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Letter to the Editor

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Reply to comment on 'Sarcopenic obesity in patients with head and neck cancer is predictive of critical weight loss during radiotherapy' (BJN-2-2024-1107)

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Dear Editor,

The authors would like to thank Topkan et al. for their interest in our recently published manuscript titled 'Sarcopenic obesity in patients with head and neck cancer is predictive of critical weight loss during radiotherapy'[1]. We would like to address their comments on our statistical methodology and terminology.

Our study aimed to:

1. Investigate the prevalence of sarcopenic obesity (SO) in patients with head and neck cancer (HNC) who had completed curative radiotherapy (± chemotherapy) and its impact on survival
2. Identify any predictors of critical weight loss (CWL).

We found CWL in 58 % of the cohort (*n* 239) and sarcopenia (CT-defined) in 43 % (*n* 177). Topkan et al. call into question statistical methodology used; however, there appears to be misinterpretation on their part with the cohorts used in the analyses. Patients with sarcopenia (*n* 177) were dichotomised by CWL category (yes/no) and compared. Among these, eighty-nine patients with sarcopenia were then dichotomised by obesity status (SO yes/no) and again compared. As reported in the Results section, paragraph 3 – 'In patients with sarcopenia, half experienced weight loss ≥ 5 % (*n* 89), and significantly more patients with SO had CWL (*n* 70 *v.* 19, $P < 0{\cdot}001$)'. This statement refers to χ^2 analysis conducted within the subset of patients with sarcopenia and CWL, not 70/116 and 19/297 as Topkan et al. suggest, rather 70/89 *v.* 19/89. In this instance, the comparison is of patients within the same group, the onus being on 'patients with sarcopenia', and this specific statistical finding relates to this subgroup of patients, not as percentages of the whole cohort. This is a comparison of categorical unpaired data, and the methodology used is appropriate as per Nayak and Hazra[2].

Our multivariable binary logistic regression model examined predictors of CWL in the entire population, with SO as a dichotomous (yes/no) predictor variable (input into the backwards stepwise model, as detailed in the Methodology section).

It is important to distinguish that the two results brought into question – elevated incidence of CWL and predictors of CWL, involved analysis of different population groups/subgroups, and this is clearly stated in our manuscript.

Regarding Table 1, again, there appears to be misinterpretation. There are not two groups in this table, rather, the second column is the subset of patients with SO (*n* 116) (these patients are present in the first column as part of the whole cohort *n* 413). This table illustrates SO group characteristics and *P*-value comparisons would only be accurate if comparing two distinct groups (SO *v.* non-SO), which is not the case here. An additional table including this comparison may have been informative; however, this was not deemed necessary as part of the rigorous peer review process prior to the publication of our paper.

No survival difference was found between SO and non-SO patients in the whole cohort (Figures 2a and 2b), and a significant difference was observed when comparing sarcopenia status (regardless of BMI) as shown in Fig. 3. The comparison of overall survival between those with SO and those without, shown in Fig. 4, was conducted on the subset of patients with sarcopenia and demonstrated better survival in those with SO, as discussed in our manuscript.

Topkan et al. suggest our use of the term 'sarcopenia' is "erroneous terminology". We disagree. The issues around defining and diagnosing sarcopenia is addressed in our Introduction, specifically mentioning radiologically defined sarcopenia as a measure of muscle depletion associated with morbidity and mortality in HNC[3–5]. While myopenia refers to clinically relevant muscle loss due to illness and at any age, the use of the terms 'sarcopenia' and 'sarcopenic obesity' is appropriate in the context of our study. Suggesting that The European Working Group on Sarcopenia in Older People's definition (EWGSOP2)[6] of sarcopenia should

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be definitive, and that the use of the term be restricted in its application is unwarranted, given its extensive use in the oncology setting. Kubrak et al. conclude that the appropriate use of the term 'sarcopenia' should be determined through peer review by the scientific community[7].

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