

Nutrition Discussion Forum

Energy balance in patients with chronic obstructive pulmonary disease

It was with great interest that we read the recent publication by Goris *et al.* (2003) on energy balance in patients with chronic obstructive pulmonary disease (COPD). The authors presented the results of a small sample of patients with COPD randomised to an intervention group (n 11) and a control group (n 9). The intervention consisted of a nutritional supplement three times per d during 3 months and the authors could not find any difference in body mass change between the two groups during the 3 months. Change in body mass was, however, not the main focus in the presented paper. The main focus was, as stated in the aim: ‘to gain insight in the energy balance’.

A carefully performed 7 d food record was used to capture the energy intake. This was converted into total energy intake. On the expenditure side of the energy-balance equation, a tri-axial accelerometer (Tracmor) was used during the same 7 d as the food record was completed. In the subjects and methods section the calculation procedure to calculate energy expenditure from the Tracmor is presented. Unfortunately, due to a typographical error in the cited reference (Goris *et al.* 2000 instead of Goris *et al.* 2001), the referred regression equation was hard to find. We have concluded that the regression equation can not be derived in the reference stated as Goris *et al.* (2001), since that study was performed on twenty-four healthy subjects, and the regression equation in the Goris *et al.* (2003) paper is not identical to the one stated in the Goris *et al.* (2001) article. It would have been of great value to refer to the study in which the presented regression equation was developed.

For us, as readers, this puts some uncertainty into the validity of the Tracmor’s ability to measure energy expenditure in COPD patients. As far as we understand, it has never been validated in this patient group. This also means that the conclusion of the study, the negative correlation between change in body mass and physical activity level, may be based on a method that has not been validated in this patient group. As the authors discuss, physical exercise is probably limited in COPD patients, and it surprises us that one of the patients in the intervention group had a physical activity level value at 2.44 at month 3. This indeed indicates a very high physical activity, and again the validity of the Tracmor is questioned.

Another major concern with the Goris *et al.* (2003) paper is the statistics used. The authors state that the difference between groups was tested with the Wilcoxon rank-sum test which is an impossible test to use for comparing unpaired groups. Probably the Mann–Whitney U test (which is the non-parametric test to be used for comparing two independent groups of observations) would not have detected any differences between the two groups anyway, but using a paired test for analysing unpaired data cannot pass without comment.

The authors conclude that ‘the individual physical activity level is necessary for estimation of the energy need of the patient’. This is in concordance with our conclusion in a recently published study using doubly labelled water (Slinde *et al.* 2003). However, our wish for the future is that a device measuring energy expenditure during activity is presented and validated on patients with COPD before it is used to estimate the energy need in the clinical setting.

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