Dear Sir,

As a group of independent specialists in the field of the hand–arm vibration syndrome (HAVS), we would like to comment on the paper by Coughlin et al. [1], which should not pass without comment. This paper contains identical data to a presentation given to the 1998 meeting of the Vascular Surgical Society of Great Britain and Ireland; the data have been known for some time.

All study group members were ‘Stage 3’ disease—but no information is given as to how the assessment was made, or by whom. No information is provided regarding the variables that should be controlled in this type of investigation, although individuals were asked not to smoke in the 24 h period prior to the investigation. Whether they did so is not recorded—nor are the other variables, e.g. recent vibration exposure, alcohol ingestion, core body temperature.

We feel the appropriate control group should comprise vibration-exposed workers but without symptoms of HAVS. Without such a group, any finding can only be interpreted as a signal that an individual has been exposed to vibration. No other conclusion is possible. The source of the control group is not defined, nor is the reproducibility of the investigation presented. Whilst the authors correctly quote from Griffin [2], they do not compare the findings in this small study with Griffin’s findings directly, preferring only to make general comments.

Some points from the analysis are important. Can a study of this size provide the answers? The control group does not reflect the experimental group in either age or sex, and the study group has an uneven spread of ages with a range of 29–81 and a median value of 45. If the data are parametric then the upper age limit should be about 61 and not 81.

The statistics cause concern. The authors use $P < 0.01$ as a cut-off value to determine statistical significance, rather than the traditional $P < 0.05$. In some instances, they then treat a statistically non-significant result as a measure of equivalence. This is a misrepresentation of what statistical significance represents. Whilst a statistically significant result is an indicator of difference, a statistically non-significant result is not an indicator of no difference, just that a difference was not found. This is a very different thing.

Also, we would point out that the modelling attempted did not describe the process of rewarming very well. The authors are testing for a significant difference in the rate of rewarming, and do this with three $t$-tests, rather than using an appropriate multivariate approach, such as a mixed analysis of variance or analysis of covariance. A more sophisticated technique, such as growth curve remodelling, would have revealed much more about the data.

If a sensitivity of 95% is achieved, together with a specificity and positive predictive value of 100% in each case, these findings are important. We have been unable to find any correlation between cold provocation testing with digital rewarming and the diagnosis of HAVS in a series of 19 085 miners tested under the scheme set up by the Department of Trade and Industry and presented to the 9th International Conference on Hand–Arm Vibration, Nancy [3]. Perhaps laboratory-based research does not easily transfer to the field.

In a more recent publication [4], Coughlin et al. presented data on aesthesiometry in the assessment of patients with HAVS. In this later article, they state that ‘There is a need to for sensitive objective investigation to confirm the diagnosis . . .’. This is repeated in the later article’s summary: ‘The diagnosis and assessment of disease severity are subjective at present’. Given the findings they describe in the earlier paper, these statements appear to be non sequiturs!

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References


