

## LETTERS TO THE EDITOR

### Pleural mesothelioma with non-Hodgkin's lymphoma

Tondini and colleagues (December 1994;49:1269-70) have presented an interesting case of non-Hodgkin's lymphoma in an asbestos worker with mesothelioma. Lymphoproliferative disorders are not recognised as prescribed asbestos-related diseases, although their case and others attempt to make this relationship.

We would like to add to the literature the case of a 60 year old man who developed histologically proven mesothelioma after heavy exposure to asbestos in the Devonport dockyard. Death occurred 15 months after presentation with a pleural effusion and at necropsy an ulcerating mass was found in the stomach. Biopsies of the lesion showed a lymphocytic lymphoma.

Contrary to the view of Tondini and colleagues, the association between gastrointestinal cancer and asbestos has not been conclusively proven,<sup>1</sup> although an increased incidence of exposure to asbestos has been noted in a study of gastric lymphoma.<sup>2</sup>

We wish to add our case to those referred to by Tondini *et al* and agree that further study into the relationship between lymphoma and asbestos should be conducted.

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- 1 Edelman DA. Exposure to asbestos and the risk of gastrointestinal cancer - a re-assessment. *Br J Ind Med* 1988;45:75-82.
- 2 Ross R, Dworsky R, Nichols P, *et al*. Asbestos exposure and lymphomas of the gastrointestinal tract and oral cavity. *Lancet* 1982;iii:1118-9.

### Vegetarian diet and tuberculosis in immigrant Asians

The paper by Dr D P Strachan and colleagues (February 1995;50:175-80) is a valuable contribution to the debate on vegetarian diet and tuberculosis; however, we do not believe a causal relationship between vegetarianism and the development of tuberculosis in immigrant Asians can be inferred from this study.

The response rate amongst cases (33%) and community controls (12%) was very low and was not stated in clinic controls. It was unclear whether community controls were all first generation immigrants (a selection criterion for cases and clinic controls). Not enough was known about the characteristics

of non-responders to make a confident assessment of the direction in which selection biases might act. Non-responders may differ from responders in several important respects other than religion, age, and sex, such as time since immigration and socioeconomic class.

The effects of many confounders on the relationship between vegetarianism and tuberculosis were investigated but diabetes (a risk factor for tuberculosis which is also related to diet) was not controlled for.

There were no vegetarians in the Muslim group, so presenting an odds ratio for vegetarianism controlled for religion is inappropriate due to the collinearity between the hypothesised exposure (vegetarianism) and the potential confounder (religion). It would have been more appropriate to examine the interaction between religion and vegetarianism and to present the odds ratio for the Hindu population separately.

We were interested in the selection of dietary groups used to calculate the dose-response relationship. They were based on the number of meat/fish eating days per month and seem unlikely to reflect substantial differences in micronutrient deficiency. The only group which significantly differed from the vegetarians was the group eating meat daily.

We feel that this study highlights an important area for further research but does not provide strong evidence for vegetarian diet as a risk factor for tuberculosis.

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**AUTHORS' REPLY** The purpose of our paper was to demonstrate that diet, rather than religion, was the statistically independent risk factor influencing the distribution of tuberculosis among Asian immigrants in our area. Although we speculated on possible mechanisms, we did not set out to prove a causal relationship.

All religious groups were included in our analysis because we wished to explore the relative influences of diet and religion on the risk of tuberculosis. If the analysis is restricted to Hindus only, the association of vegetarianism with tuberculosis is stronger, although less precisely estimated due to smaller numbers of subjects. The unadjusted odds ratio for vegetarianism comparing Hindu cases with Hindu community controls is 3.4 (95% CI 0.9 to 12.8,  $p > 0.05$ ), and comparing Hindu cases with Hindu clinic controls is 4.9 (95% CI 1.4 to 17.6,  $p < 0.01$ ). Our published analyses, based on all subjects, are therefore conservative. It is not possible to address statistical interactions between religion and vegetarianism as there were no vegetarian Muslims.

We acknowledged in our paper that the low response rate poses problems of interpretation. However, as this is an analytical rather than a descriptive study, the characteristics of the respondents are less relevant than the strength of association between exposure and disease among respondents and non-responders. We were able to assess this for religion, but not (as would ideally have been the case) for vegetarianism. The findings for religion suggest a conservative selection bias, as discussed in the paper and illustrated in table 2. Analyses based on the clinic con-

trols, among whom the response was 100%, yield higher odds ratios than similar analyses based on the community controls, supporting our conclusion that the relative risk of tuberculosis among vegetarians is more likely to have been underestimated than inflated by non-response bias.

All community controls were first generation Asians and there were only two diagnosed diabetics in our series, one Hindu case (vegetarian) and one Muslim community control (non-vegetarian).

The precision of the relative risk estimates presented in the figure depends on how finely the data are divided, and particularly on the size of the reference group. The important statistic is the test for linear trend. We chose to subdivide the non-vegetarians into five groups to illustrate that frequency of meat and fish consumption influences the risk of tuberculosis even after excluding the vegetarian group, and with no evidence of a threshold. Further work is required to determine whether micronutrient levels are affected by frequency of meat and fish consumption within this range, but this trend suggests that the association between diet and disease is not wholly confined to vegetarians.

If the data in the figure are reanalysed with the non-vegetarians being divided into two groups (eating meat or fish  $> 20$  days/month or  $1-20$  days/month) then a significant relative risk emerges in the group with less frequent consumption (odds ratio, adjusted for age, sex and religion: 3.5, 95% CI 1.4 to 8.7), with the risk among vegetarians relative to frequent meat/fish eaters being even higher (adjusted odds ratio: 8.9, 95% CI 2.7 to 29.0).

Risk factors may or may not be causal, and we pointed out in our paper that further work is required to elucidate the mechanisms underlying the association between diet and tuberculosis. Faced with adjusted relative risks of the magnitude presented here and in our paper, we find it hard to concur with the assessment by Dr Hayward *et al* that our findings provide no strong evidence for vegetarian diet as a risk factor for tuberculosis, but agree that it would be premature to assume a causal relationship.

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The retrospective case-control study of Drs Strachan *et al* on diet and tuberculosis (February 1995;50:175-80) is methodologically highly unsound. To attempt to assess the potential dietary causes of tuberculosis by administering a dietary questionnaire up to 10 years after the illness is, in itself, highly unreliable since diet may have changed in the intervening period. In addition, to examine only the diets of 15% of the total number of tuberculosis cases who could have become part of the study leaves altogether too much room for selection bias and error. It seems that the BMJ Publishing Group's normally fastidious standards for papers on the health effects of vegetarian diet<sup>1</sup> appear not to have been applied to this paper.