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Are Educational Differentials in Mortality Increasing in the United States?

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Abstract

Because of the value that individuals place on health and longevity, levels of mortality are among the most central indicators of social and economic well-being. Analysts are concerned not only with the average level of mortality but also with its distribution among social groups, which is a fundamental indicator of social inequality. The principal dimension on which these assessments are now made in the United States is educational attainment. The decisive shift from occupational groups, the classic dimension used by the Registrar-General of England and Wales, to educational groups as the basis for assessment occurred with the publication of Kitagawa and Hauser's (1973) major study of American mortality differentials in 1960. Educational attainment has two main advantages relative to occupation and income, the other common indicators of social stratification. It is available for people who are not in the labor force; and its value is less influenced by health problems that develop in adulthood. Since health problems can lead to both high mortality and low income, comparisons of death rates of different income groups, for example, are biased by their mutual dependence on a third variable, the extent of ill health. For these reasons, educational attainment has become the principal social variable used in epidemiology as well as in demography (Liberatos et al. 1988).

Keywords

educational attainment, mortality, Medicaid, Medicare, AFDC

Disciplines

Demography, Population, and Ecology | Family, Life Course, and Society | Inequality and Stratification | Medicine and Health | Social and Behavioral Sciences | Sociology

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Because of the value that individuals place on health and longevity, levels of mortality are among the most central indicators of social and economic well-being. Analysts are concerned not only with the average level of mortality but also with its distribution among social groups, which is a fundamental indicator of social inequality. The principal dimension on which these assessments are now made in the United States is educational attainment. The decisive shift from occupational groups, the classic dimension used by the Registrar-General of England and Wales, to educational groups as the basis for assessment occurred with the publication of Kitagawa and Hauser's (1973) major study of American mortality differentials in 1960. Educational attainment has two main advantages relative to occupation and income, the other common indicators of social stratification. It is available for people who are not in the labor force; and its value is less influenced by health problems that develop in adulthood. Since health problems can lead to both high mortality and low income, comparisons of death rates of different income groups, for example, are biased by their mutual dependence on a third variable, the extent of ill health. For these reasons, educational attainment has become the principal social variable used in epidemiology as well as in demography (Liberatos et al. 1988).

Two recent and highly publicized studies (Feldman et al. 1989; Pappas et al. 1993) have compared the extent of recent educational differential in mortality to those uncovered by Kitagawa and Hauser (1973). Both concluded that inequalities have widened since 1960 for certain population sub-groups. However, both data sources on which these studies relied have important deficiencies. In this paper, we reconsider trends in educational mortality differentials using a data set that is better suited to examining this issue, the National Longitudinal Mortality Survey. We also introduce additional measures of inequality in an effort to present a more complete picture of inequality trends. We apply these measures to a broader range of ages than used in either of the earlier studies. Finally, we consider the quality of data in the 1960 baseline constructed by Kitagawa and Hauser.

Data

Our 1979-1985 estimates of age-sex-education specific death rates are based on the National Longitudinal Mortality Survey (NLMS), a mortality follow-up study funded and directed by the National Heart, Lung, and Blood Institute. The NLMS Public Use Sample, the source of our data, is based on five Current Population Surveys (CPS), conducted between March 1979 and March 1981, and contains 637,324 individual records. Individuals enumerated in the CPS were followed up for a five-year period by linking the records to the National Death Index (NDI) for the years 1979-1985.[1] This record linkage identified 22,649 deaths that had occurred within a five-year period following the date of the CPS interview to members of the five CPS cohorts (for details on the linkage procedures, see Rogot et al., 1986).[2]

The NLMS has several advantages relative to the other two data sources used to study trends in educational differences in mortality. Feldman et al. (1989) used the National Health and Nutrition Survey Epidemiologic Followup Study (NHEFS). The comparative disadvantage of this source is simply one of size; only 14,407 persons were followed up, about 2% of those in the public use file from NLMS. Its small size restricted the authors' attention to those ages (55 and above) where deaths are most numerous. We show below that the data from this source are consistent with data from the NLMS, although its standard errors are higher.

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[1] The NLMS Public Use Data File is a subset of the larger NLMS database consisting of 12 census samples numbering about 1.3 million persons in the United States. Eleven of the 12 cohorts were taken from Current Population Surveys conducted during the period from March 1973 through March 1985 with one sample drawn from the 1980 Census of Population. Sample individuals were then matched to the National Death Index (NDI) beginning in 1979, when the NDI was established, with plans to continue mortality follow-up through 1993 (Rogot et al., 1992b).

