We received a letter to the editor from Ayubi et al regarding statistical and methodologic issues we used in our manuscript “Body Mass Index >35 as an Independent Predictor of Mortality in Severe Traumatic Brain Injury.”

It was suggested by Ayubi et al that we should include body mass index (BMI) as a continuous variable in our multivariate model rather than a categorical one, as we did. Their argument was that the predictive content of the BMI could better be used as a continuous measure.

If a continuous measurement is used in a regression model, it could be done in different ways: as a continuous measurement, as a categorical measurement, or as a dichotomous variable (which could be considered as the simplest way of using the categorical approach). Using a variable in its continuous form, however, implicitly means that there is a strictly linear effect (as Ayubi et al indicated in their response). An increase in the BMI of 25 to 25 is considered to have the same effect on mortality (or whatever is considered as dependent variable) as an increase from a BMI of 35 to 36. This is, however, rarely the case. To overcome this, sophisticated formulas could be used to improve the relationship, like spline functions. Such functional transformations would be nice for statisticians but much more difficult to explain to those nonstatisticians who apply the model. A dichotomous approach is, on the other side, a rough approach and often hides some interesting effects.

We chose a categorical approach with categories that were not derived from statistical methods but rather from clinical routines and are determined by the World Health Organization. This has the enormous advantage that the results could be well understood by the readers. Furthermore, the categorical approach can handle situations where for some values there are only little effects, while for other values the effect is much larger. Age is a good example in trauma patients: There is no effect until the age of about 55, and then there is a minor one that clearly increases with higher age.

Finally, if there is an effect associated with a continuous measurement, this effect will be detected with the categorical approach as well. Thus the approach that we used could not be criticized for not being able to identify an existing effect.

In the second part of the letter to the editor, the authors suggested applying statistical methods to find a more optimal cutoff than BMI 35, which we presented in our publication. Using their arguments earlier, it was not our aim to reduce the BMI discussion to a simple dichotomous decision. We used clinically relevant cutoff values, which were predefined by the World Health Organization. Of course, there were statistical methods to identify a maximally selected chi-square statistic that provides a potentially more predictive cutoff (demonstrated by the maximal area under the receiver operating characteristic curve, or similar), but again, such an “optimal” cutoff was not our aim. It would certainly be some odd number, and a validation study would certainly result in some other odd number. Finally it is an oversimplification of the BMI as a predictive variable. This was not our aim.

Therefore we think that the categorical approach we have chosen is 1) feasible, 2) appropriate, 3) clinically relevant, and 4) allows comparison with previous publications investigating the association of BMI and traumatic brain injury. Our publication indicates that a BMI >35 has an association with mortality and outcome. It may not be the “optimal” one in terms of statistical exactness, but it is not far from the optimal approach, and we assume that it is far better understood by the clinical neurosurgeons practitioners.

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