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Economic Status as a Determinant of Mortality Among Nonwhite and White Older Males: Or, Does Poverty Kill?

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ABSTRACT

The evidence presented in this paper shows that differential mortality rates by economic status are strongly present in the United States today, and that this relationship is monotonic, with the wealthiest decile having lower death rates than the next wealthiest decile. Differential mortality rates by economic status can be said to be confused with the well-known racial difference in mortality. An implication of this paper, then, is that racial differences in mortality are, in large part, a consequence of poverty or low permanent income, as opposed to racial genotype. Consequently, it may be just as valid, or even more so, to publish mortality tables by income as by race.

Another implication of this paper is that the redistributive effects of longevity-based transfer systems, such as Social Security, may be less "progressive" than assumed, since would-be poorer recipients are either less likely to live long enough to collect any benefits in the first place or will not live to collect them for as long a period of time as will more affluent recipients.

In addition, I fail to find a direct effect of schooling on survival-probability. Consequently, the beneficial effect of schooling on longevity must work through its effect upon income, with only the latter directly influencing mortality risk.

ECONOMIC STATUS AS A DETERMINANT OF MORTALITY AMONG NONWHITE AND WHITE OLDER MALES: OR, DOES POVERTY KILL?

"Although substantial differences in mortality persist within and between developed countries, they are . . . unrelated to income."

(Victor Fuchs, 1986, p. 199)

This paper investigates whether or not differential mortality rates by economic status and certain other observable criteria exist in the United States today. It is well-known that nonwhites have shorter lifespans than whites. What is not certain at all is whether nonwhites, living in identical socioeconomic environments as whites, live for a shorter period of time. Based upon research using National Longitudinal Survey data, I argue that differential mortality rates across races are dramatically reduced after controlling for a few economic status variables. If it is indeed the case that economic status is important to longevity, the well-known fact that whites live longer than nonwhites may be just another way of saying that poverty shortens life.

My interest in differential mortality rates stems from previous research undertaken with Martin David (David and Menchik, 1988) and later with two other colleagues (Jianakoplos, Menchik, and Irvine, 1989). In the latter project, Jianakoplos, Irvine, and I demonstrated, using the National Longitudinal Survey of Older Men, the biases in the estimation of age-wealth profiles one gets using only cross-sectional data. With differential mortality rates by wealth only (viz. the poor die young, the rich die old), the profiles' cross-sectional estimates were biased upward.

The implications of differential mortality rates by economic status for research are more serious than we at first realized. If differential mortality rates by economic status exist, studies that use the "synthetic cohort" approach may be presenting too optimistic a picture of the economic outlook for the elderly. For example, Hurt and Shoven, in a 1985 essay, examine successive cross-sections of the Retirement History Survey (p. 126). They claim that between 1968 and 1978, real income of the lower tail of the income distribution increased despite more retirements. The authors, however, analyzed income distributions of a birth cohort that had an ever dwindling number of living representatives. If differential mortality rates by economic status exist in the United States, poorer cohort representatives die at a faster rate than do richer cohort representatives; therefore, the reported increase in incomes among the poor is misleading, a consequence of sample selection bias.

The same criticism applies to the study by Ross, Danziger, and Smolensky (1987), which compares income, relative to needs, for those aged 60 to 64 in 1969 with those aged 70 to 74 in 1979. The rather large increase in the birth cohort's income to needs ratio is, in part, due to the compositional change resulting from differential mortality rates. Consequently, the finding that, as the elderly aged, their real incomes increased may have been oversold.

Given the formula that determines old-age pension benefits from labor earnings, many authors have noted that the Social Security System reduces income inequality among recipients (e.g. Hurd, 1990). But, if differential mortality rates by lifetime income exist, the lifetime redistributive effects of Social Security may be less equalizing than hitherto assumed, since low-wage workers, who stand to benefit the most per dollar contributed to the system, either fail to live long enough to collect any benefits in the first place, or die sooner and so collect them for a shorter period of time than do higher wage workers who live longer.