[2] The five CPS surveys were conducted in March 1979, April, August and December 1980, and March 1981. For all sample members follow-up in days is included in the Public Use Data File and all individuals who were not linked to the NDI, and thus considered to be alive at the end of the follow-up, are given a follow-up period of 1827 days (five years). We should note, however, that the March 1981 CPS cohort was followed only through the end of 1985, or approximately four years and nine and a half months. We cannot distinguish which sample individuals belong to this cohort and must accept a five-year follow-up period for them as well (National Institutes of Health, 1992). In addition, the lack of perfect ascertainment of death in the NDI results in some deaths being missed. Rogot et al. (1992, p.2) suggest that "there is some ascertainment loss, of perhaps 5%, occurring in the matching process because of recording errors in the files being matched."

The second data source, used by Pappas et al. (1993), is a combination of two sources: deaths were drawn from the 1986 National Mortality Followback Survey (N=13,491) and populations at risk were drawn from the 1986 National Health Interview Survey (N=30,725). While this source also has fewer observations than NLMS, its principal disadvantage is that characteristics of the dead and the living are drawn from two different sources. Earlier investigators have revealed large and systematic discrepancies between characteristics for the same individuals reported on death records and census records (e.g., National Center for Health Statistics, 1968, 1969), raising the possibility of serious biases in death rates constructed from dual sources. A similar follow-back survey in 1962-63 revealed a quite different pattern of educational differentials from those applying only two years earlier in a linked data file in which all characteristics were drawn from the same data source (Kitagawa and Hauser 1973:32). In particular, educational differentials were substantially larger for males in the follow-back survey than in the linked data file. Thus, the conclusions about trends that were reached by Pappas et al. (1993) may simply reflect the circumstance that the two observations used to establish trends were not comparable. If they had used the comparable sources available to them, the widening of educational differentials for males that they demonstrate would have been attenuated.

An additional problem with the Pappas et al. data source is that the deaths include those to persons living in institutions while the denominators do not include such persons, forcing the authors to restrict attention to persons below age 65 and to make a somewhat arbitrary adjustment for deaths in institutions below that age.[3] The NLMS and the NHEFS also excluded persons who were in institutions at the beginning of the follow-up period (although if they later entered institutions they would continue to be

[3] In the age range 25-64, the authors excluded deaths of persons in the military and persons who lived in institutions for more than half of the year (Pappas et al. 1993:104). Another recent study (Rogers, 1992) also used the 1986 National Health Interview Survey and the 1986 National Mortality Follow-Back Survey to examine socioeconomic mortality differentials in the United States. Education was not included as an explanatory variable in this analysis and procedures different from those in Pappas et al. (1993) were used to adjust for deaths in institutions, exemplifying the problems posed by dual data sources.

followed up); however, by the nature of the linked design, persons in institutions at baseline were excluded from both numerator and denominator. Because of the partial or complete exclusion of persons in institutions, death rates in all three sources are below the national level recorded in vital statistics.

Like the two previous studies, we also base our 1960 estimates on the Kitagawa and Hauser study (1973) that matched death certificates registered in May-August, 1960 to the 1960 Census of Population taken on April 1. Altogether, 340,033 death certificates were included in the study and searched in the 1960 census records. The authors published both standardized mortality ratios, based on an indirect adjustment for age, and age-specific death rates by educational attainment. In this paper, we base our trend estimates on the age-education specific death rates for white men and women presented in Kitagawa and Hauser (1973:Table 2.8).

The Magnitude of Educational Differentials in Mortality from Various Sources

Confidence in the reliability of measured educational differentials in mortality would clearly be increased if multiple sources for the same period yielded approximately the same pattern of differentials. Unfortunately, as Preston and Taubman (1994) note, the educational differentials in mortality published from NLMS (Rogot et al., 1992) appear to be substantially smaller than those based on the NHEFS data used by Feldman et al. (1989), despite the fact that the periods to which the rates pertain are roughly similar. This apparent inconsistency creates uncertainty about the magnitude of recent educational differentials.

