.706	3.111	-5.755	4.218
	(.737)	(1.674)	(-.581)	(.770)	(1.049)	(1.512)	(-1.262)	(1.508)
34-43	8.224	4.083	10.851	-10.426	1.238	3.65	.473	-1.649
	(1.603)	(.465)	(1.889)	(-1.397)	(.650)	(1.379)	(.186)	(-.712)
44-53	7.451	8.449	3.593	17.663	.449	1.936	3.740	2.944
	(1.756)	(1.207)	(.871)	(2.915)	(.285)	(1.357)	(2.039)	(1.567)
R^2	.8958	.986	.921	.988	.146	.548	.423	.794
Sum of squared residuals	6152.47	916.19	6582.96	943.49	848.065	38.059	1300.57	90.749
No. of observations	384	384	464	464	384	384	464	464

Table 2

Instrumental Variable Estimates of Coefficients of Black-White Wage Equations, 1968 and 1978 (t-statistics in parentheses)

Independent Variable	Ln Wage 1968 Black (1)	Ln Wage 1968 White (2)	Ln Wage 1978 Black (3)	Ln Wage 1978 White (4)	Between-Cohort Changes In Inequality (5)	Within-Cohort Changes In Inequality (6)
Constant	5.14 (18.69)	5.56 (16.07)	1.88 (4.81)	3.18 (7.36)	-.88	--
Labor force participation rate	1.96 (5.25)	2.74 (14.50)	3.82 (10.91)	4.43 (22.49)	.17	--
Education	.110 (2.82)	.056 (1.24)	.318 (6.36)	.245 (5.57)	.02	--
Veteran	.321 (.64)	-1.26 (-3.13)	.456 (.96)	-1.89 (-8.40)	.77	--
Sex, male	-.401 (-1.91)	-.451 (-3.15)	-.238 (-1.02)	-.950 (-8.48)	.66	--
Married, spouse present	.483 (3.18)	.603 (6.22)	.902 (3.54)	.628 (3.67)	.39	--
Northeast	.185 (3.43)	-.001 (-.04)	.088 (.79)	.026 (.15)	-.12	--
North Central	.212 (4.08)	-.122 (-4.36)	.023 (.20)	-.094 (-1.96)	-.22	--
Southeast	-.062 (-1.03)	-.128 (-3.76)	.054 (.44)	-.048 (-.86)	.04	--
Age-veteran interaction						
14-23	.951 (1.14)	.302 (.66)	-1.46 (-.86)	-4.41 (-4.01)	2.30	--
24-33	-.232 (-.37)	1.18 (2.42)	-1.11 (-1.21)	1.69 (3.97)	-1.39	-3.45
34-43	-.479 (-.75)	.892 (1.76)	-.349 (-.34)	1.96 (3.19)	-.94	-.90
44-53	-.177 (-.30)	1.22 (2.77)	-.607 (-.59)	2.35 (4.14)	-1.56	-1.59
Age-education interaction						
14-23	.486 (11.05)	.407 (11.31)	.263 (3.60)	.082 (2.05)	.10	--
24-33	-.054 (-.954)	.046 (.685)	-.333 (-2.74)	-.193 (-1.816)	-.04	-.22
34-43	-.021 (-.439)	-.037 (-.585)	-.276 (-2.74)	-.182 (-2.072)	-.11	.0006
44-53	-.040 (-.960)	-.033 (-.588)	-.307 (-3.64)	-.277 (-3.743)	0.02	-.04

Table 2--Continued.

Independent Variable	Ln Wage 1968 Black (1)	Ln Wage 1968 White (2)	Ln Wage 1978 Black (3)	Ln Wage 1978 White (4)	Between-Cohort Changes In Inequality (5)	Within-Cohort Changes In Inequality (6)
Age-sex interaction						
14-23	-.064 (-.307)	.072 (.511)	-.082 (-.27)	.614 (4.183)	-.56	--
24-33	.194 (.861)	.364 (1.966)	.278 (.74)	1.428 (6.704)	-.98	-1.01
34-43	-.020 (-.069)	-.014 (.056)	.560 (1.27)	1.368 (4.137)	-.80	-.64
44-53	.023 (.095)	.265 (1.487)	.409 (.817)	1.580 (3.908)	-.93	-1.17
Age						
14-23	-5.705 (-11.360)	-5.156 (-15.932)	-3.521 (-5.02)	-2.279 (-5.589)	-.69	--
24-33	.434 (.805)	-.979 (-1.267)	3.220 (1.66)	.439 (.326)	1.37	3.33
34-43	.256 (.607)	.336 (.461)	2.260 (2.127)	.328 (.268)	2.01	.52
44-53	.368 (1.048)	.027 (.049)	2.717 (3.411)	1.265 (1.281)	1.11	1.53
Mobility: Moved						
North to West	.624 (.585)	-1.21 (-.741)	1.325 (.598)	-.472 (-.263)	-.04	--
South to West	-2.333 (-1.565)	-1.646 (-1.026)	.732 (.374)	-.469 (-.169)	1.89	--
South to North	-1.176 (-1.299)	1.365 (1.239)	2.234 (1.400)	-.789 (-.446)	5.56	--
North to South	.052 (.032)	2.616 (2.851)	-.698 (-.284)	.871 (.854)	1.00	--
R^2	.924	.988	.819	.971		
Sum of squared residuals	37.744	6.004	239.62	21.525		
No. of observations	384	384	464	464		

It is worth noting that the specifications of the wage equations (Table 2) and the weeks-worked equations (Table 1) tend to perform "better" than the specification of the weeks-unemployed equations (Table 1) for both blacks and whites. This is especially interesting since the variables in the weeks-worked equations are the same as those in the weeks-unemployed equations. Those variables, based upon a framework of human capital and job search, do not seem to be as useful for explaining unemployment as they are for explaining employment. Very few of the right-hand-side variables are demonstrably statistically significant.

