Correspondence

Health, fitness, physical activity, and morbidity of middle aged factory workers I

SIR.—In their recent article (1986;43:733-53) Tuxworth and colleagues concluded that their male subjects who lived in Birmingham were mostly of “inadequate fitness” compared with Canadians and others. Their observations were based on maximal oxygen uptake ($\dot{V}O_2$ max) estimated from a submaximal cycling test but the procedures were not described in sufficient detail for the tests to be repeated or evaluated and no evidence was given that the methods were appropriate for their population. The Canadian results were apparently adjusted by 21% before the comparison was made. In view of these uncertainties, can one be confident of the conclusions?

To check on this I have compared the mean $\dot{V}O_2$ max of each of Tuxworth's five age groups with the findings of Jones and colleagues for men from Toronto, excluding athletes and those with chronic disorders.¹ For the latter, $\dot{V}O_2$ max was measured directly using a cycle ergometer. The mean results after allowing for age and body size were similar in the two studies; the average difference was well within the experimental error of indirect procedures for estimating $\dot{V}O_2$ max.^{2,3} This finding throws doubt on the interpretation of the Birmingham data which may have been unduly influenced by the subjects' estimates of their own habitual activity. Interpretation may also have been influenced by the use of 2×2 histograms which neglected important differences between subgroups and by erroneously assuming a proportionality between $\dot{V}O_2$ max and body mass. The assumption is the basis for the index $\dot{V}O_2$ per kg body mass which overcorrects for differences in mass between individuals and is difficult to interpret on this account.⁴ The authors might overcome some of these difficulties by extending their multiple regression analysis to express $\dot{V}O_2$ max (l/min) in terms of truly independent variables; this is not the case at present.

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- 2 Cotes JE, Davies CTM, Edholm OG, Healy MJR, Tanner JM. Factors relating to the aerobic capacity of 46 healthy British males and females, ages 18 to 28 years. *Proc R Soc Lond B* 1969;174:91-114.

- 3 Robertshaw SA, Reed JW, Mortimore IL, Cotes JE. Submaximal alternatives to the Harvard pack index as guides to maximal oxygen uptake (physical fitness). *Ergonomics* 1984;27:177-85.
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Dr Tuxworth replies:

Cotes appears to identify “inadequate fitness compared with Canadians” as a central message of our paper. This is not the case. Careful reading of the entire text will show that inadequacy is referred to in the abstract in the context not of comparison with other population norms but by the criteria of functional capacity and health benefit. Indeed, fitness is described as being comparable with that reported in several other studies and later, with regard to other groups, as inadequate by comparison with “particularly those of mixed activity.” In the section where the Canadian values are referred to it is pointed out that the mean values for the Cadbury-Schweppes subjects compared with “good” values and “minimal” values for the Canadians for age groups 40-49 and 50-59 respectively. As the minimal value is the middle of the five categories in the Canadian tables this would imply that the Cadbury-Schweppes subjects are at least as fit rather than of “inadequate fitness compared with Canadians,” especially as we were careful to draw attention to the fact that the Canadian population was highly screened medically, with a consequent bias towards the inclusion of the relatively fit. Cotes's comparison with the data from the study of Jones *et al*, therefore, does not call into question, but rather reinforces, any conclusions we have tentatively made in our paper.

The Scandinavian and Canadian fitness tables were included for interest as examples of the difficulty of interpreting fitness measures qualitatively. The Canadian values were adjusted, as explained in the text, on the basis of observations by Bailey *et al* (ref 32). We find it difficult to see the point Cotes is making here.

We acknowledge that our test protocol and procedures were not described in detail in the paper. Ironically, the original submission contained a full description, the entire paper being almost twice its published length. The pruning of the procedural section was an editorial decision. We are confident, however, that the methods used were appropriate to the population as stated in the paper.