The inconsistency is demonstrated in columns 2 and 3 of Table 1. Column 3 presents the index numbers of mortality differentials based on data published in the second NLMS data book for white males and females (Rogot et al. 1992: Table 6). The variable being indexed is the standardized mortality ratio

(SMR), defined as the ratio of actual to expected deaths.[4] Expected deaths are derived by multiplying the number of persons at the start of the follow-up with a given level of education in a given age-race-sex group by the overall probability of dying during the follow-up period for the entire age-race-sex group.[5] Expected deaths are calculated separately for each cohort in 5-year age groups and then summed. Column 1 presents the white age-specific death rates based on the NHEFS from Feldman et al. (1989) and column 2 converts these rates to index numbers, with the mortality level of the highest education group assigned a value of 100.[6] Evidently, the differentials are substantially larger in the NHEFS than in the NLMS.

However, different indexes are being compared and age is treated differently in the two columns. Both of these differences are expected to reduce educational differentials in the NLMS column relative to those in the NHEFS. A certain proportionate difference in age-specific death rates will always translate into a smaller proportionate difference in proportions dying.[7] And because educational differentials in

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[4] To obtain comparable educational groupings from the NLMS data book to those published by Feldman et al. (1989) we summed the observed and expected deaths within the given age range over the more detailed educational categories shown and recalculated the SMR.

[5] The follow-up period for each of the cohorts differed and ranged from 7 years to .79 years, averaging 4.8 years (Rogot et al. 1992b, Table A).

[6] Feldman et al. (1989) present age- and sex-specific annual death rates by educational level for white males and females in three age (at death) groups based on data for 2,984 white men and 3,414 white women who were aged 55-84 years at any time during the follow-up period. Death rates for age-sex-education specific subgroups were estimated by dividing the number of deaths by person-years at risk of death within each subgroup.

[7] The proof of this proposition is the following. Define $\mu(a)$ as the force of mortality at age a, i.e., the age-specific death rate in the age interval from a to $a+da$ as $da \to 0$. The probability of dying in the discrete age interval x to $x+n$, ${}_nq_x$, is then

$$ {}_nq_x \; = \; \int_x^{x+n} p(a)\,\mu(a)\,da\,/\,p(x), $$

where $p(x)$ is the probability of surviving to age x. If $\mu(a)$ is multiplied by K at all ages in the interval x to $x+n$, then the new probability of dying will be

$$ {}_nq_x^{*} \; = \; \int_x^{x+n} p^{\,K}(a) * K\mu(a)\,da\,/\,p(x). $$

Thus, the ratio of the new probability of dying to the old will be mortality generally decline with age, the fact that ages refer to the age at exposure in NHEFS and to age at the outset of exposure in NLMS will cause a contraction of differentials in the latter relative to the former. Finally, published NLMS estimates of educational differentials in mortality are also affected by the assumptions made concerning deaths that occurred before the establishment of the NDI in 1979 to members of the March 1973 and February 1978 CPS samples. In developing the denominators for NLMS rates, no allowance was made for educational (or other) differentials in mortality before 1979 (Rogot et al., 1992, Appendix). Since such differentials were likely to be present, the effect of the assumption is to reduce the size of estimated educational differentials after 1979.

In order to correct for these and other differences in approaches between the two sources, we have reanalyzed the NLMS data, using the public use sample (which contains a subset of all NLMS data) to calculate death rates by age at death in categories comparable to those presented in Feldman et al. (1989).[8] Results are shown in columns 4 and 5 of Table 1. It is clear from the index numbers in column 4 that the size of educational differentials in mortality becomes comparable in NLMS and NHEFS when similar procedures are used.[9] The mean index number for groups with less than the highest level of schooling

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$$\frac{{}_nq_x^*}{{}_nq_x} = K\left[\frac{\int_x^{x+n} p^K(a)\,\mu(a)\,da}{\int_x^{x+n} p(a)\,\mu(a)\,da}\right]$$

Since $p(a) < 1$ at some or all ages between x and $x+n$, the term in brackets will be less than one and the proportionate difference between probabilities of dying will be less than K, the proportionate difference in age-specific death rates.

[8] Our estimates of age-sex-education specific death rates are constructed by dividing the number of deaths within each age-sex-education subgroup by the person-years at risk of death within each subgroup, where age refers to age at death, and are based on 64,170 white men and 77,394 white women who were aged 50-84 at the initial interview and who thus could contribute either deaths or exposure or both to the age intervals in question during the five-year followup period. Individuals for whom education was missing are excluded from our calculations (N=281). The estimates presented are based on unweighted cases; the use of weights does not alter our conclusions.

[9] We have not age standardized the NLMS rates to be comparable with the estimates presented by Feldman et al. (1989) because we do not know the age distribution of the person-years lived in the NHEFS. Differences

across all age-sex groups in Table 1 is 137 for NHEFS. When published NLMS data are used, it is only 123. But when we impose NHEFS criteria on NLMS, the mean becomes 135. Nearly all of the disparity between the sources has been eliminated.