A careful examination of the weeks-unemployed equations in 1968 and 1978 suggests that there was a major structural shift affecting blacks, manifested in changing effects of nonwage income on unemployment. In the 1968 weeks-unemployed equation for blacks <u>none</u> of the types of income appear to be statistically significant at the 1% level. In the 1978 equation, both welfare and other transfers (including unemployment compensation) appear to be statistically significant and both bear a positive relationship to weeks unemployed.

The discrepancy between 1968 and 1978 may seem to support the Butler-Heckman view that the expansion of federal support programs has led to an increasing refusal of blacks to accept certain kinds of work. Equally, the change in the coefficients between the two years may reflect another fact entirely: 1968 was a year of economic expansion, and 1978 was a period of lower aggregate economic activity. The direction of causality implied by our model suggests that blacks have chosen to work less in 1978 owing to greater access to federal income maintenance programs. However, the absence of such a result in the 1968 equation, during a period of relatively low black joblessness, indicates that our regressions may be picking up an expansion in transfer programs associated with a growth in black unemployment that was attributable to a change in general business conditions.

The white weeks-unemployed equations do not reveal a similar structural shift, particularly with respect to the effects of nonearned incomes on unemployment. For all white cohorts in 1968, the only type of income with a significant influence on weeks unemployed was farm self-employment income. In 1978 only Social Security income had a significant influence. Both of these bear an inverse relationship with weeks unemployed. It is unclear whether those persons receiving farm self-employment income are working more weeks or are out of the labor force more frequently, but recipients of Social Security income are, quite clearly, more likely to be out of the labor force altogether.

The weeks-worked equations for blacks and whites appear more consistent with the human capital, job-search perspective. There is a significant positive relationship between the real wage and the number of weeks worked in all four equations. Access to nonfarm self-employment income also has a significant positive impact on weeks worked. A similar effect emerges for farm self-employment income, although for blacks and whites in 1978 the coefficients are not statistically significant.

Access to welfare seems to reduce weeks worked more than does access to Social Security income. It is interesting to note, however, that the effect of welfare on black weeks worked in 1968 is not significant

<u>and</u> is positive in sign. By 1978 the coefficient associated with this type of transfer takes the anticipated negative sign and is significant. But the coefficient for whites in 1978 is larger. This implies that for an additional dollar of welfare payments, whites are likely to reduce weeks worked by more than twice as much as blacks. This finding is problematical for Butler and Heckman, whose analysis hinges on the assumption that blacks have a lower opportunity cost of "leisure" and, therefore, respond more drastically to the expansion of transfer programs. In none of the equations do other transfers or income from wealth have a statistically significant influence on weeks worked. But our results suggest Butler and Heckman are wrong in presuming that blacks are disproportionately affected by sample selection bias.

Current family characteristics appear to have prominent effects on weeks worked: the larger the family income and household size, the lower the number of weeks worked, and an increase in the number of earners in the household <u>raises</u> them. Education has no statistically significant effects on the labor force participation equations, both for weeks worked and weeks unemployed.

For blacks, there is no evidence of sexual differences in weeks worked, but for whites the differences are dramatic. According to our results, in 1968 to be white and female meant to work almost eight weeks less than a white male; in 1978 it meant working nearly six weeks less than a white male. Marriage appears to lower weeks worked for blacks, although the 1978 coefficient is not statistically significant--while marriage clearly raises weeks worked for whites. Similarly, veteran status lowers weeks worked for blacks in 1968 without exhibiting a significant effect in 1978.

Being in the youngest age cohort precipitiously lowers weeks worked, weeks unemployed, and earnings, although the magnitude of the effects are different for whites and blacks. While being in the youngest age cohort lowers weeks worked for whites more so than for blacks in both 1968 and 1978, it lowers weeks unemployed for whites to a greater extent than for blacks only in 1978. In addition, black wages are lower by a greater amount than white wages are for the 14-23 age group. These differences are crucial to an underestimating of the importance of younger "vintages" of workers for overall changes in black-white income inequality. We concentrate, below, then on the wage equations in Table 2.

The results of Table 2 can be used descriptively to examine the sources of changes in black-white wage income inequality between 1968 and 1978. Note that the more dramatic the rise in black income, the greater will be the ratio of black-white wages in 1978 to relative wages in 1968. This ratio, given by $(T^B_{78}/Y^W_{78})/(Y^B_{68}/Y^W_{68})$, can be used to measure changes in inequality. As inequality declines (black wage incomes grow more rapidly relative to white incomes), this ratio increases, as does its natural logarithm.

