Letters to the Editor

Re: Smoking and lung cancer: recent evidence and a discussion of some questions
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Thank you for republishing the landmark paper by Cornfield et al.,1 which the late George Comstock felt should be read by all epidemiologists at least once per year. Of the many innovative aspects for which this paper is praised, we would like to add here one that is rarely mentioned.

Appendix B of the paper provides the algebraic proof of an important property of risk ratios described in the body of the paper as follows:

If two uncorrelated agents, A and B, each increase the risk of a disease, and if the risk of the disease in the absence of either agent is small (in a sense to be defined), then the apparent relative risk for A, r, is less than the risk for A in the absence of B. The presence of other real causes thus reduces the apparent relative risk.

Consider that A and B are two variables independently related to the studied outcomes. The rule says that the risk ratio for A measured in the subgroup of the population not exposed to B will be larger than the risk ratio for A measured in the whole population. This property of risk ratios has a practical implication: if a population is at low risk for a specific outcome because its members can only be exposed to a single cause of that outcome (e.g. A and not B), the apparent risk ratio in this population will be stronger than a risk ratio estimated in another population in which several causes of the outcome are present. Sampling strictly in this low-risk population can therefore increase the statistical power of the study.

This ‘low-risk group’ approach has been since then described by Rothman and Poole2 and by Szklo and Nieto.3 However, the republication of this historical paper provides an opportunity to give back to Cornfield et al. what belongs to Cornfield et al.4

Conflict of interest: None declared.

References

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A potential cause of the reported increase in rates of autism
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There has been a discussion on the cause(s) of the reported secular increase in rates of autism over the past few decades. Does this increase merely mean an increased recognition of the condition, or a widening of the criteria—or has the increase been, at least partially, real? Rutter1 wrote of the need for hypothesis