Reply


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Dr. Mantel focuses attention on the urinary cotinine values of the relatively few controls (21 of 944) who reported themselves to be nonsmokers but who were probably active smokers. The fact that these individuals were identified and excluded from the analysis can only be considered a strength of the study. The distribution of cotinine values in this study is quite similar to those reported in the 10-country collaborative study by the International Agency for Research on Cancer of self-report and biochemical indicators of ETS exposure (1). In that study, 1.9% of the reported nonsmokers had urinary cotinine/creatinine values above 100 ng/mg, compared to 1.99% in our study (0.8% of lung cancer cases, 2.7% of colon cancer controls, and 2.0% of population controls). The lower proportion of cases with cotinine levels above 100 ng/mg as compared to colon cancer controls or population controls may well be a result of the prior use of tobacco was obtained first from the medical record and the physician was almost always originally provided by the study subject herself; therefore, in the case of a study subject unable to personally respond or provide a urine specimen, her status as a never smoker was obtained from multiple sources, and she was considered eligible only when there was agreement among sources. This approach greatly minimized the possibility of including smokers or ex-smokers.

In response to Dr. Mantel's concerns about misclassification bias, interviews were conducted with next-of-kin respondents to obtain information about 34% of the lung cancer cases and 10% of the colon cancer controls because the study subjects themselves were either deceased or too ill to respond, a reflection of the survival differential for these two types of cancer: a 72% 1-year survival rate for women with colon cancer as compared to 41% for lung cancer (2). The estimates of relative risk did not differ in analyses restricted to self or proxy respondents.

The association between active smoking and lung cancer is well established for all the main histological types of lung cancer, including adenocarcinoma and large cell, small cell, and squamous cell carcinomas, with differences only in the magnitude of the risk (3). One of the specific aims of this study was to evaluate the histological specificity of the ETS-lung cancer association by examining the relationship for each of the main histological types. We did not have our choice of which histological type to emphasize because most cases turned out to be adenocarcinomas after histological review. The number of squamous cell, large cell, and small cell carcinomas was insufficient to achieve reasonable statistical power in histological type-specific analyses. The very high proportion of adenocarcinomas, consistent across all five study centers, was an unanticipated and interesting finding, even though it is known that adenocarcinoma is the predominant cell type in nonsmokers and in all women regardless of smoking history. The number of cases with large cell, squamous cell, and small cell carcinomas will increase with the 2 additional years of study, but if the present trend continues, they will likely remain a relatively small proportion of the total lung cancer cases.

References

Letter

P. N. Lee

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Letters to the Editor

Fontham and her colleagues report results from the largest case-control study of ETS and lung cancer, which they claim provide additional evidence in favor of a causal relationship between exposure to ETS and lung cancer in women who have never used tobacco themselves. Although the study has advantages over previ-
ously published studies in a number of respects, it is important to realize that it has a number of limitations which affect interpretation.

One limitation is that it is an interim report, representing the findings of the first 3 years of a 5-year study, so that, even for the subjects considered, some data (on cotinine and on type of lung cancer) are incomplete. More seriously, no use at all has been made, at this stage, of data collected on such other risk factors as occupation, diet, medical history, and other exposures of interest, so that a potential confounding of ETS with some of these factors has not been taken into account. Two recent studies (1, 2) have demonstrated substantially reduced dietary beta-carotene levels in nonsmokers in relation to ETS exposure and have estimated that confounding of the ETS/lung cancer relationship from this source alone could bias the relative risk upward by 10% or more, and it seems plausible that adjustment for this and other confounding variables might have substantially reduced, and made statistically insignificant, the observed associations of lung cancer with various indices of ETS exposure.

Failure to take into account what might be termed "relevant denominators" is another source of potential confounding. Thus the analysis of spousal exposure is not limited to married women, the analysis of occupational exposure is not limited to working women, the analysis of social exposure is not adjusted for number of social occasions, and so on. This leads to an inevitable confusion of possible effects of ETS with possible effects of marital status, occupation, and sociability (and their correlates).

An explicit attempt has been made in this study to minimize bias due to misclassification of active smoking status. However, the procedures used seem in fact to be of limited value. Using smoking data from the medical record and from the physician, which would normally have been provided by the subject, is not of much use in validating statements by the subject. Independent statements by the next of kin or colleagues at work would have been more valuable. Urinary cotinine taken from women with lung cancer is also less useful than it might seem, since many would have given up smoking after contracting the disease. In any case, urinary cotinine does not reflect past smoking.

It is claimed that the possibility of recall bias is minimized by the use of colon cancer controls who, compared with the cases, are "similarly motivated to recall earlier exposures." In view of the attention given in the media to ETS as a possible cause of lung cancer, but not colon cancer, this seems arguable, and the possibility of recall bias is heightened by the large difference in the proportion of next-of-kin respondents providing data (34% for cases and 10% for colon cancer controls).