The effect on relative income growth (or reduction in income inequality) of increases in some independent variable, say X_i, is found by differentiation:

$$\frac{\partial \ln [(Y^B_{78}/Y^W_{78})/(Y^B_{68}/Y^W_{68})]}{\partial X_i}.$$

The sign, of course, is positive if the factor X_i leads to a reduction in income inequality; it is negative if the factor leads to an increase in inequality. Let us take one example: in 1978, the effect of increasing labor force participation on log-wages is found to be 3.82 for blacks and 4.43 for whites; in 1968, it is 1.96 for blacks, and 2.74 for whites. Hence, the effect of increasing labor force participation is to reduce income inequality between blacks and whites ($3.82 - 4.43 - 1.96 + 2.74 = .17 > 0$). This result suggests that as labor force participation rates decline, income inequality among those in the <u>potential</u> labor force tends to increase. This is consistent with the Heckman-Butler argument that reductions in the labor force participation rate can account for the observed decrease in black-white inequality among those who are in the <u>actual</u> labor force and have positive earnings.

From column 5 of Table 2, we find a number of factors that account for declines in inequality: increased education, veteran status, being married, being male, migration (both from the North to the South and from the South to the North and West) and residency in the Southeast.

Now, it is possible to explore closely the change in inequality among young cohorts. The age interaction terms permit two types of comparisons. One comparison (column 5 of Table 2) permits examination of vintage effects: it looks at a given cohort in 1968 and the equivalent cohort in 1978. The second comparison (column 6 of Table 2) permits analysis of within-cohort changes in inequality: it compares a given cohort in 1968 with that same cohort ten years later.[7]

The results for the young cohorts are striking. If we ignore interactions of age with other variables, we find that wage income for whites aged 14-23 grew faster since 1968 than it has for blacks in the same age group (-2.279 - (-5.156) = 2.877 as opposed to -3.521 - (-5.705) = 2.184). Thus, inequality appears to have increased for 14- to 23-year-olds between 1968 and 1978. Taking into account age interactions with other variables, though, reveals that the presence of 14- to 23-year-old veterans improves the relative position of blacks as does a generally higher level of education, but the presence of females in this same age group adds to income inequality. These ambiguous results suggest that the "improvement" experienced by young workers between 1968 and 1978 is very mixed, and our analysis in the next section suggests that if anything we should reject the vintage hypothesis.

Similarly, the cohort that was 14-23 years old in 1968 exhibits a very mixed picture in 1978. If we ignore age interaction effects, this cohort, which grew to be 24-33 years old in 1978, experienced a decline in inequality over the ten years since 1968. This within-cohort change in inequality is not duplicated when age interactions with veteran status, education, and sex are introduced. Within this younger cohort, being a veteran, having better education, and being female all contributed to an <u>increase</u> in black-white wage inequality between 1968 and 1978. Given these offsetting results, we cannot, <u>a priori</u>, conclude that there was a <u>net</u> reduction in racial inequality within that young birth-cohort.

Moreover, even if it could unequivocally be shown that inequality had declined for young cohorts, it is unlikely that these groups alone would account for the largest changes in black-white inequality, as proponents of the vintage hypothesis might contend. Indeed, the factor that has the largest marginal effect on inequality is found to be

Table 3

Instrumental Variable Estimates of Coefficients of Black-White Employment
Equations, 1968 and 1978: Positive Income Sample
(t-statistics in parentheses)

	Weeks Worked				Weeks Unemployed			
	1968		1978		1968		1978	
ndependent Variable	Black	White	Black	White	Black	White	Black	White
onstant	32.774	29.654	12.506	33.351	1.037	2.865	-.860	5.080
	(5.993)	(5.182)	(4.273)	(9.324)	(.475)	(1.866)	(-.524)	(4.181)
eal Income								
Wages and salary	.719	.156	.414	.180	.100	-.373	.130	-.255
	(3.328)	(1.804)	(1.969)	(3.058)	(.116)	(-1.605)	(1.100)	(-1.279)
Nonfarm self-employed	-.114	.588	.407	.251	-.687	.771	.241	-.676
	(-.381)	(.504)	(.959)	(2.258)	(-.577)	(.246)	(1.012)	(-.179)
Farm self-employed	.222	.585	-.903	-.656	.169	-.629	.107	.331
	(3.019)	(2.238)	(-.548)	(-.320)	(.577)	(-.897)	(1.160)	(.473)
Social Security	-.461	-.425	.484	-.414	-.886	-.125	.616	-.153
	(-1.170)	(-1.745)	(1.749)	(-4.500)	(-.563)	(-1.926)	(.397)	(-4.971)
Rent, dividends, and interest	-.926	-.194	-.335	.256	.194	.661	-.125	-.744
	(-1.744)	(-1.275)	(-1.328)	(.354)	(.917)	(1.613)	(-.883)	(-.302)
Welfare and public assistance	.730	.690	-.763	-.120	.219	.134	.422	
	(.182)	(.768)	(-1.855)	(-.112)	(.136)	(.556)	(1.825)	1.361
Other transfers	.263	.228	-.600	-.160	.244	-.555	.238	.719
	(.714)	(.995)	(-2.280)	(-2.054)	(1.655)	(-.902)	(1.609)	(2.707)
Real family income	-.679	.119	-.134	-.216	-.188	.714	-.908	-.457
	(-1.035)	(.301)	(-1.558)	(-.911)	(-.717)	(.673)	(-1.881)	(-.568)
Household size	-.243	-1.800	.715	-1.546	.896	-.503	.490	.172
	(-.592)	(-5.401)	(1.577)	(-3.326)	(-.546)	(-.562)	(-1.926)	(1.088)
Number of earners	-.321	-.232	4.223	.732	.656	-.274	2.422	.240
	(-.149)	-.149	(1.641)	(.630)	(.764)	(-.658)	(1.676)	(.608)
Education	-.457	.389	.631	-.110	.857	.261	-.504	.649
	(-.889)	(.739)	(1.138)	(-.398)	(.417)	(.184)	(-.162)	(.693)
Veteran	-7.762	1.092	.424	.642	-.697	.319	-2.350	-1.056
	(-2.046)	(.333)	(.128)	(.247)	(-.447)	(.363)	(-1.267)	(-1.199)
Sex	4.912	3.325	8.892	4.999	-.147	-1.190	.555	-2.054
	1.647	(2.064)	(3.070)	(4.586)	(-.123)	(-2.752)	(.341)	(-5.547)
Marital status	-3.185	6.466	5.847	4.433	.432	.146	.586	-1.620
	(-1.261)	(3.586)	(3.072)	(3.352)	(.428)	(.301)	(.548)	-3.606
egion								
Northeast	-1.457	2.010	1.922	1.444	-.763	-.314	.816	.225
	(-1.538)	(7.096)	(2.190)	(4.624)	(-.202)	(-4.133)	(1.655)	(2.125)