PREVIOUS INVESTIGATION

There is a large literature in Britain presenting clear, age-adjusted mortality-rate differentials by "social class" (occupational class) (see Inequalities in Health and The Black Report). Death records were used to divide decedents into six classes, three white-collar and three blue-collar. Generally, those men in the "highest" (or highest paying) occupational classes had lower mortality rates than those in the "lower" classes.[1] It is often pointed out, however, that since class is an imperfect measure of income, an income/mortality link would produce more reliable differentials than the present class/mortality link (see Wilkinson, 1986). Accordingly, an emphasis has shifted from social class as a determinant of mortality-rate differentials to traditional indicators of wealth, such as housing accommodations (privately owned home, rental, and public housing) and automobile ownership (see Goldblatt, 1987, unpublished paper cited in Inequalities in Health, p. 237).

On this side of the Atlantic there exist only a handful of studies relating income and mortality (see, most notably, Kitagawa and Hauser, 1973; Hadley, 1982; Rosen and Taubman, 1979; and Taubman and Rosen, 1982). Much of the U.S. work relies on the health capital and production function approach (see Auster, Leveson, and Sarachek, 1969; Silver, 1972; and Grossman, 1976). The U.S. empirical research is based on a high level of aggregation, Standard Metropolitan Statistical Areas (SMSA's), states, counties, birth cohorts, or, at best, on census tracts, and individuals are classified by annual income.

Analyses of data taken from large aggregations raise questions of unmeasured differences across groups, while analyses of single year income data raise issues of transitory versus permanent effects and reverse causality. Suppose, for example, one is in poor health for reasons unrelated to his or her current economic status: he or she is more likely to die and less likely to work and thus to earn while ill. Hence, even though it cannot be said that low income causes or even contributes to death, the negative statistical association between the two might lead a researcher to make such erroneous inferences. Using permanent income measures (e.g., wealth) over a number of years prior to death, or income over a long period of time, would allow a researcher to better estimate the causal relationship between economic status and mortality rates denied by Fuchs in the quotation heading this paper. Another way to avoid such erroneous inferences is to control for bad health, thereby discovering if income within health status groups indeed matters. Some results in this paper were obtained by controlling, albeit imperfectly, health status. It should be noted that the above reasoning does not rule out the possibility of a significant short-term or transitory income effect on mortality-probability. Indeed, people with equal, permanent, or average incomes may differ in their risk of falling into poverty. It would be interesting to know if short-term or temporary impoverishment, ceteris paribus, is associated with higher mortality rates.

Income may affect mortality-probability in several ways: through patterns of diet and nutrition, access to and use of health services, demand for "inferior" goods, etc. Low-income people are likely to consume more cheap but filling goods, more starch and sugar, and less fresh fruit and vegetables than are higher-income people (p. 111 in Inequalities in Health). Although the consumption of health care services--either preventative or acute--is a normal good, low income would lead some to delay consumption until too late.

Compensating differentials aside, lower-income people might be exposed to more health risks on the job. It has been argued that unemployment is associated with deleterious movements in health indicators. Brenner (1977) has found that economic recessions, after a lag, lead to increases in deaths from cirrhosis of the liver, cardiovascular diseases, suicides, and homicides. One argument contends that stress levels, intensified during periods of recession, are to blame for such increases. Of particular interest is a British study by Michael Marmot, et al. (1984) of fatal cardiovascular diseases among civil servants. The study breaks the sample into administrators (the highest paid group), executives/professionals, clerical workers, and others. Age-adjusted death rates varied inversely with occupational class: there was a 60 percent difference in death rates between the top two groups. What is particularly noteworthy about this study is that even after controlling for lifestyle differences such as smoking, a 50 percent mortality-rate differential remained between the two groups.[2]

METHOD OF ANALYSIS

A natural way to proceed is to hypothesize mortality-probability as functionally related to four types of effects: **family background** effects, including race; **permanent income** effects; **transitory income** effects; and **other environmental** effects.

$$\mathbf{M} = f(\mathbf{B}, \mathbf{Y}_p, \mathbf{Y}_t, \mathbf{E})$$

Panel data helps distinguish permanent from transitory income factors; logistic analysis is used to estimate the conditional effects of explanatory variables on mortality-probability.

