
Sir,—The article by Cooper and Johnson (1990;47:52-7)^1 raises some epidemiological issues. Their enthusiasm for multivariate statistical methods leads them to underestimate their use for many years in respiratory epidemiological studies.

There are important conceptual differences between variable types in their relation to each other and to various outcomes. Multivariate methods are generally used to control or adjust for confounding or to display effect modification by extraneous variables affecting the relation between exposure and outcome variables.

For an extraneous variable to be a confounder it must not be an intermediate step in the causal pathway between exposure and outcome. By entering exposures such as smoking or experience of mining and intermediate variables such as forced expiratory volume in one second (FEV1) and forced vital capacity (FVC) into the same regression analysis of exercise minutes, the authors have erred epidemiologically. This may explain why the contributions of smoking and age were not significant and why this finding was “contrary to common wisdom.” It is also not clear from the text whether they forced age into the regression model or not, bearing in mind that mining experience is likely to be strongly colinear with age.

Causal is a difficult word to use in epidemiology. It would perhaps have been better for the authors to have taken an a priori decision about which variables to examine in relation to which outcomes. The hypothesis thus stated, the remaining task is then to choose the most appropriate statistical method that does not violate basic epidemiological (or statistical) requirements.

Two other epidemiological points relate to the issues of misclassification and external validity. The authors generalise rather hastily from their findings. Because they find no relation between mining experience (years underground) and FEV1, they make the rather radical recommendation that FEV1 be dropped as a criterion for determination of black lung disability if their findings are substantiated by others. Yet studies on both United States and British populations of coal miners have shown effects of dust exposure on FEV1.14

Mining experience is a relatively poor proxy indicator of exposure to dust. Also, it is not clear whether or not the authors excluded spirometric “test failures” as required by the ATS (1979) criteria they employed. A misclassified indicator of true exposure to dust together with the exclusion of “test failures,” which biases associations with FEV1, towards the null, could suppress an association between exposure to dust and FEV1.

It appears to us to be somewhat artificial to characterise respiratory disability in coal miners as purely restrictive. There is evidence that non-specific exposure to dust in coal mines (and elsewhere) may cause obstruction (decreased FEV1). Both restriction and obstruction could afflict the same worker and both could contribute to respiratory disability as measured by exercise capacity.

We should like to end with a question. Were cases of progressive massive fibrosis excluded from the study group? This is not clear from the text and could influence FVC findings.

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