Table 3--continued

	Weeks Worked				Weeks Unemployed			
	1968		1978		1968		1978	
dependent Variable	Black	White	Black	White	Black	White	Black	White
North Central	-2.833	1.369	2.049	1.176	.206	-.405	.838	-.294
	(-3.081)	(5.132)	(2.185)	(3.951)	(.561)	(-5.662)	(1.592)	(-2.908)
Southeast	2.597	1.099	(2.333)	.580	-.533	-.490	-.802	-.244
	(3.013)	(3.507)	2.541	(1.856)	(-1.549)	(-5.828)	(-.155)	(-2.299)
e-Education Interaction								
44-53	-.730	-.152	-.972	-.575	-.853	-.835	-.188	-.178
	(-1.297)	(-.218)	(-1.729)	(-.785)	(-.380)	(-.446)	(-.597)	(-.717)
34-43	-.663	-.788	-.963	-.980	-.152	-.132	-.356	(.128
	(-1.077)	(-1.087)	(-1.260)	(-1.248)	(-.618)	(-.679)	(-.829)	.478)
24-33	-.100	-1.705	-.314	-.315	-.276	.846	.868	-.359
	(-.139)	(-2.361)	(-.247)	(-.381)	(-.957)	(.437)	(1.214)	(-.128)
14-23	1.410	.788	2.790	2.203	.418	.300	.504	.943
	(2.285)	(1.444)	(5.237)	(6.405)	(1.695)	(2.051)	(1.686)	(8.067)
e-Sex Interaction								
44-53	4.090	-.520	2.328	-.532	-1.546	-.215	-.179	.168
	(2.237)	(-.570)	(-1.185)	(-.483)	(-2.117)	(-.880)	(-.163)	(.448)
34-43	1.574	-2.584	-1.804	-1.340	.417	-.364	1.035	.336
	(.790)	(-2.475)	(-.947)	(-.899)	(-.527)	(-1.301)	(.967)	(.663)
24-33	-.239	-6.368	-3.991	-4.743	.184	-.230	-.124	.298
	(-.138)	(-7.361)	(-1.962)	(-4.566)	(.266)	(-.993)	(-.108)	(.843)
14-23	-3.032	-4.982	-8.241	-5.259	-.799	.364	-.908	.687
	(-1.247)	(-3.398)	(-2.900)	(-4.670)	(-.823)	(.927)	(-.569)	(1.794)
e								
44-53	3.622	1.661	10.611	6.996	1.572	1.024	2.911	2.221
	.762	(.226)	(1.867)	(.816)	(.828)	(.518)	(.912)	(.766)
34-43	4.786	9.822	8.630	13.084	1.669	1.632	4.967	-1.824
	(.885)	1.218	(.963)	(1.293)	(.770)	(.754)	(.987)	(-.530)
24-33	-1.999	21.849	2.946	6.648	3.052	-10.212	-8.230	-1.420
	(-.269)	(2.674)	(.196)	(.603)	(1.028)	(-.553)	(-.973)	(.112)
14-23	-18.919	-11.757	-38.439	-26.160	-3.397	-4.385	1.785	-12.310
	(-2.782)	(-1.986)	(-4.866)	(-5.498)	-1.250	(-2.760)	(.402)	(-7.617)
2	.767	.966	.733	.930	.117	.344	.182	.596
ım of squared residuals	7825.35	923.351	16921.8	1891.98	1248.86	66.478	5336.38	218.296
›. of observations	384	384	464	464	384	384	464	464

South-to-North migration, which led to declines in incomes for blacks in 1968 and whites in 1978. The South-to-North movement resulted in higher wages for blacks in 1978 and whites in 1968. Thus over the decade the net effect has been a relative improvement in blacks' position as individuals migrate to the North. This is a disturbing conclusion, hardly justifying the optimism of the proponents of "dramatic progress." Not only has the tide of the South-to-North migration ebbed for both blacks and whites, but the prospects for further improvement of black economic status in declining northern industrial areas is dismal.

IV. THE "POSITIVE INCOME" SAMPLE

When the age, race, sex, and region cohorts are restricted to individuals with positive incomes, clearer support for a standard view of labor market performance based on human capital and job search is provided. Although the weeks-unemployed equations predict poorly and the effects of education on labor force participation are weak, generally there are no surprises in the results of estimates of weeks worked, weeks unemployed, and wage equations restricted to positive income earners.

