

Dear Editor,

We would like to say many thanks the Referee for taking the time to review our manuscript and valuable recommendations. We have tried to follow the referee remarks and have utilized all of them.

In the following, we address the comments point by point and show how the manuscript has been changed accordingly to the comments. Below comments by Referees are in red, our responses are in black, the changes in the manuscript are in blue and brackets.

Response to the comments on the paper by Referee 1

The title does not accurately reflect the essence of what has been done in this article. One might get the impression that the equilibrium boundary was taken from satellite data. In reality, this characteristic was found using SABER data. Should be corrected, for example: "...: long-term evolution of the boundary determined (OR derived) using 20-year satellite observations".

The title has been corrected to "**Boundary of nighttime ozone chemical equilibrium in the mesopause region: long-term evolution determined using 20-year satellite observations**".

I would like to note that the nighttime ozone equilibrium boundary was investigated in previous papers of the authors using 3D MLT modelling. There the criterion was also proposed, which in this work is applied to determine the altitude position of this boundary, using already the data of real measurements. It is well known that any model is an idealized representation of the real reality and, in principle, may not take into account some important features of the natural object, poorly formulated mathematically. In this regard, a very important question that needs to be clarified at least in the discussion. Can the authors present any other indications of the nighttime ozone equilibrium boundary in the SABER profiles or in the O and H profiles reconstructed from these data?

In the Discussion of revised manuscript, we added short discussion concerning this recommendation (see lines 386-392):

“Finally, let us briefly discuss other qualitative indicators of the NOCE boundary, which could be found in the SABER database. As mentioned above, Kulikov et al. (2019) showed that the nighttime O SABER profiles are correct above the NOCE boundary, whereas the H profiles hold within the whole pressure interval. Kulikov et al. (2021) demonstrated that, in the altitude range of 80-85 km, many H profiles have a sharp jump in concentration when it increases from $\sim 10^7 \text{ cm}^{-3}$ to $\sim 10^8 \text{ cm}^{-3}$. Our analysis with the criterion (9) shows that the altitude of these jumps can be used as a rough indicator of the NOCE boundary.”

There are quite a large number of figures (20) with different number of panels (from 1 to 20) in the article with a relatively small volume of text. For better structuring of the article, some of the figures should be omitted or merged, for example: (4, 8, 13 and 17), (6 and 15), (7 and 16), (10, 12, 19 and 20).

In the revised manuscript, the Figures were reorganized according to the Referee note.

Figures 1-3 show variations of O and H normalised to some mean daily values. It is not clear what these values are. If these are averages over the entire range of altitudes, then the figures should show known maximums of O and H, but this is not present in the figures. Apparently different daily average O and H values were used for each altitude. Please clarify this issue. Also, the figures show white spots where the normalised concentrations appear to fall below 10⁻⁶. Apparently the range of variation shown needs to be increased.

We have added the necessary clarification to the revised manuscript (see lines 157-159):

“In order to focus attention on diurnal oscillations, the concentrations are normalized by mean daily values, which were calculated as a function of altitude. These daily average O and H values were different for each altitude.”

Also, the Figures were corrected according the Referee note.

Lines 315-317. The authors write "Basing on daytime O and H distributions in the mesopause region obtained in Kulikov et al. (2022), we calculated O/H in summer and winter." It would be nice to provide figures, which confirm that "this ratio at middle latitudes is remarkably less than in winter".

In the revised manuscript, we have added new Figure (Figure 14), which confirms this statement.

I have my doubts that quite a few of the many instances in which the articles are mentioned correctly. Furthermore, there are a number of questions about the use of English expressions. Therefore I strongly recommend checking the text of the article with the help of a professional translator.

The revised manuscript was verified and corrected by a professional translator.

Other changes are related to the recommendations and demands of other referee.

Thank you for taking your time to review our manuscript.

With respect,

Michael Kulikov, Michael Belikovich, Alexey Chubarov, Svetlana Dementyeva, and Alexander Feigin