Critics of the British research on differential mortality rates by social class argue that mortality-rate differentials that may appear to be a consequence of economic or social position might instead result from one's family background. These background effects may be learned (acquired tastes) or inherited (genotype), but if they are related to observed economic or social positions, researchers might wrongly infer that differences in position are the source of differences in mortality rates when, in fact, background jointly determines both. In other words, background is a key variable omitted in the British approach. Since the omitted variable is related to both the included variables (social class) and the dependent variable (death rate), observed empirical estimates would be afflicted with omitted-variable bias.

In this study, I attempt to avoid the omitted-variable problem by using three background variables as controls: how many of the subject's parents were alive in 1966 (when the median respondent was 51 years old); years of schooling completed by the head of the subject's household when the subject was 15; and the subject's race (black or nonblack).[3]

Two variables convey the concept of permanent income. First, the net worth of the subject's household in 1966 (the year the panel study began), calculated by combining the value of his home less debts on his home; the value of investment real estate less debts on the real estate; and the value of bank accounts, mutual funds, stocks, bonds, farm equity, business equity, U.S. savings bonds, and personal loans made to others, all less unsecured debt. (This data, as well as all the data in this project, come from the National Longitudinal Survey of Mature Men, a panel study covering the years 1966 to 1983.) Second, an empirical permanent earnings measure, the subject's average annual discounted earnings (under a 2 percent real discount rate), is used. Only earnings data prior to age 62 are used unless the subject continued to work full-time beyond age 61, in which case all earnings are used, thereby allowing us to distinguish earnings capacity from differential retirement effects. All financial variables are expressed in constant 1976 dollars. Since the data base is a panel study, this earnings variable uses information on as many as 11 years of reported annual earnings.[4] The earnings data are reported as the income is earned and are not subject to the recall error present in retrospective surveys.

Transitory income is measured in terms of poverty status. I have computed poverty status by aggregating all sources of income and, after following the official practice of subtracting the bonus value of food stamps, comparing this amount with the official poverty lines adjusted for family size and age. The actual variable used in the estimated equations is "poverty-intensity": the number of years the subject lived in poverty divided by the number of years income was observed in the survey. The index, which can range from zero to one, has a sample mean of .156. For nonblacks the mean is .101, and for blacks--a group overrepresented in this study--the mean is .293. My approach is to answer the question, "Holding long-run or permanent income status constant, were those who had a relatively intense poverty experience more likely to have died than those not as intensely impoverished?"

Environmental variables include a dummy variable for living in the South in 1966 (Census Regions 5, 6, and 7), and for living in a small town (an area with a labor market of 50,000 people or less). I also used another dummy variable that assumes a value of one if the subject was in poor health in 1966, and a continuous variable, representing years of schooling completed by the subject as of 1966.

Researchers have found a link between marital status and mortality rates (see, e.g., Korenman, Goldman, and Hu, 1990). It appears that married men have higher survival rates than those not married, although it is not known if this is because of "selection" or "protection." Either married men receive health-augmenting care from their wives, or marriage itself is a precondition for good health. In any case, I utilized three marital status categories: married; never married; and finally widowed, divorced or separated, all in 1966. Finally, death rates are age related. The age variable is the subject's age in 1966, the first survey year. The NLS men varied in age from 45 to 59 years old, with 51.5 the average age. (Consequently, these men were born in the period 1907 to 1921.)

The dependent variable indicates whether the subject was dead in 1983. The National Longitudinal Survey records death as one source of nonresponse. Of the 5,020 men surveyed in 1966, 1,498 had died by the 1983 interview.