In Table 3 the results in weeks worked and weeks unemployed are displayed. Among cohorts of positive income earners, increases in wage and salary income tend to increase weeks worked. This is true for both blacks and whites, in 1968 and 1978. The effects are of a greater magnitude than that discovered for the potential labor force sample, so that one extra dollar of wages tends to stimulate those with positive incomes to work more hours than potential labor force participants would work. Although higher wages have no effect on the unemployment of blacks, whites tend to experience fewer weeks of unemployment when wage and salary incomes rise.

The effects of nonlabor income are not everywhere statistically significant, but their signs are nonetheless consistent with an orthodox intuition. Increases in dividends, rents, and interest tend to lower weeks worked. This sort of capital income has insignificant effects on the weeks unemployed of blacks in 1968 or 1978, and of whites in 1978, although it raises the weeks unemployed of whites in 1968.

Social Security benefits tend to reduce both weeks worked and weeks unemployed of whites in 1978 and 1968, as one would expect for selective withdrawal from the labor force of older workers. Among blacks, the only significant effect of Social Security is on 1978 weeks worked. Here it appears that the prospect of an extra $10.00 of Social Security benefits induces blacks to work nearly 5 extra weeks. This result contrasts with that observed among potential labor force participants, where it was found that higher Social Security income was associated unilaterally with fewer weeks worked.

Specific support for the assertions of Heckman and Butler is found when the estimated effects of transfer payments on labor supply are examined. While in 1968 welfare benefits and public assistance payments had no effect on the number of weeks blacks or whites worked or spent looking for work, in 1978 another scenario appears. Blacks worked fewer weeks when welfare benefits were increased, while experiencing more weeks of unemployment. Whites, who were unaffected by increases in their welfare receipts in 1968, looked a little longer for work, yet did not actually work more weeks in 1978. Because the fall in weeks worked exceeds the rise in the weeks unemployed among blacks, one could argue that this is an indication of selective withdrawal from the labor market occasioned by public assistance, i.e., the basic premise of the Heckman-Butler hypothesis. Recall, however, that when the weeks-worked and weeks-unemployed equations are estimated using cohorts comprising the entire adult population, selective withdrawal appears operative for both blacks and whites. It is even more pronounced for whites! For each additional $1000 in annual welfare and public assistance benefits, whites worked 12 fewer weeks and blacks only 5 fewer weeks in 1978, as estimated from the potential labor force sample in Table 1. While a $1000 increase in welfare increases black unemployment by 2 weeks, there is no effect on whites. Thus, public assistance programs draw whites out of the labor force at a greater rate than blacks. This finding of course is not replicated when the sample is restricted to positive income earners, and thus provides a clue to the origins of the faulty Heckman-Butler argument.

"Other transfer" income effects again support a Heckman-Butler view when the sample is restricted to positive income earners. Increases in such payments as unemployment insurance and alimony tend to reduce weeks worked and raise weeks unemployed for both blacks and whites in

1978, a result not generally found in 1968. But the fewer weeks worked by blacks, for each extra dollar of transfers, exceeds that of whites and exceeds the increase in weeks unemployed by blacks. Since the extra weeks unemployed by whites far exceeds the fall in weeks worked by them, this means that the withdrawal effect is greater for blacks. To see this, we observe from Table 3 that in 1978 blacks work .6 fewer weeks and are unemployed .2 extra weeks for a dollar's increase in other transfers. Whites in that same year work .2 fewer weeks and are unemployed .7 extra weeks for an extra dollar of transfers. So blacks are withdrawing from the labor market--.4 fewer weeks neither looking for work nor working--while whites are merely spending more time looking for work: 1/2 week remains for whites after subtracting the weeks not working from weeks unemployed. The dependence of this conclusion on sample selection bias is illuminated when one notes that no such result is found from the potential labor force sample. Neither blacks nor whites experience a change in overall labor force participation as a result of increases in "other transfers" when the sample composition is unrestricted as is evidenced in Table 1.

Current family variables have differential effects on blacks and whites. Higher family income and fewer earners in the household induce withdrawal among blacks in 1978, but not for blacks in 1968 nor whites in either year. Growth in household size is associated with lower labor force participation among whites but not blacks, suggesting that black individuals' labor force participation is more sensitive to the economic resources available in the family than whites. But a quick look at the current family variables based on the potential labor force sample reveals that this is not true. Blacks and whites are indeed quite similar in the labor force participation responses to family size and resources.

Employment for all females with positive earnings rose from 1968 to 1978, although white females worked fewer weeks than black females. And while being married generally increases labor force participation, education and veteran status seem not to affect the number of weeks worked or unemployed. There are exceptions of course: black veterans were unemployed less often in 1978 and worked fewer weeks in 1968, at the same time that being married lowered weeks worked for blacks. These exceptions correspond more closely to the general findings based on the potential labor force sample: Among blacks, veterans and married persons are in the labor force fewer weeks during the year.

Another prominent reversal in results comes about in the estimated effects of age on labor force participation. Previously we found that among potential income earners, members of the youngest age cohort worked fewer weeks than older cohorts, with whites in that age group (14-23) experiencing a greater drop in weeks worked than blacks. For the positive income sample, however, being in the youngest age group still reduces weeks worked, yet white youths experience a smaller decline than blacks.

