

## Reply to "Letter in response to the Article: The Norton scale is an independent prognostic marker for mortality in critically ill patients"

KALONJI G.<sup>1,2,\*</sup>, KNOPS O.<sup>1,2,\*</sup>, FIVEZ T.<sup>1</sup>, ENGELEN K.<sup>1</sup>, VANDER LAENEN M.<sup>1</sup>, WILLAERT X.<sup>1</sup>, BOER W.<sup>1</sup>, DE BAERDEMAEKER L.<sup>2</sup>, MESOTTEN D.<sup>1,3,°</sup>, THIESSEN S.<sup>1,3,4,°</sup>

<sup>1</sup>Dienst Kritieke Diensten ZOL Genk, 3600 Genk, Belgium; <sup>2</sup>Dienst Anesthesie UZ Gent, 9000 Gent, Belgium; <sup>3</sup>Departement Neurowetenschappen UHasselt, 3500 Hasselt, Belgium; <sup>4</sup>Departement of Cardiovascular Sciences KULeuven, 3000 Leuven, Belgium.

\*contributed equally as first author.

°contributed equally as last author.

Corresponding author: Oliver Knops, Schuurstraat 59, 9000 Ghent, Belgium. E-mail: oliverknops.ok@gmail.com

Dear Editor,

We would like to thank Dr. Yadav for the interest in our work and for the thoughtful and constructive comments regarding our article. We particularly appreciate the careful review of our manuscript and the opportunity to clarify several points.

First, regarding the number of included patients, a total of 1889 patients were analyzed, as correctly stated in the abstract and tables. The mention of 1888 patients in the Results section was a typographical error, for which we apologize. In addition, we acknowledge a transcription error in Table I. Specifically, 1035 (60%) of the 1706 patients with a Norton score >14 were male, whereas 90 (49%) of the 183 patients with a Norton score ≤14 were male. These values were inadvertently interchanged during table preparation. We are grateful to the author for identifying this inconsistency. With respect to Table II, percentages were rounded to whole numbers for clarity, which may explain minor discrepancies between absolute numbers and reported percentages.

Second, this study was designed as a retrospective cohort study, and the only exclusion criterion was the absence of a recorded Norton score. Approximately 17% of patients did not have a documented Norton score, as this assessment was not systematically or mandatorily recorded in our ICU at the time of data collection. As correctly noted, this may introduce selection bias. This limitation has been explicitly acknowledged and discussed in the Discussion section of our manuscript.

Third, we agree with the comment that the Norton scale was originally developed as a tool for pressure ulcer risk assessment rather than as a prognostic instrument for mortality. The use of the Norton score in this context should therefore be considered exploratory and hypothesis-generating. We believe that our findings suggest a potential association between the Norton score and mortality in critically ill patients, which warrants further investigation in prospective and multicenter studies.

Finally, we concur with the interpretation that the Norton score may, at least in part, reflect patients' baseline functional status and frailty, both of which are well-established determinants of outcome in critically ill patients. In this regard, the observed association between lower Norton scores and increased mortality may be mediated by underlying frailty rather than representing a direct prognostic effect of the score itself. This perspective has been addressed in the Discussion. We also acknowledge, as highlighted, that no validated frailty measures were available in our dataset, which represents an important limitation. Future studies directly comparing the Norton score with established frailty scales would be of particular interest.

We thank Dr Yadav once again for the constructive comments, which have helped to further clarify and contextualize our findings.

Sincerely,

Thiessen Steven and Mesotten Dieter

[doi.org/10.56126/77.2.02](https://doi.org/10.56126/77.2.02)