for attributing the decrement in FEV\textsubscript{1} to exposure to
dust are explained clearly in the source paper\textsuperscript{10}; and
just as we are not yet clear on the functionally im-
portant pathological changes related to dust, we also
have little basis on which to judge the relevance of
Morgan's point on pulmonary hypertension or cor
pulmonale. Incidentally, the study by Fernie \textit{et al}
included only eight non-smoking miners without
progressive massive fibrosis,\textsuperscript{11} too small a group to
provide substantial support for Morgan's argument.

References

1 Amandus HE, Lapp NL, Jacobson G, Reger RB. Significance of
irregular small opacities in radiographs of coalminers in the
and relation to underground exposure of radiological irregular
opacities in south Wales coal workers with pneumoconiosis. \textit{Br
3 Dick JA, Jacobson M, Gauld S, Pern PO. The significance of
irregular opacities in the chest radiographs of British coal min-
ers. In: Proceedings of the VI International Pneumoconiosis
4 Amandus HE, Pendergrass EP, Dennis JM, Morgan WKC.
Pneumoconiosis: inter-reader variability in the classification of
the type of small opacities in the chest roentgenogram. \textit{Am J
5 Cockcroft A, Berry G, Cotes JE, Lyons JP. Shape of small opac-
6 Musk AW, Cotes JE, Bevan C, Campbell MJ. Relationship be-
tween types of simple coalworkers' pneumoconiosis and lung
function. A nine year follow-up study of subjects with small
7 Collins HPR, Dick JA, Bennett JG, \textit{et al.} Irregularly shaped small
shadows on chest radiographs, dust exposure, and lung func-
8 Soutar CA, Hurley JF. Relation between dust exposure and lung
9 Hankinson L, Reger RB, Morgan WKC. Maximal expiratory
10 Hurley JF, Soutar CA. Can exposure to coalmine dust cause a
11 Fernie JM, Douglas AN, Lamb D, Ruckley VA. Right ventric-

Incidence of cancer among vinylchloride and poly-
vinylchloride workers: further evidence for an associa-
tion with malignant melanoma

\textit{SIR—Recently} Heldaas \textit{et al} presented the results of a
cancer morbidity study in vinylchloride exposed
workers (1987;44:278–80). They are suggesting a rel-
ation between exposure to VC and the incidence of
malignant melanomas. This suggestion is based on
visual comparison of observed/expected cases in sub-
groups, stratified on levels of exposure or on years
from first exposure (resp tables 1 and 2).

A statistically significant trend between duration or
level of exposure and specific cancer morbidity
may be tested according to Breslow \textit{et al}. This test
on significant trend was applied on the
observed/expected cases of melanoma in relation to
level of exposure (table 1) or in relation to years from
first exposure (table 2). The probability, that the dis-
tribution of observed/expected cases of melanoma in
the subgroups of table 1 and 2 was due to chance, was
respectively 65% and 22%.

The only significant finding is the increase in the
number of cases of melanoma in the total group of
workers. Such an increase, however, was also found
in cohort studies of workers with widely differing oc-
cupations. Moreover, in all other cohort studies of
workers exposed to vinylchloride no increase of mor-
tality from melanoma was found.\textsuperscript{2} Therefore, the
present epidemiologic evidence does not support a
causative relation between the incidence of melanoma
and exposure to occupational vinylchloride.

\textbf{W F TEN BERGE}

\textbf{Correspondence}

\textbf{References}

1 Breslow NE, Lubin JH, Mark P, Langholz B. Multiplicative
models and cohort analysis. \textit{Journal of the American Statistical
Association} 1983;78:1–12.
2 Nicholson WJ, Henneberger PK, Seideman H. Occupational

Drs Heldaas, Laugård and Andersen reply:

Ten Berge seems to put a great deal of emphasis on the
statistical inference of the results in our study.\textsuperscript{1}
We are less concerned about the statistics; in our view
the emphasis should be put more on the design of the
study. We think that the quality of epidemiological
studies in occupational medicine should be judged
primarily by the characterisation of the level and du-
ration of exposure to the potentially harmful agent,
and to what extent selection has been avoided, and
whether confounding has been dealt with in a proper
way. Occupational diseases with presumed long
latency periods have also to be dealt with in a manner
which takes care of this phenomenon.