The actual estimates of the age-sex specific death rates based on the NHEFS and the NLMS are not identical. In 13 out of 20 estimates shown, estimates based on the NHEFS are higher than those based on the NLMS. Given the longer follow-up period for the NHEFS (an average of 10 years) than for the NLMS (5 years), the NHEFS results are likely to capture more of the mortality experience of the institutionalized and thus one would expect death rates to be higher based on the NHEFS than on the NLMS. The former data also refer to a time period (1971-84) that is, on average, four years earlier than the latter (1979-1985), and since adult mortality was declining during this period, some of the small discrepancy between the sources is likely to reflect mortality trends. Nevertheless, among the 20 age-sex-education groups shown in Table 1, in only one case (males aged 65-74 at the lowest educational level) do death rates from the two sources differ significantly from one another at a 5% level of significance.[10] This is the extent of disagreement expected by chance alone. We conclude that evidence on the extent of recent educational differentials in mortality from these two independent sources is consistent and that they help to confirm one another's reliability.

Such is not the case when we consider the death rates constructed by Pappas et al. (1993). They present age-adjusted death rates (using the entire U.S. Population in 1940 as a standard) in 1986 among persons 25 through 64 years of age in selected education groups by age and sex (Pappas et al. 1993: Table 1). In Table 2 we have compared these rates with those obtained from the NLMS; the death rates based

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in age distribution between the two sources could lead to minor differences between the rates.

[10] Statistical tests to determine whether death rates based on the NHEFS and the NLMS are statistically different from one another are based on the following formula: $(DR_{nhefs} - DR_{nlms})/[(var_{nhefs} + var_{nlms})]^{1/2}$.

on the NLMS are also age-adjusted (within five-year age groups) using the same standard.[11] Estimates based on the NLMS show a somewhat lower gradient in mortality by educational attainment than those obtained by Pappas et al. using separate sources for numerators and denominators. In particular, the NLMS-based death rates are somewhat lower for individuals with 0-11 years of schooling and somewhat higher for the most educated group than are the estimates by Pappas et al.. The result is that the ratio of death rates in the extreme educational categories for males is 2.7:1 in Pappas et al., compared to only 2.0:1 in NLMS. For females, the discrepancies are in the same direction but smaller: 1.9:1 in Pappas et al. vs. 1.5:1 in NLMS. The combination of two sources used by Pappas et al. (1993) appears to have biased upwards the estimated extent of educational inequality in mortality.

Measures

The ratio of death rates among extreme groups is a very crude measure of inequality, since it takes no account of death rates in other groups nor of the relative size of the groups. In this section, we describe better measures of inequality. When applied to data from the NLMS and from the Kitagawa and Hauser study, they will be used to reassess trends in educational mortality differentials.

1. Slope Index of Inequality

The Slope Index of Inequality (SII) is an estimate of how much change in death rates is associated with moving up the educational ladder (Preston, Haines, and Pamuk, 1981). In particular, it is an estimate of how much absolute decline in death rates occurs from the lowest educational level (the 0th percentile) to the highest (100th percentile). To construct the SII, we arrange the education groups from the fewest to the most years of school completed on a horizontal axis and compute each education group's range in the cumulative proportionate distribution of the population from the lowest to the highest levels of schooling (that is, from 0 to 100 in the percentile education distribution). We then plot each group's age-

[11] Age-sex-education specific death rates were obtained in a manner similar to that described in footnote 9.

standardized death rate at the mid-point of its range. For each sex/education group, we compute an age-standardized death rates by weighting the age-sex-education specific death rates in NLMS by a standard age distribution. To facilitate comparisons with the Kitagawa-Hauser results for 1960, we used the 1960 age distribution for white persons as the standard in the age interval 25-64. Separate sex-specific standards are used. In the age interval 65-74, the sex-education specific death rates are not age-standardized.[12]

We then obtain the SII, the relation between the death rates and location on the cumulative education distribution, by weighted least squares regression, where weights are the proportions in each education category. An equation is fitted for both 1960 and 1979-1985, separately for males and females. By allowing a particular educational group to change its location in the percentile distribution under this procedure, the method takes account of changes in the educational distribution over time; a group's death rate is always "located" at its midpoint in the percentile distribution of educational groups.

