

Table 1. Major causes of higher mortality in men

Ratio of male to female death rates	Cause of death	Male death rate (deaths 100,000 population)	Female death rate <sup>1</sup>
5.9	Malignant neoplasm of respiratory system, not specified as secondary	50.1	8.5
4.9	Other bronchopulmonic disease (71 percent emphysema)	24.4	5.0
2.8	Motor vehicle accidents	39.4	14.2
2.7	Suicide	15.7	5.8
2.4	Other accidents	41.1	17.4
2.0	Cirrhosis of liver	18.5	9.1
2.0	Arteriosclerotic heart disease, including coronary disease	357.0	175.6
1.6	All causes	1081.7	657.0

This table lists all causes of death which had a sex mortality ratio of 2.0 or more and were responsible for at least 1 percent of all deaths in the United States in 1967. These causes of death are responsible for three-quarters of the sex differential in mortality.

<sup>1</sup> Female death rates have been age-adjusted using the age-specific death rates for females and the age distribution for males to calculate the death rate which would be expected for a population of females that had the same age distribution as the male population. Thus, the male and female death rates are directly comparable and are not affected by the higher proportion of females at older ages.

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The second criticism, concerning differential nonresponse, is based in part on the assertion that we “essentially considered” all nonrespondents to be smokers. This assertion is erroneous. We assumed that the percentage of nonsmokers among nonrespondents was the same as among respondents. In order to test the effect of this assumption, we performed a sensitivity analysis; we recalculated the male life table, assuming that nonsmoking among nonrespondent males was actually double that of respondent males. The life table results were not substantially different, indicating that the possibility of differential nonresponse does not lessen confidence in our conclusion.

More broadly, Feinleib and Luoto cite as exemplary Waldron’s 1976 review of the literature (2), which concluded that “very roughly” one-third of the difference between male and female death rates may be due to men’s cigarette smoking, one-sixth to a greater prevalence of coronary-prone behavior pattern, one-twelfth to higher alcohol consumption (increased accidents and cirrhosis), and one-twelfth to physical hazards related to employment (increased accidents and lung cancer). We reproduce Waldron’s table 1 listing causes of death accounting for three-fourths of all male-female death rate differences.

With external causes removed, the male-female longevity difference is dominated by smoking-related diseases: respiratory cancer, pulmonary obstructive disease, and heart disease. The issue is what proportion of the male deficit in life expectancy is owed to cigarette smoking. We have pointed out the relative inaccuracy of the classification procedures which many studies, including those based on National Center for Health Statistics data (3,4), have used to assign smoking categories. These procedures place some smokers in the nonsmoking category and vice versa, which reduces the estimated mortality difference due to smoking. It is no surprise that the strongest

positive finding in the massive MRFIT study (5) was the reduction in mortality risk incident to proven sustained abstinence from smoking.

We encourage other researchers interested in longevity differences and related epidemiological matters to take greater care to assure accurate data and use appropriate smoking categories. When this is done, we expect that their results will be very similar to those we obtained.

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## Knowledge of Health Does Not Always Begin With Disease, Dr. Krause

Bravo to Dr. Edward Brandt for his spirited response to Dr. Richard Krause’s broad-based attack on health promotion and disease prevention (*Public Health Reports*, vol. 98, pp. 529–530 and 531–535). Dr. Brandt stated the case for prevention clearly, concisely, and well. Of course it is too bad that we are in an era of runaway military spending such that prevention and basic research must quarrel with each other over the leftovers of Federal monies. The best solution, of course, would be to do as much of both as is necessary. That might require reducing military spending by up to 2 percent. Lacking that solution, however, there are several additional points that should be made about Dr. Krause’s paper.

- Health, as Hippocrates and many successors have correctly noted, is a positive state, not merely the absence of disease. Therefore, although the “beginning of health” can be “to know the disease,” knowledge of health does not always begin with disease and it never ends simply with knowledge of disease.

- Preventionists are not asking that “scientists” (Dr. Krause’s word) “apply what we know now” but that the health care delivery system, through its practitioners, applies what we know now. We also do ask that more scientific research be devoted to prevention and health promotion. Indeed, much work remains to be done in behavioral medicine, environmental health, political science, and economics related to health promotion and disease prevention.