A small sample size is a common weakness in the
design phase of small studies. Ten Berge, however,
seems to interpret this problem of small sample size
and failure to obtain "statistical significance" as being
evidence for non-causality. It is not obvious from ten
Berge's letter how he has tested statistical significance.
Correspondence

He has, however, avoided discussing the six cases of melanoma versus one expected, which we consider to be the most important result. As pointed out in the previous study, studies of cancer incidence have advantages compared with studies of mortality, particularly for tumour sites that are successfully cured.

We feel confident that the methodological aspects have been properly considered in our study. Consequently, we are less concerned as to whether the results are “statistically significant” or not. We consider the fact that two cases of melanoma developed in the prior high risk group selected from the same VCM exposed population before the performance of the previous study as being of more significance for the interpretation of possible causality than the presence of statistical significance. Development of new cases in a preselected risk group may be of greater significance in a causality discussion than is the presence of “statistically significant” results. As opposed to “statistical significance,” a previously selected a priori high risk group cannot be manipulated.

A recent study from Du Pont, USA, seems to support our finding, but apparently our results have not attracted the author’s attention.

The current lack of adequate animal and in vitro models for studying the development of malignant melonanas makes the study of the possible causal relation between VCM exposure and this tumour complicated. Even so, Maltoni et al. succeeded in producing malignant melonanas of the skin in Syrian golden hamsters exposed to a wide range of VCM levels, and they concluded that the tumours were related to the exposure.

References


Pulmonary fibrosis in asbestos insulation workers with lung cancer

SIR—Rudd’s letter (1987;44:428–9) points to several interesting questions.

1. Histopathological pulmonary findings in asbestos insulation workers with long exposure to the dust. I reviewed lung parenchyma sections submitted to me by pathologists in various institutions in the United States and Canada. As expected, the material was taken from various parts of the lung, often unspecified. It is widely accepted that pulmonary asbestosis is characterised microscopically by diffuse interstitial fibrosis caused by inhaled asbestos fibres. This was sought in my examinations in available parenchyma; Rudd argues that the subpleural areas be excluded, a rather arbitrary preference in view of the absence of data that parenchyma here differs from parenchyma elsewhere; particularly since this part of the lung often has a considerable concentration of asbestos fibres, the result of “pleural drift” of inhaled dust. Parenthetically, lung cancer after asbestos inhalation is not infrequently peripheral in origin.

2. I reported the presence or absence of microscopically evident diffuse interstitial fibrosis. I also reported that in 130 of 138 one or more asbestos bodies were seen. It is commonplace that in single (5 μ) sections, asbestos bodies may be absent. Does this mean that they are not present (sometimes in large numbers) in the lung? Of course not. The likelihood of showing asbestos bodies varies directly with the amount of tissue studied. They are more readily seen in thick (30μ) sections than thin, more when bulk tissue is digested and filtered than in sections, more when exposure has been to amphiboles than chrysotile, and more when long and thick fibres were inhaled than short and thin. It is thus not enough to say that so many or so many asbestos bodies are seen; one should immediately add comment regarding how much tissue was available for examination and how they were sought. Indeed, in our study, we did not expect to find so many cases (since, occasionally, there was only a single thin section available for review) in which at least one asbestos body was to be seen. My colleagues consider that perhaps this was related to the diligent, prolonged examination to which each slide was subjected.

3. I was rather surprised to learn from Rudd that in the United Kingdom when there are “too few” asbestos bodies in slides of asbestos workers’ lungs the diffuse interstitial fibrosis is not infrequently categorised as cryptogenic fibrosing alveolitis. There must be a great deal of this disease nowadays in the United Kingdom among asbestos workers. And many asbestos workers with diffuse interstitial fibrosis are not eligible for disability compensation.

4. Asbestos bodies again. I am sure that Rudd is as familiar as we are with the multitude of observations that in lung tissue of asbestos workers with or without lung cancer there tend to be myriads of asbestos fibres uncoated by the iron protein material that allows them to be visible by (relatively insensitive) optical microscopy and thereby seen as asbestos bodies. Generally, the uncoated fibres are demonstrable only by...