

Review of “Quantifying and predicting austral summer mesopause height with a temperature-based upwelling proxy” by Zhang et al.

The paper describes the relation between a temperature index, T80, and mesopause height of the summer hemisphere at high latitudes. The index, along with the mesopause height, is derived through the combination of SABER and MLS observations. Based on the correlation coefficients and further sensitivity study, especially the one between T80 and the mesopause height, the author argues that prediction of the summer mesopause height at high latitudes in the southern hemisphere is possible through the measurements of T80. The work also highlights the so called “dipole response” in the mesopause region during the summer of southern hemisphere, which I think is a new terminology for a well-known phenomenon/process. Furthermore, the description is somewhat hand waving and a more scientifically thorough description is needed. The author conducted considerable amount of work, including the data from two satellite and calculations on several parameters. But the efforts of data processing of the two instruments are missing. It is critical, because the two instruments have very different sampling scheme, thus, the biases due to long-period waves are different. This and the long-term consistency of the instruments need to be handled carefully, when combining the two measurements, and should be addressed in the early part of the paper. The close relation between T80 and mesopause height at high latitudes is interesting, but I would not conclude it can be utilized to forecast the mesopause height in the coming month. On monthly basis, there are little changes of mesopause height once the dynamic and thermal balance is achieved. Therefore, this claim is somewhat misleading, in my opinion. The author also states that “... the summer mesopause height in the SH is almost perfectly controlled by the T80 index...” in the conclusion, which is an overstatement, I think. Both T80 and mesopause height are symptoms, not the source that “controls” and leads to these symptoms. Thus, the author needs to modify some of these statements. The other results presented in the paper are mostly expected and/or well known, but, overall, the paper is decent. It does, however, need some clarification on the data processing. The most critical T80 is loosely defined in figure caption of Fig. 1, which is quite strange. I would like to see detailed definition and algorithm for the derivation of this index. The author refers Zhang et al., 2025c, but I think it is important to have it clarified in the early section in this manuscript. In addition, how the mesopause height is defined and calculated? This work is based on combined SABER and MLT observations. Yet, only SABER is mentioned in the abstract and conclusion sections.

Minor technical issues:

Line 94, “zonal-mean temperature near 80 km” need to be defined in details.

Line 113-114, this statement is confusing, needs to be rephrased.

Line 125, in caption of Fig. 1, it should be red-solid lines and blue-dashed line, respectively. What does the black solid line represent in a, b and c?

Line 153, the word “perfect” is used in several places in the manuscript. I would not use it in a scientific publication.

Line 175, in caption of Fig. 3, “red diamonds” and “blue dots”

Line 245, in captions of Fig. 7 and 8, please define the red and blue solid lines in the figure.