Note, in addition, that between 1968 and 1978 both whites and blacks 14-to-23 years old saw a dramatic fall in their labor force attachment, with the gap between them narrowing. This could come about as both white and black youths stay in school and become closer in educational attainment. Indeed, this is precisely the case upon which Welch and Smith rest their argument of a "vintage effect"--and it is here where the case flounders. The positive income sample provides all of the right answers to vintage-hypothesis questions. The age-education interaction terms have the anticipated positive signs; the disparities in age-education effects in the youngest age group narrows between 1968 and 1978. But few of these findings are supported by the earlier estimations based on the potential labor force sample. This point can be seen more clearly by a direct examination of the wage-equation estimations for the positive income sample displayed in Table 4. These equations show that when the sample is restricted to individuals with positive earnings, reductions in labor force participation tend to reduce black-white inequality and that the younger cohorts tend to contribute to reduced inequality. Not only does greater educational attainment among young adults (24-33 years old) drastically diminish black-white income inequality among "positive income earners," we demonstrate below that being in the youngest age group also results in lessening income inequality.

In 1968 blacks aged 14 to 23 with positive incomes received lower wages than the relevant comparison group--in this case blacks over 53 years old, the omitted age dummy. Being black and 14 to 23 years old in 1968 meant receiving \$1.10 less in log-wages. However, white youths received \$2.98 less in log-wages in 1968 and \$3.34 less in log-wages in 1978 as compared to the oldest white age cohorts in

Table 4

Instrumental Variable Estimates of Coefficients of Black-White Wage Equations 1968 and 1978: Positive Income Sample
(t-statistics in parentheses)

	Ln Wage					
	1968		1978			
	Black (1)	White (2)	Black (3)	White (4)	Between Cohort Changes in Inequality (5)	Within-Cohort Changes in Inequality (6)
Constant	4.228 (9.828)	5.423 (5.602)	2.051 (7.768)	4.899 (23.315)	-1.653	--
Labor force participation rate	3.735 (7.423)	3.416 (9.550)	2.533 (3.901)	3.782 (14.067)	-1.568	--
Education	.384 (1.311)	.253 (.718)	.277 (10.189)	.705 (3.633)	-.559	--
Veteran	.252 .652	-.426 (-1.328)	1.207 (5.527)	-.383 (-2.681)	.912	--
Sex	-.242 (-1.794)	-.555 (-5.095)	.161 (1.241)	-.819 (-8.138)	.677	--
Married, spouse present	.209 (1.672)	.178 (1.528)	1.147 (7.258)	.144 (.124)	.972	--
Northeast	.624 (1.160)	-.613 (-2.304)	.251 (3.089)	-.119 (-3.671)	-.867	--
North Central	.155 (3.194)	-.863 (-3.679)	.184 (2.025)	-.903 (-2.922)	.069	--
Southeast	-.127 (-2.336)	-.100 (-3.820)	.213 (2.510)	-.424 (-1.247)	.644	--
<u>Age-veteran interaction</u>						
14-23	.341 (.476)	.978 (2.627)	-.762 (-.739)	-1.211 (-1.584)	1.086	--
24-33	-.124 (-.236)	.632 (1.615)	-1.480 (-2.782)	1.099 (3.939)	-1.823	-1.942
54-43	-.385 (-.750)	-.427 (-.104)	-1.564 (-2.648)	.440 (1.041)	-2.046	-1.248
44-53	-.155 (-.322)	.467 (1.293)	-1.472 (-3.078)	.345 (.901)	-1.195	-1.859

Table 4--continued

	Ln Wage				Between Cohort Changes in Inequality	Within-Cohort Changes in Inequality
	1968		1978			
	Black (1)	White (2)	Black (3)	White (4)	(5)	(6)
Age-Education interaction						
14-23	.123 (2.947)	.222 (6.304)	-.393 (-.740)	.220 (8.560)	-.514	--
24-33	-.850 (-.171)	.427 (.886)	-.218 (-2.445)	-.387 (-.611)	1.446	.268
34-43	.267 (.632)	.517 (1.059)	-.122 (-1.932)	.305 (.544)	-.177	.850
44-53	.199 (.539)	.108 (.221)	-.213 (-4.588)	.329 (.652)	-.633	-.812
Age-Sex interaction						
14-23	.222 (.138)	.347 (2.895)	-.390 (-2.127)	.449 (3.680)	-.714	--
24-33	.205 (1.056)	.474 (2.943)	-.400 (-1.754)	.868 (5.506)	-.999	-1.143
34-43	-.128 (-.537)	-.196 (-.919)	-.572 (-2.156)	.389 (1.679)	-1.029	-.692
44-53	-.597 (-.286)	.961 (.588)	-.495 (-1.590)	.249 (.882)	.814	-.812
Age						
14-23	-1.097 (-2.119)	-2.976 (-8.079)	.408 (.617)	-3.344 (-13.133)	1.873	--
24-33	.203 .417	-.778 (-1.452)	2.652 (2.385)	-.310 (-.381)	1.981	1.083
34-43	-.504 (-.139)	-.299 (-.533)	1.604 (2.268)	-.717 (-.896)	2.526	1.340
44-53	.707 (-.220)	-.208 (-.430)	2.698 (5.449)	-.646 (-.946)	2.429	3.849