To construct the SSI for 1960, we use age-sex-education specific death rates published in Kitagawa and Hauser (1973, Table 2.8). For the age interval 25-64, death rates are given by ten-year age groups for the following educational categories: less than 8 years, 8 years, high school: 1-3 years, 4 years, and college: 1 year or more. For the age interval 65-74, death rates are given for the following educational groups: less than 8 years, 8 years, high school: 1-4 years, and college: 1 year or more.[13]

2. Relative Index of Inequality

The slope index of inequality measures the absolute expected change in death rates in moving from

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[12] Kitagawa and Hauser (1973) published age-sex-education specific death rates in ten-year age intervals.

[13] To test whether our results are sensitive to the way in which education is categorized in 1979-1985, we reanalyzed the data with an alternative classification of the population by educational attainment in the age interval 25-64. Here we follow Pappas et al. (1993) who classified educational attainment in 1986 as follows: 0-11 years of school, 12 years, college: 1-3 years and 4 or more years. Results were scarcely altered: the value of SSI shown in Table 4 at ages 25-64 in 1980-85 was higher by .03 for males and .01 for females using this alternative grouping. This slight change is not large enough to affect any interpretations regarding trends.

the lowest to the highest levels of schooling. Many measures of inequality are based on relative, rather than absolute, differences. The slope index of inequality can be converted into a relative measure by simply dividing its value by the death rate for all groups combined. The result, which we will refer to as the relative index of inequality, is equivalent to the slope of the log of death rates when plotted on the cumulative educational distribution scale. It indicates the mean proportionate decline in mortality when educational levels advance from the lowest to the highest.

Confusing the issue of relative versus absolute measures is the fact that the ratio of survival probabilities between two groups, which is a relative rather than an absolute measure of survival, is a function of the difference between their death rates, an absolute measure of mortality (Keyfitz, 1968). There is no obviously best measure of social class differences in mortality. Hence, we use several in this paper.

3. Index of Dissimilarity

The third measure of inequality that we employ is the Index of Dissimilarity (ID), which is widely used in the social sciences as a summary measure of the difference between two distributions. In this case, the two distributions to be compared are the distributions of deaths and of populations at risk of dying by educational attainment. The value of ID is calculated as the sum of the absolute differences between proportions of total deaths and of population in each education group (and dividing by two so that the range of all possible values extends from 0 to 1). Its value can be interpreted as the minimum proportion of deaths that would have to be redistributed in order to equalize the distributions of deaths and population, thereby eliminating all educational differences in mortality. An age-standardized number of deaths for each education group is obtained by multiplying the age-standardized death rate for each education category by the number of persons in that educational category. For a particular sex, the 1960 age distribution for whites is used as the standard for every educational category in both 1960 and 1983. The distribution of deaths is then compared with the distribution of the population by educational attainment to yield the value of ID.

ID is a relative index of inequality, since its value would not change if all death rates were multiplied by a scalar. Unlike the Relative Index of Inequality, the value of ID does not depend on the ordering of death rates by educational group. It would have the same value regardless of what educational labels were attached to an observed set of deaths rates and populations at risk. A closely related index of inequality is the Gini coefficient, usually employed in studies of income inequality. It is also a relative measure that does not attempt to preserve the ordering of educational groups. We have also used the Gini coefficient in this study, but results were so similar to those for the Index of Dissimilarity that we will not present them here.

Results

The basic results of our analyses are presented in Table 3, which shows the value of each of our three measures for men and women aged 25-64 and 65-74 in both 1960 and 1979-85. Our results are consistent with previous findings that show a widening of educational differentials in mortality for white men. In both age intervals, each of the three measures indicates that educational inequality in mortality was greater in 1979-85 than in 1960. Although the data source used by Pappas et al.(1993) appears to exaggerate the extent of recent inequality at ages 25-64, the use of more reliable data does not reverse the direction of change that they describe.

For white females, however, our results contradict those of Pappas et al. Both the absolute and relative measures of inequality at ages 25-64 suggest that educational differentials in mortality have narrowed rather than expanded for white women since 1960. Although they use a slightly different educational classification than the one used here, we have shown that this difference cannot account for the disparity in results. Most likely, it is attributable to biases in their measures that result from using different data sources for deaths and for populations at risk.[14]

For women aged 65-74, our results are more mixed. The absolute measure of inequality declines, while both relative measures increase. Being at the low end of the educational distribution in 1979-85 was associated with a smaller absolute penalty in death rates than in 1960, but with a larger relative penalty. Such an outcome is possible only when overall death rates have fallen, which is clearly the case for this group in Table 3. The widening of relative inequality has, however, been substantially less for older women than for older men. For both sexes, our results at ages 65-74 provide strong confirmation, using a much larger dataset and more elaborate measures, of the general tendencies described by Feldman et al.(1989).