If several factors together can predict mortality, there is a natural way (at least natural to economists) to think of a locus of points predicting a given death rate. Consider the **isomort**, a combination of factors that predict a specific death rate. If a person's wealth, conditional upon age, is inversely related to the probability of dying during a period of time, there exists a set of age/wealth combinations that predict a given risk of death: there is an amount of wealth that "compensates" for the increased likelihood of death associated with increased age (at least within a range around the mean of the data). Alternatively, one could compute the amount of compensating wealth that would predict equal death rates by race or even by gender. In the next section I present estimates of logit models that could be used to calibrate the "isomort" model.

RESULTS

The starting point in this paper is a byproduct of the research with Jianakoplos and Irvine (p. 565), referred to above. In Table 1, the men who died before the 1981 survey are grouped according to net wealth. Note that the ratio of those who died in the poorest group exceeds the ratio of those who died in the richest group by nearly three to one! Tabular presentations here do not control for other factors that influence survival, such as age.

Table 2 presents logit equation estimates of the prospects of failing to survive the 17 years of the survey. The dependent variable, "kndead83," is unity if the respondent is known by the NLS to have died prior to the 1983 survey and zero otherwise. Note that sample sizes vary across equations as a consequence of variations in the number of full-cell observations. In Column 1, the subject's age in 1966 is shown to have a significant, positive effect on mortality-probability. This should come as no surprise, since the likelihood of death increases with age. The coefficient "age66" translates into an increased death risk of about 1.9 percent for an additional year of age. Put another way, a man one year older than the sample mean would have a 31.8 percent chance of not surviving the study as compared with the 29.9 percent chance of the mean aged man.

In Column 2, "pared"--the completed years of schooling of the subject's household head when the subject was 15--is added. The sign of this variable is negative, or mortality-reducing, when no other regressors besides age are present in the equation. Hence, it appears that subjects whose parents were not well-educated faced a higher mortality risk than those whose parents had more schooling. (Using "pared" drastically reduced the sample size, since the response rate on this question was so low.)

In Column 3, a different background variable, "nparal"--the number of living parents the respondent had in 1966--is utilized. Its effect is strong and predictable: the greater the number of living parents a subject had in 1966, holding his own age constant, the lower his mortality-

Table 1

Mortality Rates By Wealth Percentile, 1966

1966 Wealth Percentile	Fraction of Remaining Cohort 1966-69	1969-71	1971-76	1976-81	1966-81 Fraction of Initial Cohort
1-20	.07	.06	.18	.19	.35
21-40	.05	.04	.11	.13	.24
41-60	.04	.03	.11	.15	.21
61-70	.06	.04	.09	.10	.22
71-80	.04	.03	.08	.10	.15
81-90	.04	.02	.06	.11	.15
91-100	.03	.02	.04	.09	.12
1-100	.05	.03	.11	.14	.22

Source: Computed from the National Longitudinal Surveys of Mature Men (1966-1981).

Note: Sample consists of 4,546 households which reported valid age and wealth data in the 1966 survey.

Table 2

Logit Equation Estimates of Failure to Survive 17-Year NLS Older Men Panel Study, 1966-1983 (t-statistic in parentheses)

Variable Name	Effects of: (1) Age	(2) Parents' Education	(3) Living Parents	(4) Parents' Education and Living Parents	(5) Wealth and Permanent Earnings
age66	.0945 (12.58)	.0818 (8.303)	.0892 (10.503)	.0763 (7.387)	.0691 (5.692)
pared		-.0198 (2.018)		-.0174 (1.777)	.0202 (0.854)
nparal			-.3061 (5.982)	-.2931 (4.516)	-.2462 (3.360)
weal66p					-1.54e-06 (2.051)
pearn					-2.26e-05 (2.830)
constant	-5.76 (14.62)	-5.430 (10.31)	-4.944 (11.91)	-4.600 (8.281)	-4.216 (6.361)
chi square (df)	163.38 (1)	88.97 (2)	197.30 (2)	109.77 (3)	95.45 (5)
N	4,971	2,952	4,943	2,950	2,438
Mean of kndead83	.299	.278	.298	.278	.253

Note: The term "e-05" stands for 10 taken to the minus 5 power, "e-06" 10 taken to the minus 6 power.

