Studies differed in the extent to which former smokers were included in the nonsmoker category. However, these different definitions of nonsmokers did not appear to be associated with significant differences in the estimates of the contribution of smoking to sex differences in mortality (*1*). This suggests that this methodological problem did not cause significant errors in the results.

The importance of smoking as a cause of sex differences in mortality varies for different causes of death. It appears that smoking accounts for about 90 percent of sex differences in lung cancer mortality, and consequently the increasing similarity of men's and women's smoking habits has resulted in increasingly similar lung cancer rates (*1,2,4*). By 1973–77 the incidence of lung cancer appears to have been virtually equal for men and women under age 45 in the United States (*6*). In contrast, despite the decrease in sex differences in smoking, there has been relatively little change in sex differences in ischemic or coronary heart disease mortality in the same period, reflecting the importance of factors other than smoking as causes of sex differences in ischemic heart disease (*1,2*).

With regard to Miller and Gerstein's assertion that "studies of nonsmoking populations show no differences between the life expectancies of men and women" it should be noted that (*a*) there are methodological problems with some of the studies of nonsmoking populations they cite, (*b*) in some nonsmoking populations men have higher mortality than women, and (*c*) in some populations where men smoke more than women, men nevertheless have as low or lower mortality than women for some age groups (*1,2,4*). Current evidence indicates that, in all these populations, factors other than smoking have had a significant influence on sex differences in mortality (*1–4*).

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## Use of All Age Data May Change Results of Hansen's Disease Study

In the November-December 1985 issue of *Public Health Reports*, Joseph, Yoder, and Jacobson report that " . . . the average age of diagnosis (of Hansen's disease) in native-born citizens is increasing at the rate of 2.7 years per decade." This result is based on a simple linear regression model in which the average ages at diagnosis for ten 5-year time intervals from 1932–1981 inclusive were regressed on time.

This result may or may not have been achieved if, instead of using means, all the actual ages at diagnosis and actual years of diagnosis of all 1,309 native-born patients had been employed in the regression analysis. The use of the means of age at diagnosis for the time intervals has suppressed the within-time-interval variability, which could be of considerable importance for a complete and proper analysis of these data. Since the within-time-interval variance is suppressed, the value of $R^2$ and $F$, the test statistic, is inflated. The authors' analysis yielded a slope of $+0.27$ years of age of diagnosis per elapsed year, which is quite low. This value may not be sustained if all the data are used in the analysis.

The anticipated disappearance of Hansen's disease from a population is indeed noteworthy. I am not sure, however, that this analysis supports such an expectation. Since the Joseph, Yoder, and Jacobsen paper has been cited at least once since its publication, I believe a cautionary note is justified.

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