By far the largest increase in inequality on any measure between 1960 and 1979-85 occurred for men aged 65-74. There are reasons to be cautious about the huge increases in inequality for men in this age group. The complete absence of differentials for white men aged 65+ in 1960 in the Kitagawa-Hauser study appears quite curious in view of the many economic and social advantages enjoyed by those with better education: mortality indexes for white men 65+ with <8, 8, 9-12, and 13+ years of schooling are 1.02, 1.00, .98, and 1.00 respectively (Kitagawa and Hauser, 1973, p. 27). Nonwhite men and women above age 64 also fail to exhibit an educational gradient in mortality in 1960. These results are not readily reconciled with indicators of disability. Disability rates showed sharp differentials in 1960-61 by

[14] Two other factors may have caused the Pappas results to overstate the increase in equality. First, rather than using the same age distribution at both time points, they apparently used the 1960 age distribution to develop their inequality measure in 1960 and the 1986 age distribution to develop their inequality measure in 1986 (Pappas et al. 1993:104). Since educational differentials in mortality diminish with age (see text; Feldman et al. 1989), and the 1986 age distribution of 25-64 year olds is much younger than that in 1960 thanks to swollen baby boom cohorts aged 25-44 in 1986, the use of different age distributions may have exaggerated the increase in inequality. Second, their 1960 data point for white women aged 25-64 with the lowest educational attainment, 0-4 years, appears to be a transcription error. According to Kitagawa and Hauser (1973:12), the mortality ratio for this group should have been 1.60; on the graph supplied by Pappas et al. (1993:104), this point is plotted at a level of about 1.15-1.20. Since this group is at one extreme of the education distribution, its value has a large impact on the index of inequality.

socioeconomic status, as shown in Table 4.[15] Differentials of roughly 2.5:1 in restricted activity days and bed disability days between the highest and lowest income classes make it seem less plausible that differences in mortality by educational attainment would be completely absent. As noted above, a follow-back survey of decedents in 1962-63 showed much larger differentials by education for men above age 64 than did the Kitagawa-Hauser study, although this source is hardly definitive.

If a problem exists with the reliability of Kitagawa/Hauser results for older men, it is likely to be a product of a relatively high non-match rate in their study. An appropriate census form could be located for only 74.7% of the deaths (Ibid., p. 187). While a small sample of non-matched cases was followed up, an educational level was reported for only 72.3% of these cases (Ibid., 189-190). Furthermore, when educational level was reported (typically by relatives of the decedent) they were likely to be in error (i.e., different from what was recorded in the census). The extent of this problem is indicated by the results of using the same follow-up questionnaire among a sample of matched cases: among white men aged 65+, years of schooling were the same in the census and the follow-up for only 128 out of 231 matched cases, or 55% (Ibid., p. 206). It is impossible to know whether imputations were accurate in the Kitagawa/Hauser study, but the high non-match rate raises the possibility that serious error could have crept in.

The most telling indication that educational differentials probably did widen between 1960 and the 1979-85 period for this group is that the amount of coding error in the 1960 study that would be required to avert a widening is implausibly large. For example, if all of the unmatched deaths among white males aged 65+ that were assigned to "college 1+" in the Kitagawa-Hauser study were instead assigned to "0-7 years of schooling", it would lower the death rate of the "college 1+" group by 17.8% and raise that of the "0-7" group by 3.6% (compiled from Kitagawa and Hauser, 1973, p. 203). The resulting ratio of death

[15] No tabulations by education areavailable from the National Health Survey for this period. For males aged 65+, income differentials in mortality were, as for education, small in the Kitagawa-Hauser study; a difference of less than 14% was reported between the lowest (<$2000) and highest (>=$8000) income categories for whites (Kitagawa and Hauser, 1973, p.18). It should be noted that educational differentials in disability are much larger than educational differentials in mortality for more recent periods as well (Preston and Taubman, 1994).

rates of the two groups would still be well below the ratios from the NLMS shown in Table 1. Even assigning all of the unmatched deaths to the 0-7 category would raise its death rate by only 20.4%, not enough to avert an increase in differentials between 1960 and 1979-85. We conclude that educational differentials in mortality among older white males have almost certainly increased, although the magnitude of the increase is uncertain.