Table 4--continued

	Ln Wage					
	1968		1978		Between Cohort Changes in Inequality	Within-Cohort Changes in Inequality
	Black (1)	White (2)	Black (3)	White (4)	(5)	(6)
Mobility: Moved						
North to West	1.633 (1.370)	-.778 (-.699)	.956 (.692)	.813 (.758)	-2.268	--
South to West	-1.334 -1.189	-1.052 (.849)	2.795 (2.293)	-1.318 (-.773)	4.395	--
South to North	.578 (.743)	.563 (.664)	1.736 (2.261)	.544 (.496)	1.177	--
North to South	.191 (.120)	1.171 (1.731)	-.490 (-.302)	.219 (.380)	.271	--
R^2	.855	.985	.871	.970		
Sum of squared residuals	35.278	4.451	105.816	10.987		
No. of observations	384	384	464	464		

those years. Since black youths actually receive more log-wages than the oldest blacks in 1978, the 14-to-23-year-old cohort contributed to a decline in overall black-white inequality, as the vintage hypothesis suggests. We reiterate, though, that this finding is only forthcoming when the sample is restricted to positive income earners. For the potential labor force sample, the youngest age group adds to inequality. This evidence is found in columns 5 of Tables 2 and 4.

Lest one conclude that the challenge to the vintage hypothesis posed here stems from the inclusion in Table 2 of in-school youths--who, while adding to their human capital, temporarily forego earnings--an examination of the age-education interaction terms is revealing. Because the increase in earnings for increments in educational attainment among 14-to-23-year-olds is greater for whites than it is for blacks in both 1968 and 1978, the net effect of the progress in education for this group is to increase inequality. But this is the very age group that is in school, and thus likely to be out of the labor force altogether. So the relevant comparison, Smith and Welch would argue, is between black and white 24-to-33-year-olds, those who have completed their schooling. And indeed, when the positive income sample is examined it is found that the effect of increased education among the young-adult cohort contributes to a decline in overall black-white income inequality. Moreover, as the 14-to-23-year-olds age to 24 through 33 years old from 1968 to 1978, the impact of increased education is to reduce black-white wage inequality.

We calculate that the age-education interaction effect on inequality to be respectively 1.446 and .268 for cohorts 24 to 33 years old in 1968 and 1978 (between-cohort changes) and for the cohort 14-23 years old in 1968 and 23-33 years old in 1978. Since both of these estimates are positive, this means that inequality, both between age cohorts and within age cohorts, was declining for young adults receiving positive incomes. This clear support for the vintage hypothesis is, however, conspicuously absent from the evidence on potential labor force participants, where we found that increased education among 24-33 year olds increased both between-cohort and within-cohort inequality.

While the comparison of estimated wage equations utilizing both "potential labor force" and "positive income" samples casts doubt upon the vintage hypothesis, it sheds some light on the Butler-Heckman selectivity bias argument. In Table 4 it can be seen that increases in the labor force participation rates of black and white earners raise their respective log-wages. However, the impact is reduced for blacks and rises for whites between 1968 and 1978. So, should labor force participation increase, the net effect would be to increase black-white income inequality. The fact is, though, that between 1968 and 1978 black labor force participation rates <u>fell</u>. The result was reduced inequality. This illusory progress is precisely the brunt of the Butler-Heckman attack. Still, our evidence from the analysis of weeks worked and weeks unemployed of blacks and whites pointedly challenges the Butler-Heckman view that the source of selective withdrawal from the labor market is the greater attraction of social transfers to blacks. What really appears to be happening is that gains from labor force participation are diminishing for blacks. And while it is innocent enough to suppose that alternative income sources thereby appear more attractive to them, it is not necessarily the case that blacks respond by gorging the free meals of the new welfare state while whites dutifully continue to look for work. It is as plausible that the gains to black labor force participation have fallen because of increased competition among blacks for new and existing jobs as it is because of rising returns to illegitimate activity, for those who do not work.[8] Neither possibility has been systematically modelled here or elsewhere. Hence, in the absence of more convincing evidence, we view the ultimate causes of selective withdrawal with great caution.

V. SUMMARY AND CONCLUSIONS

We have argued that the economic progress made by blacks in the decade 1968 to 1978 is much less dramatic than other researchers have suggested. Our case is based upon estimates of wage equations using data on what we call the "potential labor force" instead of the commonly used "positive earners." Whereas a simple computation of the ratio of black to white wage and salary incomes for positive earners would show that the mean earnings ratio rose from .605 in 1968 to .748 in 1978, when the potential labor force sample is examined the rise in the black-to-white earnings ratio is only from .660 to .700 during that decade.[9] This evidence alone would have been enough to question whether the economic progress made by blacks during the 1960s was sustained in the 1970s.

But we have gone further. We have attempted to model the determinants of changes in black-white income inequality in order to question whether the explanations given for the apparent narrowing of the pre-1970s gap in black-white incomes are plausible explanations for changes in inequality between identical black and white age, sex, and region cohorts. We have argued at length in a previous paper that it is entirely possible for the overall mean of black incomes to be rising relative to mean white incomes when the ratio of black to white income is not improving in any specific sex, age, or region cohort.[10] In fact, had we been content in questioning only whether individual age, sex, and region cohorts of blacks have progressed relative to identical whites, we would have discovered that on the average there has been little continued convergence in black-white cohort incomes.[11] But even if average relative incomes of equally weighted cohorts remained essentially stable, while overall black-white income ratios increased, should not those factors that can be counted on to reduce inequality between blacks, as a group, and whites, as a group, also be reliable determinants of changes in inequality between individual black and white cohorts?