(table continued)

Table 2 (continued)

Logit Equation Estimates of Failure to Survive 17-Year NLS Older Men Panel Study, 1966-1983 (t-statistic in parentheses)

Variable Name	Effects of: (6) Excluding Parents' Education	(7) Years in Poverty	(8) Location	(9) Marital Status
age66	.0749 (8.03)	.0748 (8.01)	.0741 (7.93)	.0743 (7.94)
nparal	-.2780 (4.73)	-.2824 (4.79)	-.2758 (4.67)	-.2731 (4.62)
weal66p	-2.78e.06 (3.67)	-2.81e-06 (3.74)	-2.46e-06 (3.30)	-2.42e-06 (3.27)
pearn	-2.09e-05 (3.29)	-1.46e-05 (2.09)	-1.83e-05 (2.54)	-1.60e-05 (2.21)
npov		.320 (2.03)	.411 (2.49)	.401 (2.43)
south			.018 (0.230)	.027 (0.337)
small town			-.249 (2.87)	-.241 (2.77)
dmar				-.232 (2.10)
constant	-4.36 (8.57)	-4.47 (8.68)	-4.25 (8.36)	-4.19 (8.00)
chi square (df)	192.46 (4)	195.06 (5)	203.46 (7)	207.83 (8)
N	3,969	3,946	3,946	3,946
Mean of kndead83	.274	.274	.274	.274

Note: The term "e-05" stands for 10 taken to the minus 5 power, "e-06" 10 taken to the minus 6 power.

(table continued)

Table 2 (continued)

Logit Equation Estimates of Failure to Survive 17-Year
NLS Older Men Panel Study, 1966-1983 (t-statistic in parentheses)

Variable Name	Effects of: (10) Health[a]	(11) Better Health[b]
age66	.075 (7.89)	.078 (7.21)
nparal	-.260 (4.32)	-.256 (3.76)
weal66p	-2.44e-06 (3.28)	-2.46e-06 (2.92)
pearn	-1.36e-05 (1.88)	-1.35e-05 (1.65)
npov	.359 (2.06)	.437 (2.05)
south	-.020 (0.249)	.086 (0.91)
small town	-.216 (2.43)	-.187 (1.82)
dmar	-.279 (2.49)	-.381 (2.94)
constant	-4.26 (7.97)	-4.43 (7.24)
chi square (df)	191.0 (8)	166.1 (8)
N	3,834	3,058
mean of kndead83	.268	.254

Note: The term "e-05" stands for 10 taken to the minus 5 power, "e-06" 10 taken to the minus 6 power.

a Only those whose health was better than "poor" are represented.
b Only those whose health was "excellent" or "good" are represented.

probability. Based on the estimated coefficients, having one parent alive (as compared with none) is equivalent (in the sense of lying along the same isomort) to three fewer three years of age in terms of survival-probability.

When both background variables are included (Column 4), parental education becomes statistically insignificant while the effect of "nparal" hardly changes. This insignificance, coupled with the large loss of full-cell cases, makes the use of the background variable "pared" problematic.

In Column 5, we see the significant effects the wealth (weal66p) and permanent earnings (pearn) variables have on mortality-probability. The effects are clearly in the direction indicated in Table 1: higher-income/wealthy subjects have lower death rates than the less-well-off members of their birth cohort. Note the continuing strong effect of "nparal" and the insignificance (and wrong sign) of "pared." In Column 6, "pared" is dropped and the model fit is improved with the larger sample.

The poverty variable "npov" is included in Column 7, and its statistically significant effect is life-shortening. In Column 8, the location dummies are included. It appears that, cet. par., living in the South is unrelated to mortality risk, but living in a small town is associated with a reduced mortality risk. Marital status is included as a regressor in Column 9. The variable "dmar" is unity if the subject reports being married in 1966. It appears that those men who were married had a significantly lower mortality risk than had nonmarried men.[5]

Although the association between economic status and mortality is, in the tables, fairly clear, is this association indeed causal? After all, it could be the case that poor health over one's lifetime is a precondition for both a low economic status and an early death. Respondents were asked in 1966 to assess their own health status as compared with other men their age. The answers were "excellent" (33.8 percent), "good" (41.5 percent), "fair" (17.5 percent) and "poor" (7.2 percent). The estimated equations in Column 10 are only for those not in poor health, and in Column 11, only for those in excellent and good health. The coefficients of the wealth and poverty variables are large and retain statistical significance, although the size and significance of "pearn" appears to decline. Consequently, we can say that even <u>within</u> health classes, more affluent men have a lower mortality risk; therefore, the economic status/mortality link is, in all likelihood, causal.

