Revisiting: Involuntary Smoking and Lung Cancer: a Case-Control Study

I was pleased to hear that the errors in the 95% confidence limits presented by Garfinkel et al. (1) to which I referred in my previous letter (2) arose from misapplying the program by Rothman and Boice (3) rather than from an error in the program itself, which would have had more alarming consequences. Although I am sorry that I falsely accused the program of being erroneous, it remains a matter of concern that incorrect data have been published. Elsewhere, I propose some tests and procedures to help avoid such errors appearing in published papers, errors that are, unfortunately, not that uncommon in the literature on environmental tobacco smoke (ETS) (4).

I was extremely surprised to hear Rothman and Boice (3) suggest that I rely on statistical significance as a primary indicator of effect, since in my previous letter I had cited a paper of mine (5) that argued at length that the statistically significant but weak association between lung cancer and smoking by the husband does not provide convincing evidence of a true effect of ETS exposure on risk of lung cancer. The reasons for this are precisely those to which Rothman and Boice (3) refer, biases such as those arising from misclassification of exposure to active smoking and from confounding. While it is certainly true that the presence of a statistically significant association does
not demonstrate the existence of a true effect, it is also true, as Rothman and Boice say, that lack of statistical signifi-
cance does not imply lack of an effect. I have never thought otherwise, and it is unclear to me why Rothman and Boice suggest that I had. Statistical signifi-
cance is, however, a relevant issue, as a bias-corrected analysis that gave a rela-
tive risk estimate that was not statistic-
ally significant and thus could hardly provide an adequate demonstration of a
true ETS effect.

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NOTES

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