The answer is one-third "yes" if we blindly constrain our analysis to a highly selective and biased sample. Among positive income earners, being in the younger cohorts and receiving additional education significantly contributes to the decline in racial wage inequality. Yet, this well-received explanation of Smith and Welch is refuted when the data on the potential labor force participants are examined. The results of

Table 2, which details estimates of black and white wage equations for all individuals over 14 years, reveal that not only does increased education among younger adults not reduce racial income inequality, it does not generally reduce racial inequality within a birth-cohort as that cohort ages. So it is seen that the case for the "vintage effects" explanation for changes in black-white <u>cohort</u> inequality is significantly weakened when the broader sample of "potential" labor force participants is explored.

Another one-third of the answer addresses the Butler-Heckman argument that the convergence in black-white earnings among earners is an artifact of self-selection bias. Indeed, it is found that increases in labor force participation tend to reduce racial income inequality among "potential" labor force participants but to increase it among positive earners. Since black labor force participation rates--especially for males--have tended to fall, the observed effect is a narrowing of the income gap between blacks and whites who have "chosen" to remain in the labor force and a widening of the gap between all blacks and all whites, including those who have withdrawn from the labor market. Butler and Heckman argue that this narrowing of the gap observed among positive earners is occasioned by the selective withdrawal of blacks who choose to receive welfare and other transfer payments rather than to work. Our findings suggest that there is an element of truth to the selective withdrawal argument, but that the <u>cause</u> of the withdrawal is still open to question. In particular, we do find that both whites and blacks in the potential labor force work fewer weeks as welfare and public assistance income increases. But the reduction is greater for whites! Thus labor force withdrawal due to the lure of a life on welfare can hardly be an adequate explanation for the improved relative position of blacks among positive income earners (unless, perhaps, the whites who are drawn out of the labor force have <u>higher</u> potential earnings than the whites who remain).

A final third of the answer is not addressed explicitly in this paper. Richard Freeman has argued that affirmative action and related civil rights activities can account for a substantial decline in the gap between black and white incomes; any gap that may remain, he suggests, is due to differences in family background variables. We have neither directly explored the impact of affirmative action on earnings and labor force participation in this paper, nor have we developed rigorous tests of the effects of family background on racial income inequality. Thus, in many ways our pessimistic assessment of the changing relative position of black age, region, and sex cohorts may be in part due to the omission of a systematic exploration of a Freeman-type explanation of a decline in labor market discrimination--along with its optimistic assessment of continued improvement in blacks' relative economic position. Coupled with our strong finding of selectivity biases however, future research could be directed toward (1) examining the differential cohort-specific effects of affirmative action and (2) investigating whether declines in civil-rights-inspired legislative and court efforts to reduce racial employment inequalities have been accompanied by stabilized or widening earnings differences.

In sum, then, a strong case against the dramatic improvement hypothesis relating to black economic progress from 1968 to 1978 can be made. The case depends heavily upon the inherent bias arising from sample selection. Our findings suggest a much less simplistic view of the selection bias problem than that offered previously by Butler and Heckman. Hence, the task is left for further research to unravel the paradox of simultaneously deteriorating employment experiences among blacks and their rising relative incomes.

NOTES

[1]Richard B. Freeman, "Changes in the Labor Market for Black Americans, 1948-1972," Brookings Papers on Economic Activity 1, 1973: idem, "Decline of Labor Market Discrimination and Economic Analysis," American Economic Review: AEA Papers and Proceedings 63:280-286, 1973; James P. Smith and Finis Welch, "Black-White Male Wage Ratios: 1960-1970," American Economic Review 67:323-38, 1977.

[2]Richard Butler and James J. Heckman, "The Impact of the Government on the Labor Market Status of Black Americans: A Critical Review of the Literature and Some New Evidence," in Equal Rights and Industrial Relations (Leonard S. Hausman et al., eds., Madison, Wisconsin: Industrial Relations Research Association, 1977); idem, "A New Look at the Empirical Evidence that Government Policy Has Shifted the Aggregate Relative Demand Function in Favor of Blacks," unpublished manuscript, University of Chicago, 1978.

[3]Edward Lazear, "The Narrowing of Black-White Wage Differentials is Illusory," American Economic Review 69:553-564, 1979.

[4]Robert B. Hill, The Illusion of Black Progress (Washington: NUL Research Department, 1978).

[5]Jerome Culp and Glenn Loury, "Impact of Affirmative Action on Equal Opportunity," Review of Black Political Economy (forthcoming).

[6]William Darity, Jr., "Misleading Averages and the Illusion of Black Progress," Review of Black Political Economy (forthcoming).

[7]Since adequate 1968 data are not available for those individuals 14-23 years old in 1978, no similar comparison can be made for this group of young persons.

[8]See Samuel Myers, Jr., "The Economics of Crime in the Urban Ghetto," Review of Black Political Economy, Fall, 1978.

[9]See Table 1, William A. Darity, Jr. and Samuel L. Myers, Jr. "Changes in Black-White Income Inequality, 1968-1978: A Decade of Progress?" Review of Black Political Economy (forthcoming).

[10]Ibid.

[11]The mean log-wages for black and white cohorts in the potential labor force in 1968 are 7.38311, 7.66207, and in 1978 are 7.14848, and 7.57403. For positive income earners the 1968 figures are 7.77623, and 8.13737, whereas the 1978 figures are 7.78944, and 8.16110. Hence the ratio of average black to white cohort earnings fell from .76 to .65 from the potential labor force perspective but remained about stable at .70 when measured using only positive earners.