Table 3 depicts the association between race and mortality. Blacks face greater mortality risks than whites, as Column 1 shows; in fact, the effect of being black on mortality risk is equivalent to over five years of increased age. Controlling for background by using "nparal" results in a small reduction of the black coefficient, but the effect of being black is still equivalent to over five years of increased age.[6]

Controlling for permanent income and wealth leads to a major reduction of the black coefficient. As Column 3 reveals, the effect of being black on mortality falls from being equivalent to over five years of increased age to about two and one-half. In Column 4, poverty and the location variables are included, and in Column 5, marital status is added as well. As can be seen, controlling for economic and environmental variables leads to a 75 percent reduction of the effect of being black on mortality. The coefficient "DBLACK" is still positive at .135, but is of marginal statistical significance. If we exclude "nparal" from the model (in which case we would be arguing that <u>all</u> of the effects of having longer-lived parents were due to genotype), the coefficient of "DBLACK" is .175, with the t-ratio 1.92 (Column 6).

Table 4 examines the role schooling plays on mortality risk. The variable "GRADEC" is the number of grades of school completed by the subject as of 1966. The range is from zero to eighteen, with whites averaging 10.3, and blacks, 6.8. The first three columns show the effects of schooling-completion on mortality risk, controlling only for age. In the overall sample and among nonblacks, but notably <u>not</u> among blacks, schooling appears to have beneficial effects. Once, however, the permanent income variables are included (Column 4), the additional, beneficial effect of schooling is

Table 3

Logit Equation Estimates of the Role of Race in Failure to Survive
17-Year NLS Older Men Panel Study, 1966-1983 (t-statistic in parentheses)

Variable Name	Effects of: (1) Race	(2) Race and Number of Living Parents	(3) Wealth and Permanent Earnings	(4) Poverty, Living in the South, and in Small Town	(5) Being Married	(6) Excluding Number of Living Parents
age66	.0951 (12.56)	.0838 (10.68)	.0764 (8.16)	.0751 (8.01)	.075 (8.01)	.086 (9.46)
DBLACK	.517 (7.61)	.466 (6.78)	.191 (2.26)	.147 (1.61)	.135 (1.48)	.175 (1.92)
nparal		-.269 (5.20)	-.267 (4.52)	-.269 (4.54)	-.267 (4.51)	
weal66p			-2.54e-06 (3.42)	-2.31e-06 (3.13)	-2.28e-05 (3.11)	-2.33e-06 (3.17)
pearn			-1.71e-05 (2.63)	-1.67e-05 (2.31)	-1.46e-05 (2.02)	-1.60e-05 (2.23)
npov				.366 (2.19)	.360 (2.15)	.349 (2.09)
south				-.022 (.265)	-.011 (0.130)	-.008 (0.096)
small town				-.232 (2.65)	-.225 (2.57)	-.231 (2.65)
dmar					-.221 (2.00)	-.227 (2.07)
constant	-5.94 (14.94)	-5.20 (12.40)	-4.55 (8.81)	-4.46 (8.52)	-4.29 (8.12)	-4.99 (9.80)
chi square (df)	220.46 (2)	242.67 (3)	197.55 (5)	206.07 (8)	210.03 (9)	191.4 (8)
N	4,971	4,943	3,969	3,946	3,946	3,954
Mean of kndead83	.2989	.2743	.2741	.2744	.2744	.274

Note: The term "e-05" stands for 10 taken to the minus 5 power, "e-06" 10 taken to the minus 6 power.

