

foreign body airway obstruction. The students in cardiopulmonary resuscitation (CPR) classes will be told that back blows were discontinued because "as a single method, back blows may not be as effective as the Heimlich maneuver in adults" and "in an effort to simplify training, the Heimlich maneuver is the only method recommended at this time" (2).

Dr. Schwartz is confused about the process surrounding the 1985 National Conference and is referred to the editorial which accompanied the Standards and Guidelines for a complete explanation (3). The standard-setting process was a continuum of events, including preconference study groups, the conference itself, conference participant peer review, and finally *JAMA* peer review. The recommendations which appeared in *JAMA* were reviewed and approved by key individuals such as Dr. Heimlich as well as the American Academy of Pediatrics. Dr. Koop's endorsement of the recommendations relating to the choking infant less than 1 year of age was appreciated (4).

The panel recommendations in all 23 areas addressed at the conference were dealt with in a similar fashion. Contrary to the implication, the panel recommendations on foreign body airway obstruction management were a significant part of the final Standards and Guidelines and all viewpoints were included in the final recommendations.

It is acknowledged that external chest compressions have produced complications in adults during prolonged CPR; however, the distinction should be made between chest thrusts to relieve foreign body airway obstruction and external chest compression delivered during CPR. They are not the same and should not be compared. This is very clear to those who teach or are taught CPR and management of foreign body airway obstruction. Despite Dr. Schwartz's statement to the contrary, there is a paucity of data on children and particularly on the infant less than 1 year of age. There were no new data presented at the conference to mandate a change from existing recommendations regarding young children. The accusations of experimentation are groundless and in poor taste.

The Standards and Guidelines have been published. A uniform approach to all aspects of CPR yields optimal results. The next National Conference is only 3 years away. Why don't we stop the needless controversies, the senseless accusations, and collect good data, do good research, and confirm or reform the current recommendations at the next National Conference!

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## Response to Dr. Waldron's Critique of Smoking Studies

Dr. Waldron's recent review of the effect of smoking on sex differences in longevity (1) appears 10 years after her earlier review of the subject (2). In 1976 she attributed "very roughly" a third of the male-female longevity differences to cigarette smoking, an estimate she now revises upward to one-half.

In the earlier paper, Waldron discussed at some length research on coronary prone behavior (Type A), sex hormones, cross-cultural differences in alcohol consumption, and other factors. The 1986 article focuses nearly exclusively on cigarette smoking. Though Waldron does not cite her earlier review, we believe these differences between the two papers reflect the growing persuasiveness of the evidence that smoking is the overwhelming cause of the male-female longevity difference (MFLD).

Waldron discusses some of the methodological problems of the Miller-Gerstein study (3) that provides the highest estimate of how much of the longevity gap is due to smoking. She correctly points out that all studies have problems with methods and data. Balanced criticism should extend to all of the studies examined. For example, the American Cancer Society study (4) uses a nonrandom sample. Waldron notes this feature of the study but does not discuss its potential deficiencies.

She emphasizes that our retrospective study has lower participation rates than some of the prospective studies. Although prospective studies always begin with higher participation rates, the rates decline over time because of the difficulty of relocating all the original participants. She argues that the 63 percent and the 73 percent participation rates in our two-sample study introduce a statistical bias in our results which requires that new statistical adjustments be made in our results. But she does not investigate the statistical implications of the 69 percent participation rate of the Framingham study or the 67 percent participation rate in the British Physicians Study. We took full account of this methodological issue in the interpretations of our findings. Moreover, we estimated this bias and reported our calculations in the original study.

The problem that we consider most significant—namely, the classification of smoking habits—was dis-

counted in Waldron's critique. Poor classifications result from both the respondents' tendencies to underreport smoking and the loose definitions of smoking classifications, that is, overlooking the differences between lifetime nonsmokers, former smokers, and lifetime smokers. Overlapping categorizations such as those noted in the British-Norwegian Migrant study where a nonsmoker was defined as "not a *current regular* cigarette smoker" lower the estimates of the apparent effect of smoking on mortality.

There are several indications of the large contribution smoking makes to the MFLD. A study by Miegs (5) in Connecticut has shown higher rates of lung cancer mortality among female smokers ages 35-44 than among male smokers in that age group. Since men and women in that age group now have similar smoking histories, Miegs' results imply that, given the same smoking patterns, as many women as men contract lung cancer.

An Amish society in the United States (6) and the rural society of Slieve Loughner (7) in Ireland provide examples of nonsmoking populations in which men and women have the same life expectancy. Contrary to Waldron's claim that Amish women are now living longer than Amish men, the Miller (6) and Hamman et al. studies of the Amish (8) show that the men live as long as the women (9).

We are encouraged that in Waldron's recent review (1) of the research she increases substantially her earlier estimates of the contribution of smoking to the MFLD. However, most studies have weaknesses in classifications that result in a lower estimate of the impact of smoking, and studies of nonsmoking populations show no differences between the life expectancies of men and women. Therefore, we estimate that the true contribution of smoking to the MFLD lies in the range of 80 to 95 percent. If more research with detailed smoking histories and nonoverlapping smoking categories is conducted, we should expect the findings reported in the future to be much closer to the Miller-Gerstein data.

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## Dr. Waldron Answers

Due to limitations of space, it is not possible to respond to each of Miller and Gerstein's points in detail in this reply, so the reader is referred to my paper (1) for a discussion of many of these points. For evidence that there are other important causes of sex differences in mortality, in addition to smoking, see recent reviews by the present author and others (2-4).

Miller and Gerstein suggest several methodological problems with the prospective studies I reviewed. The decrease in participation rates during followup does not appear to have been a serious problem, since followup was more than 90 percent complete in all but one of these studies (see table 1 in reference 1). It is possible that the findings of some of these studies were influenced by low initial participation rates and nonrandom samples, but we lack the requisite data to estimate the importance of these methodological problems (1).

One reason to believe that these methodological problems have not introduced serious bias in the results is that, with the exception of the Miller and Gerstein study, the results from all the other studies were reasonably consistent, despite the variation in methods (1). The finding that some of the results of the Miller and Gerstein study deviated from an otherwise consistent pattern of results was my reason for devoting greater attention to possible methodological problems in the Miller and Gerstein study.

Miller and Gerstein raise the problem of erroneous classification of smoking habits as the most significant methodological problem of other studies. It appears that underreporting of current smoking was not a significant problem, but underreporting of former smoking may have led to some underestimation of the contribution of smoking (5). On the other hand, the methods of the Miller and Gerstein study may have led to overestimation of the contribution of smoking. Specifically, interviewers in this study were encouraged to probe for evidence of past smoking (Dr. G. H. Miller, personal communication, July 1986) and, in a retrospective study of this type, such discretionary probing could result in biased reporting of smoking histories that could lead to overestimation of the importance of smoking (1).