- The science of prevention is not, as Dr. Krause would have us believe, merely epidemiology, although it is the most basic one. Further, epidemiology is not “only” observation. Descriptive epidemiology is observational; analytic epidemiology, not covered effectively in very many medical schools, is certainly “perturbational.” All epidemiology uses the same scientific method in the field that Dr. Krause uses in his lab.

- Prevention is technically straightforward and conceptually simple. Unlike disease treatment, it is neither complex nor expensive. Why then is it not universally implemented? Primarily because, as [former HHS Secretary] Richard Schweiker said, prevention is a state of mind. It is an ideological, not a scientific, question. That is why there is never, and can never be, enough evidence to convince close-minded disease treaters of what should be done, just as the tobacco industry can never be convinced that cigarette smoking directly causes disease and death. The solution to the problem is, and will be, political. In that context, only the opponents of change will suffer.

Dialogue is certainly a useful way to resolve conflict, but only when both sides are looking for change from the status quo. When one side wants change and the other side does not, dialogue leads only to delay. In the case of health promotion and disease prevention, dialogue about how the biomedical research community can best help to implement the Surgeon General’s report “Healthy People” would certainly be most useful. I hope that it begins soon because presently we are hearing all too often, as in Dr. Krause’s paper, that there is nothing different to do.

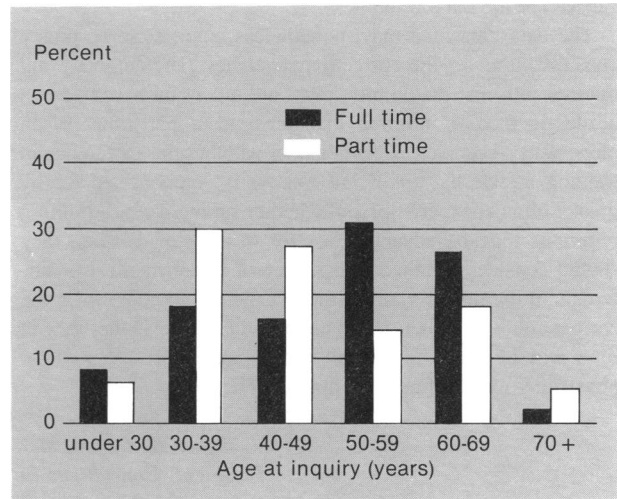
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### Prison Health Care: Training Differences Reflect Age Cohort Differences

The article profiling licensed prison physicians in the November-December issue (*J*) is disappointing in its data analysis and in the broad conclusions it reaches. It attempts to predict the quality of care given by full-time versus part-time prison physicians, on the basis of their questionnaire responses on attitudes and training, with little heed paid to the significant demographic distinctions between these two groups.

It is noted that full-time prison physicians responding to the questionnaire had a mean age at time of inquiry that was but 3

Age distribution of prison physicians (full-time and part-time)



years greater than that for part-time prison physicians answering the questionnaire. Analysis by mean age, however, has obscured the very significant difference in age distribution existing between the two groups. Analysis by median age would have shown a difference closer to 10 years. Further, as the attached graph of the authors’ data shows, 57 percent of the full-time physicians were in the age group 50–69 years old, while 57 percent of the part-time physicians were in the age group 30–49 years old at the time of response.

The data demonstrate a significant cohort difference between the full-time and the part-time physicians. The study might as well be described as comparing attitude and training responses for prison physicians who are for the most part 40 ± 10 years of age and part-time employees with those who are for the most part 60 ± 10 years of age and full-time employees. The analysis presented attributes all differences to the employment status without giving adequate attention to the age difference.

Additionally, no consideration in the analysis has been given to the duration of prison employment at the time of inquiry. I would suggest that the younger, part-time physicians may have been prison physicians for a far shorter time than had the older, full-time prison physicians. It would not be surprising if physicians, like other employees, working full-time in underfunded prison health units did not become both jaded by the experience and trapped by pension and other financial restrictions.

An appropriate analysis might have matched full-time and part-time prison physicians by age and duration of prison employment and then have compared the attitudes and training of those who acquired full-time employment and stayed employed versus those who acquired part-time employment and stayed employed as long.

Interpretation of the results in terms of predicted quality of care must be carefully performed so as not to “blame the victim” as appears to have been done. Quality of care may well be more dependent upon the quantity and quality of support provided the physician than upon his initial training or subsequent attitude or on whether he was employed full time or part time. It is not clear that this paper contains data on critical variables of care, but rather presents analysis of “pseudovaria-