Discussion

This study confirms two previous studies that found educational differentials in mortality to have increased for males since 1960. The data set on which our conclusion is based appears to provide a more reliable basis for this claim and to produce more precise estimates of the changes. Feldman et al. (1989) have a useful discussion of the factors that may have been responsible for this widening. They note that trends in heart disease mortality are responsible for most of the change in inequality from all causes combined. Among the factors they consider are changes in the educational distribution, which we have dealt with more directly here and shown not to have accounted for the trend; cohort-specific factors that reflect changing childhood environments; changes in the distribution of cigarette smoking by educational group, which clearly does contribute to the trend but quantitatively is not of major importance; improvements in medical and surgical treatment of heart disease that may have diffused more rapidly among better educated people; and the possibility that better educated people in 1960 were actually harmed by the medical practices to which they had better access. They conclude that reasons for the changes in educational mortality differentials are not easily explained and are likely to be multifactorial. Pappas et al. (1993) add changes in the income distribution and in access to a broad range of health care to the list of factors that need to be considered.

Whatever set of factors is ultimately implicated, some or all of them must be highly differentiated by sex. We have shown that inequality actually declined among women aged 25-64 and was basically stationary for women aged 65-74. The latter result is consistent with that of Feldman et al. (1989); the former result contradicts that of Pappas et al. (1993). We can think of two factors that may have impinged on women in a way that differentiated their inequality trends from those of men. First, the period 1960-1985 witnessed a huge increase in labor force participation rates among women. For example, the labor force participation rate of married women aged 35-44 increased from 36.2% in 1960 to 68.1% in 1985 (U.S. Census Bureau 1990:384). This movement of women into the labor force provided them with a source of income that was independent of that of their husbands. At the same time, women's earnings were gaining on those of men (Goldin, 1990). These changes may have enhanced women's economic security and reduced the prevalence of stress-related, as well as poverty-related, diseases. However, the increase in labor force participation was actually larger for better educated women, so that this factor may not have induced a contraction in mortality differentials unless the changes were especially beneficial for poorer women.

A second factor that may have disproportionately benefited women, especially poorer women, is Medicaid. This program, enacted in 1965, provides heavily-subsidized health care for the poor. Eligibility for Aid to Families with Dependent Children (AFDC), a welfare program for single-parent households, confers automatic eligibility for Medicaid. Women are more likely to qualify for AFDC than men, and in fiscal year 1983, 69.3% of Medicaid recipients qualified for Medicaid through AFDC. The result is that, of all Medicaid recipients that year (including children), 64.1% were female (U.S. Department of Health and Human Services, 1987).

However, the role of health insurance and health care access is called into question by the age pattern of changes in health inequalities. For both men and women, inequality trends over the period 1960-1985 were more adverse for persons aged 65+ than for younger persons. Since Medicare was passed in 1965, all persons aged 65+ have been entitled to basic health care at little, if any, cost. If access to health care were a major factor in mortality differentials, one would have expected some contraction in differentials above age 64 relative to changes among younger persons. The American experience with Medicare resembles the British experience with the National Health Service. After this Service was introduced in 1946, inequalities in mortality widened (Pamuk 1985). Nevertheless, both Medicare and the National Health Service provided universal coverage, whereas Medicaid has been targeted specifically to the most disadvantaged groups. Its role in mortality levels and inequalities, especially those of women, deserves further investigation.

Our purpose in this paper is not to supply an explanation of the various trends in inequality but to establish these trends with greater certainty. We find that the picture is far more mixed than the unremittingly grim picture that has recently been painted. While inequality in death rates has clearly risen for men since 1960, it has fallen among women aged 25-64 and remained approximately stationary for women aged 65-74. For each sex, inequality trends are more adverse for older persons. Whatever explanations are ultimately offered for these changes, and whatever policy inferences are drawn, will have to attend to the variegated pattern that we have described.

