of this magnitude would comprise several thousands of lung cancer cases and would be unrealistically large. Despite the fact that some "negative" studies on asbestos-exposed populations have turned out to be of questionable quality or based on inadequate population-time experience, the logic of inference from epidemiologic evidence to the probability of causation should be flawless before a conclusion is drawn, because, in tort suits, the drawing of hasty conclusions can affect compensation decisions.

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REFERENCES

To the Editor:

Drs. Nurminen and Tossavainen are correct that there is some contention as to whether asbestososis (parenchymal) is a necessary condition to diagnose asbestos-related lung cancer. Most of the current literature on this subject in my introduction, however, provide data and opinions that asbestososis is necessary.

In their first counterclaim, they state that lung cancer can appear in asbestos-exposed workers without asbestososis and cite their references 2 and 3 to support this. Their reference 2, however, by Anttila et al, is not an epidemiologic study. It is merely a collection of resected cases of lung cancer and is, therefore, biased by the exclusion of cases with substantial asbestososis since this condition would obviate resection because of poor pulmonary function.

Their reference 3 by Nurminen was an epidemiologic mortality study using death certificate information on both lung cancer and asbestososis in a cohort of asbestos factory workers. He observed 13 cases of lung cancer and expected only 6 based on national rates by age and sex, giving a relative risk of 2.2 with no adjustment for differences in smoking habits between the cohort and the Finnish population. Tables 4 and 5 in the Nurminen article reveal that 6 of the 13 cases of lung cancer also had diagnoses of asbestososis on the death certificates. Therefore, the relative risk for those without asbestososis, assuming that asbestososis was always recorded on the certificates, would be seven observed and six expected or 1.2, a value which is easily the result of chance. Since smoking is the dominant causal factor for lung cancer and high proportions of asbestos-exposed workers have been smokers, it is obvious that some lung cancers among such workers would have occurred without asbestos exposure.

In their second counterclaim they say that the validity of pleural plaques as a marker of asbestos exposure is far from perfect. I agree with this since plaques are sometimes not visible on chest roentgenograms and this may bias the results. The three autopsy studies I described, however, also yielded insignificant estimates of relative risk. Even the pooled data for the autopsy studies, not presented in my review, gave a statistically insignificant odds ratio of 1.38 with 95 percent confidence limits of 0.81 to 2.37.

I agree that it would be necessary to do very large studies to validate a very low elevated relative risk for lung cancer among people with pleural plaques who do not have asbestososis, and this may be unrealistic. It is difficult to prove a negative. However, their example is only speculation. We must make reasonable decisions based on the best hard data, and there is no such thing as flawless epidemiologic evidence of an observational nature.

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Survival of Patients Undergoing Nd-YAG Laser Therapy Compared With Nd-YAG Laser Therapy and Brachytherapy for Malignant Airway Disease

To the Editor:

I read with interest the article by Shea et al1 in the April 1993 issue of Chest. The authors describe their experience with endobronchial therapy for exophytic inoperable squamous cell carcinoma of the lung with Nd-YAG plus or minus low dose brachytherapy. Their conclusions about longer survival of patients receiving combination therapy may be related to the deficiencies that exist in their study rather than a true benefit. I cite various factors that would affect survival of inoperable lung cancer and that are not addressed in their study.

First, this was a retrospective study and patients were not randomized to each arm. Second, the patients had inoperable squamous cell carcinoma. Were they inoperable because of poor pulmonary reserve, stage IIIIB disease, or stage IV disease? Patients in each group will have a marked difference in survival irrespective of the therapy administered.

Third, why was endobronchial therapy given? Just because an endobronchial exophytic lesion existed or because these patients had postobstructive pneumonia? They mention that patients were symptomatic, but the exact severity of their symptoms and complications before therapy were not mentioned. Did more patients in the laser therapy group have postobstructive pneumonia? What was the performance status, P(A-a)O2 gradient, FEVi, etc, of the patients in the two groups?

Endobronchial brachytherapy should not be administered to all patients with an exophytic lesion. Without any kind of systemic therapy, it is hard to realize survival benefits in patients with metastatic nonsmall cell lung cancer. Patients with problems likely to lead to imminent death, ie, gas exchange abnormalities from major bronchial obstruction or postobstructive pneumonia are likely to benefit from immediate Nd-YAG laser resection of exophytic tumors. It would make sense to follow this with endobronchial brachytherapy or subsequent laser resection on a timely basis to keep the airway open, depending on the overall condition of the patient.

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