Comments on article "Passive smoking exposure in adults and chronic respiratory symptoms (SAPALDIA study) by Leuenberger et al Am J Resp Crit Care Med 1994;150:1222-8 (R Rylander)

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The following comments are made in addition to the extensive methodological review presented by Dr Schorp at FTR.

The symptoms and diseases in the study were defined using questionnaires. The main results (Table 2) report data for non-smokers exposed to ETS and those not exposed.

The differences are very small and attain statistical significance only because of the numbers involved. It is noteworthy that the proportion of persons with a maternal history of asthma is higher in the ETS-exposed group, which suggests some kind of selection of the material. One would have expected an equal distribution if the sample was randomized for all variables except ETS exposure.

The incidence of wheeze is very low - in a recent study from the UK a prevalence of 23% was found in normal persons. This could represent interviewer bias or a default technique for presenting the question.

Physician diagnosed asthma is marginally higher in the ETS group. Again this suggests selection as asthma is not related to ETS exposure. It may also be a mix-up with the diagnosis of airways inflammation - not always distinguishable in a routine diagnosis by an ordinary physician. This figure should be compared to "sibling with asthma" which reports a higher figure and where no difference is found in the ETS group.

Dyspnea on exertion is a functional question related to narrow airways. There is however such a number of conditions that can affect dyspnea, including altitude, that the question has very little diagnostic value.

The two questions on chronic bronchitis show high prevalence values. It is not possible that this could be related to ETS exposure as chronic bronchitis requires the exposure to quite high levels of pollutant over many years.

Figures 1-5 show the highest risk at the workplace. This is contrary to most studies which report that workplace exposure is less than the exposure at home. As no measurements were made, this finding has little impact.

The discussion is the usual ETS jargon where information that supports the findings is cited whereas those data that do not support the conclusions are ignored. For instance one should have mentioned the study by Lebowitz et al (2) where careful environmental and indoor measurements of pollution were made and no relation was found between ETS exposure and lung function in children.

As of today the greatest flaw in the study is that dietary factors were not controlled for. There is now a large number of studies where a relation between different lung disease and symptoms and diet has been shown (3-5). Also it was recently reported that the intake of magnesium in food, correlated inversely with lung function and increased airway responsiveness (1).

In view of this, a counter hypothesis is that all the (small) differences found were caused by differences in dietary habits or that they reflect a selection of groups.

cc Richard Carchman
Matthias Schorp

References

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