Table 1: Comparison of Educational Differentials in Mortality Based on the NHEFS 1971-84 and the NLMS 1979-1985, White Males and Females 55-84

Age and Education	Death rates per 1,000 Feldman et al. 1971-84	Mortality Ratio Index: Feldman et al. 1971-84	Mortality Ratio Index: NLMS Source Book 1979-85	Mortality Ratio Index: NLMS Public Use File 1979-85	Death Rates per 1,000 NLMS Public Use File 1979-85
			MALES		
55 - 64					
0-11 years	17.9 (2.2)	153	137	151	18.9 (0.02)
12+ years	11.7 (1.7)	100	100	100	12.5 (0.01)
65 - 74					
0-7 years	55.4 (4.7)	195	134	157	42.3 (0.06)
8 years	41.8 (4.4)	147	135	151	40.8 (0.05)
9-12 years	37.7 (3.4)	133	120	134	36.3 (0.03)
13+ years	28.4 (4.2)	100	100	100	26.9 (0.04)
75 - 84					
0-7 years	87.3 (7.8)	138	117	136	85.6 (0.10)
8 years	74.4 (8.0)	118	120	134	84.1 (0.09)
9-12 years	70.8 (7.4)	112	110	115	72.1 (0.07)
13+ years	63.2 (9.6)	100	100	100	62.8 (0.09)
			FEMALES		
55 - 64					
0-11 years	8.4 (1.5)	140	124	128	9.2 (0.01)
12+ years	6.0 (1.1)	100	100	100	7.2 (0.01)
65 - 74					
0-7 years	26.6 (3.4)	182	138	163	23.6 (0.04)
8 years	21.0 (2.9)	144	131	136	19.7 (0.03)
9-12 years	18.0 (1.9)	123	124	117	17.0 (0.02)
13+ years	14.6 (2.7)	100	100	100	14.5 (0.03)
75 - 84					
0-7 years	49.7 (5.7)	119	112	128	44.3 (0.06)
8 years	50.4 (5.9)	121	114	117	40.6 (0.05)
9-12 years	37.9 (4.1)	91	110	120	41.3 (0.04)
13+ years	41.7 (6.4)	100	100	100	34.5 (0.05)

Standard errors of the estimates are given in parentheses. Variance estimates from the NLMS are based on the following formula: $\hat{m}_i/A_i$; where $\hat{m}_i$ is the estimated death rate and A_i is the aggregate exposure time, under the assumptions that the death rate remains time invariant during the observation period and is shared by all segments of the i^{th} stratum (Namboodiri, 1991, p.65).

Source: Feldman et al. (1989), Table 2; adapted from Rogot et al. (1992), Table 6; calculations by the authors from the NLMS public use file.

Table 2: Age-Adjusted Death Rates in 1979-85 and 1986 among Persons Age 25-64 in Selected Education Groups, White Males and White Females (Deaths per 1,000)

Educational attainment	White Males		White Females	
	NLMS	Pappas et al.	NLMS	Pappas et al.
School				
0-11 years	6.3	7.6	2.9	3.4
12 years	4.5	4.3	2.2	2.5
College				
1-3 years	4.3	4.3	2.3	2.1
4+ years	3.1	2.8	1.9	1.8

Pappas et al. estimates are age-adjusted death rates calculated from age-specific death rates with the entire U.S. Population in 1940 used as the standard, based on data from the 1986 Mortality Followback Survey and the 1986 National Health Interview Survey. Age-adjusted death rates from the NLMS are calculated from age-specific death rates by five-year age groups with the entire U.S. population in 1940 used as the standard.

Source: Pappas et al. (1993), Table 1.

Table 3: Educational Inequality in Mortality, United States 1960 and 1979-85 White Males and Females

Sex & Year	Age standardized Death Rate (per 100)	Slope Index[1] of Inequality (SSI)	SSI/ Death rate	Index of Dissimilarity
		25-64 Years		
Males				
1960	0.81	-0.39	-0.48	0.060
1980-85	0.51	-0.41	-0.80	0.090
Females				
1960	0.41	-0.23	-0.56	0.076
1980-85	0.27	-0.11	-0.41	0.050
		65-74 Years		
Males				
1960	4.89	-0.66	-0.13	0.016
1980-85	3.66	-1.93	-0.53	0.058
Females				
1960	2.86	-1.25	-0.44	0.051
1980-85	1.87	-0.98	-0.52	0.059

[1] The Slope Index of Inequality presented here is multiplied by 100 and shows the average decline in standardized death rate as one moves from the lowest to the highest levels of schooling, that is, from 0 to 1 in the proportionate education distribution. Note that the age-standardized death rates in this table are also given per 100.

Table 4: Differences in Disability Rates by Family Income for Males Aged 65+, 1960-61

Annual Family Income ($)	Number of Restricted Activity Days Per Person Per Year	Number of Bed-Disability Days Per Person Per Year
<$2000	50.7	17.9
2000-3999	40.8	12.6
4000-6999	32.7	10.2
$7000+	21.6	6.4

Source: National Center for Health Statistics (1963).

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