

DISCUSSION

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The problem discussed by Dr. Moriyama has concerned health authorities now for a decade. We had become so used to declining death rates that resistance to further decreases is difficult to accept. We must keep in mind that man is mortal, that the population will continue to have increasing numbers of old people and that dramatic new therapies have not appeared in the sixties. Nevertheless, as Moriyama has pointed out, such explanations are inadequate, since lower age-specific mortality rates do exist in other countries. The white-nonwhite differentials in the United States are alone sufficient evidence that improvement in the gross rates must be possible.

There is little I can add to Moriyama's discussion, but the change in rate of decline he notes does seem most evident (Figure 2 A) among white males at ages 15-24 and 25-34 and to have occurred earlier than at other ages--about 1948, I should judge from the chart. This date is immediately after World War II with its high peak of mortality at these ages. Could these war losses, affecting the healthiest young men, have been, in part at least, responsible for the earlier change in trend at these ages--and also for the later stabilization (about 1955) at ages 35-44, as this younger cohort grew older? Perhaps some birth cohort analyses might shed light on this point. However, the same course is not visible among nonwhite males and could not be expected among females of either age group. Nor, if true, can it explain the later stabilization at younger or older ages.

I wonder if our emphasis on age or cause-of-death information will lead us to solutions of the apparent enigma in the course of U. S. mortality rates. Let us consider infant mortality, for example. Is it not possible that social factors that do not necessarily reveal themselves directly in cause-of-death statistics are the important influences? Marriage has been occurring at younger ages; divorce rates have been high; the proportion of working mothers has increased. Hence satisfactory family life has been disturbed. What about the possible effect of determined efforts to maintain the life of poor risk infants (i.e., those of low birthweight and/or short gestation)? Studies of their later progress are few and results are not uniformly clear that they do as well as mature infants.

We pride ourselves on the levels of inoculation of infants against infectious diseases in most areas and among most population subgroups. But is there less concern for babies, if not neglect, in the disruptions of family life, in the striving for financial success, in the efforts to provide the so-called benefits of suburban living? It seems to me we may need more research into the sociology of mortality rather than the medical aspects.

Moriyama also raises a question about the change in rate of decline of pneumonia mortality during the past decade. To some extent I sus-

pect this may reflect an artifact of reporting, but reporting changes would not alter the lack of change in crude rates; they would only magnify upward trends for chronic diseases. There appears to be need for bacteriologic work that has been neglected because of availability of broad-spectrum antibiotics. However, as with infant mortality, I believe we have to seek other factors that are not immediately evident no matter how closely we look at age- or cause-specific mortality. Here again, I urge study of the sociology of mortality.

We have become an effete society. I recall an old Harold Lloyd film where his chauffeur simply made a U-turn to drive him to visit his fiancée who lived directly across the street. The scene would not be as hilarious nowadays when the most muscular, agile adult apparently cannot walk a block or two to get a pack of cigarettes or his bottle of bourbon. If home, he has to take the car. On arrival at his destination, he must double park lest he have to walk twenty paces from the empty parking space a short distance away. It is an appalling thought that the 16-year old might take a bus (or even, horrible dictu, walk) to school. Lack of exercise, poor diet (*largesse de richesse*), smoking, excess alcohol, possible effects of air pollution or food additives; there appear to be many possibilities of social (and environmental) factors that only after many years are showing their effects in terms of mortality.

These considerations are pointed up in reviewing the paper by Puffer and Griffith based on their work with their several collaborators. The paper indicates that differences in diagnosis or classification are not the explanation of international intercity variations in mortality of substantial magnitude. The authors have described these intercity variations in gross, by sex, by age and by cause of death and have also delineated the patterns peculiar to each city. They are to be congratulated, with their colleagues, on this accomplishment. Perhaps the most important contribution, from my point of view at this time, is the relatively considerable proportion of the mortality rates in these cities that is contributed by conditions associated with alcoholism, by tuberculosis and by external causes.

The exclusion of these conditions among males at 15-34 years of age (Table 3) reduces the rates to a range of 37.2 (San Francisco) to 80.3 (Guatemala City) instead of 104.8 (Bristol) to 275.3 (Guatemala City). There is not much difference in the ratios of these high rates to the lows (2.2 and 2.6). But now the major contribution rests in the residual, "all other causes". At 35-54, however, the same exclusions reduce the rates to a range of 336.0 (Guatemala City) to 468.6 (Santiago), with a ratio between high and low of 1.4 instead of 2.6 (between Bristol and Santiago).

Again, it seems to me that social action is indicated. It is obvious with tuberculosis, but alcoholism, accidents, suicides, homicides are also social problems. These observations, too, confirm my opinion that the sociology of mortality needs to be studied.

It is perhaps particularly fitting that these two papers are read as a pair--and before the Social Statistics Section of ASA. Until

medicine discovers or devises major advances in cancer and heart disease prevention and/or cure, influencing the social body seems to provide the greatest promise for further reduction in death rates. I also pose the question whether the present stagnation in mortality rates in the United States has a social rather than a purely medical component underlying it.

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EVALUATION OF 1960 CENSUS COUNT

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