Table 4

Logit Equation Estimates of the Role of Education in Failure to Survive
17-Year NLS Older Men Panel Study, 1966-1983 (t-statistic in parentheses)

	Effects of:				
	(1)	(2)	(3)	(4)	(5)
Variable Name	Education (All)	Education (Nonblacks)	Education (Blacks)	Education, Wealth, and Permanent Earnings (All)	Education, Poverty, Living in South, Living in Small Town, Being Married, Being Black (All)
age66	.0877 (11.50)	.0852 (9.25)	.1030 (7.41)	.0853 (9.40)	.085 (9.36)
GRADEC	-.0579 (7.11)	-.0571 (5.11)	-.0080 (0.544)	-.0157 (1.404)	-.0076 (0.649)
weal66p				-2.79e-06 (3.69)	-2.28e-06 (3.11)
pearn				-1.85e-05 (2.62)	-1.45e-05 (1.91)
npov					.337 (2.00)
south					-.021 (0.250)
small town					-.237 (2.72)
dmar					-.242 (2.19)
DBLACK					.167 (1.80)
constant	-4.88 (11.80)	-4.85 (9.59)	-5.78 (7.69)	-4.93 (9.92)	-4.88 (9.31)
chi square (df)	213.26 (2)	127.6 (2)	61.38 (2)	172.98 (4)	191.8 (9)
N	4,938	3,546	1,392	3,968	3,945
Mean of kndead83	.298	.267	.378	.274	.274

Note: The term "e-05" stands for 10 taken to the minus 5 power, "e-06" 10 taken to the minus 6 power.

greatly diminished. In Column 5, the entire model, including schooling, is presented. There appears to be no independent effect of schooling on mortality risk; consequently, whatever beneficial effect it has must work through its effect on income.

The difference between the controlled and uncontrolled effects of schooling on survival deserves further examination, since it may lead us to an understanding of why mortality rates, whether controlled or not, have differed by race. One interpretation of the data in the tables is that the return to schooling measured in survival--a direct measure of "human capital"--was lower for blacks than for whites. Others have pointed out (see, e.g., Welch, 1973) that there were big differences in the quality of schooling received by blacks and whites in this birth cohort. Moreover, blacks received quantitatively much less schooling than did whites. These differences may not only manifest themselves in lifetime earnings, but in lifetimes as well.

CONCLUSION

In contrast to the Fuchs quotation that heads this paper, it appears that differential mortality rates by economic status are strongly present in the United States today. This differential can be said to be confused with the well-known racial differences in mortality. One implication of this paper is that racial differences in mortality are, in large part, a consequence of poverty or a low permanent income status, as opposed to racial genotype. Consequently, it may be just as valid, or even more so, to publish mortality tables by income as by race.

In addition, I fail to find a direct effect of schooling on mortality. Consequently, the beneficial effect of schooling on longevity must work through its effect on income, with only the latter directly influencing mortality risk.

ENDNOTES

1. These studies also show that these mortality differentials by class apply to every major cause of death (with skin cancer the only exception) as well as death by all causes.

2. The use of tobacco, if correlated with low income status, may confound an estimated differential mortality relationship. U.S. data (Congressional Budget Office, 1990), however, reveal that expenditures on tobacco are not inversely related to income; in fact, the poor spend somewhat less on tobacco than the nonpoor (page 28).

3. Originally, race was divided into "white" and "nonwhite." The small percentage of the total sample, however, that was both nonwhite <u>and</u> nonblack (1.5 percent) appeared to be far more similar, in mortality rates, to whites than to blacks.

4. The survey was not done every year, so eleven years of earnings data is the maximum number of observations.

5. I also tested whether there was a significant difference in mortality risk between those in the never-married group and those in the widowed/separated/divorced group. The never-married group had a lower mortality risk than the widowed/separated/divorced group, but the difference was not statistically significant.

6. The use of "nparal" may be disputed if indeed racial genotype is a source of longevity differences. Being black means having black parents and, if genotype matters, controlling for parents alive may be objectionable.

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