It is claimed that the association of ETS with lung cancer was specific for adenocarcinoma of the lung. Is this in fact true? Table 6 of the Fontham article gives a relative risk for household exposure for adenocarcinoma of 1.38 (95% confidence interval, 1.04–1.82) and for all lung cancer of 1.21 (0.96–1.54), from which I estimate (approximately) a relative risk of 0.95 (0.65–1.35) for nonadenocarcinoma, which is not significantly different statistically from that for adenocarcinoma, thus failing to justify the special attention given to the adenocarcinoma results.

It would indeed be remarkable if ETS were really to increase the risk of adenocarcinoma by 50% as claimed, given that the association of active smoking with adenocarcinoma is so weak, and given the much lower exposure to smoke constituents from ETS than from active smoking.

Particular attention is given to the dose relationship for lung cancer risk in relation to pack-years of exposure from the spouse, and it is stated that "a dose response, not likely due to chance, was apparent for exposure to tobacco smoke during adult life from a variety of exposure sources." When one examines the data shown in Table 6 of Fontham et al. a different impression is given. Within the exposed categories, a tendency for response to rise smoothly with increasing exposure is only seen in two of the eight dose-response relationships, and even in these two the trend is clearly not significant. One gets the impression that the reason the authors chose specifically to present in graphical form results for spouse pack-years of exposure was that this was the index (of many tried) that best showed a smooth relationship. It would have been better had they not overemphasized one specific relationship selected a posteriori.

References


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Dr. Lee raises several issues with regard to our report on ETS and lung cancer. His first concern is that it is an interim report, representing 3 years of a 5-year study. We felt at the time, and remain convinced, that the availability of a large data set with which to address an unresolved issue of great public health importance was compelling justification for publishing a report. Data based on cotinine analysis and histopathological review were not complete for all study subjects at the time of submission. Assuming the proportion of study subjects

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Many, but certainly not all, potential confounders were considered in the analyses, including age, race, geographic region, respondent type, income, and education. Only age had a significant effect on the observed relationships. Given the large number of study subjects exposed to ETS and the relatively small number with occupational, medical, or “other” risk factors, it is unlikely that confounding by these factors would either substantially reduce or elevate the observed associations. The effects of these risk factors will be examined in further analyses. We are doing an extensive analysis of dietary factors, in particular fruits and vegetables, fats, and antioxidant micronutrients including \( \beta \)-carotene. Potential confounding of the ETS lung cancer relationship by relevant dietary factors will be evaluated. It does not appear that dietary intake of \( \beta \)-carotene is related to the spouse’s smoking habits in our study. Mean daily intake of \( \beta \)-carotene does not significantly differ between study subjects whose spouse smoked and those whose spouse never smoked. This is true for the total pool of study subjects and after stratification by case-control status. This is, of course, only one indicator and does not eliminate the possibility of some confounding, but suggests that effects, if any, are not likely to be major.

Dr. Lee suggests that the “relevant” denominators are not all female lifetime never smokers at risk of lung cancer with and without exposure, but rather female lifetime never smokers with exposure and a subset without exposure (e.g., married, but spouse did not smoke) after excluding another subset without exposure (e.g., never married, therefore not exposed to spouse who smoked). The relevance of a denominator selected in this fashion is debatable. That aside, restricting analysis to ever-married women had a minimal effect on the total sample size or on estimates of risk. Ten of 420 cases (2.4%) and 43 of 1131 controls (3.8%) had never been employed (50 cases and 85 controls). The estimates of dose and duration (pack-years) were selected a priori as indicators of exposure from an individual (spouse, mother, father, other household members). Duration is biologically plausible. The possibility remains that nonsmoking women with lung cancer and nonsmoking women with colon cancer are not similarly motivated to remember exposures to the tobacco smoke of others. The larger proportion of next-of-kin respondents providing data for lung cancer cases (34%) than for colon cancer controls (10%) reflects the realities of the two diseases: the 1-year survival rate for white women with colon cancer is 72% compared to 41% for lung cancer (1). As reported, the estimates of relative risk were similar regardless of respondent type.

A strong association of active smoking has been reported for all types of pulmonary carcinomas including squamous cell, small cell, and adenocarcinoma, differing only in the magnitude of the effect (2). The special attention given to adenocarcinoma is warranted in view of the fact that 78% of the reviewed cases in female never smokers were adenocarcinoma, a remarkably high proportion. The differences in composition of mainstream and sidestream smoke have been well described (3). That exposure to sidestream smoke might result in a distribution of histological types of lung cancer different from that associated with exposure to mainstream smoke is biologically plausible.

Dose (cigarettes/day), duration (years), and an index of dose and duration (pack-years) were selected a priori as indicators of exposure from an individual (spouse, mother, father, other household members). Duration is more easily quantified in a setting where “dose” is provided from multiple individuals. That was the rationale for the selection of duration as an indicator of ETS exposure in such settings as shown in Table 6.

References


P N Lee


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