The test used was an entirely conventional progressive exercise test, the only “novel” feature being a system whereby load increments were individually adjusted according to heart rate response to the previous workload. The tables for such a sensitivity of adjustment were derived from an extensive pilot study on the same population. The advantage of this procedure was that the bicycle test represented as nearly as possible a comparable level of relative exertion for

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each subject. It enabled the subjects' heart rate in the final load to be brought within two to five beats of 80% of $\bar{x}$ maximum heart rate for age, a level which in our view conformed with recommended maxima for exercise testing (ref 18) but also was high enough to ensure the minimum extrapolation for estimation of $\dot{V}O_2$ max.

Cotes rightly draws attention to the imperfection of weight related $\dot{V}O_2$ max as a fitness indicator. Our main reason for using $\dot{V}O_2$ max per kg was its near universal use in other studies (such as those in table 5). In interpreting Van der Walt and Wyndham's prediction of the energy cost of walking and running we have used their formulas for an individual weighing 76 kg, the mean weight for our population, recognising that there is not a direct proportionality.

We would prefer to use a more sensitive index when we come to the stage of reporting on morbidity in relation to fitness but it is not clear what that index should be. The gross value of $\dot{V}O_2$ max would appear to be much less meaningful, and relating $\dot{V}O_2$ to lean body mass or other body dimensions also can be less than helpful in clarifying the relation between $\dot{V}O_2$ and relative functional capacity. Other indices such as W150—used by some authors across entire age ranges—seem to give no helpful basis for comparison. We would welcome suggestions from other readers and in the meantime will certainly consider that of Cotes—but what are the “truly independent variables?”

The 2×2 histograms were not used in any analytical process but only to present mean results for different categories of one dimension in relation to another dimension. We acknowledge that individuals may poorly estimate their participation in activity. Such large numbers cannot be individually monitored—even if they could this would then affect their behaviour. We did, however, attempt to make the questionnaire as dependable as possible by carefully piloting it and using trained interviewers.

We hope that some interest will have been aroused by the paper despite the imperfections of our measures and procedures, most of which arose from compromises which have to be made in field work between ideality and practicality.

Benzene exposure in chemical workers

SIR,—In the 8th revision of the International Classification of Diseases (ICD) other neoplasms of lymphoid tissue is coded 202 and polycythemia vera is coded 208. There is an error in table 4 of the paper by Bond *et al* (1986;43:685–91) in which other lymphatic tissue was reported as ICD 208. Since there is interest

in the association between lymphomas and benzene exposure, it would be helpful if the authors could give the results obtained for ICD 202 specifically in order to derive maximum value from this interesting study.

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Dr Bond and Dr Cook reply:

We are grateful to Dr Johnson for calling attention to a small error which appeared in table 4 of our paper. The category “Other lymphatic tissue” as used in our mortality analysis program¹ is not restricted to ICDA code 208 as is implied, but also includes codes 202 and 203. This was an oversight on our part in preparing the manuscript.

The one death coded to this category was the multiple myeloma (ICDA = 203) which is discussed on page 690. No deaths were attributable to code 202. Our mortality analysis program does not have rates specific for ICDA code 202, which would allow for the calculation of an expected value. In 1975, however, 46% of all deaths coded to ICDA 202, 203, or 208 among United States white men were attributable to ICDA 202.² Therefore, a rough estimate of the expected number of deaths in the cohort due to code 202 is $(0.46 \times 1.3) = 0.6$.

References

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Lung function in coalworkers' pneumoconiosis

SIR,—In a recent report (1986;43:644–5) Zhicheng considers the issue of lung function in coal workers' pneumoconiosis. Although this is an important topic, the study design used in this report limits the inferences which may be drawn from the data, particularly with regard to the aetiology of the deficits in pulmonary function.

I think that the most serious flaw in this study is the lack of quantitative information regarding smoking history—for example, in pack-years—in the four groups. Cigarette smoking is associated with both restrictive and obstructive lung defects^{1 2} and