Below Referee's comments are marked by red, our responses are in black.

Uncertainties are briefly discussed in the last paragraph of Section 3. Basically, the authors refer to the uncertainty analysis by Fytterer et al (2019) which they have re-peated. This needs to be extended. Please summarize the ideas of the uncertainty analysis (based on Fytterer et al.) and state in particular which reaction steps and reaction rates are critical for the uncertainty. This extended paragraph should be moved to the end of Section 2, i.e. the uncertainties of the method should be discussed before the results are presented.

We agree with the comment. In the revised manuscript, the uncertainty analysis was extended. First of all, we added the new Table with the list of systematic uncertainties of measured data and constants and corresponding uncertainties in derived $O(^1D)$ local concentration. The corresponding paragraph was added by a couple of sentences:

«The systematic uncertainty of retrieved data is defined by uncertainties in $VER_{2\,\mu m}$, O_3, T measurements, and in the rates of chemical and physical processes included in the OH(v) model. We reproduced the analysis presented in Fytterer et al. (2019) (see Sect. 3.4) and took into account the uncertainties of measured data and rate constants which are shown in Table 2. The third column of the Table demonstrates the uncertainties' individual impact at derived $O(^1D)$ local concentration. It can be noted that the most critical for $O(^1D)$ are the uncertainties in T, rates of reactions (2-3), Einstein coefficients for the v =8-9 states, and $VER_{2\,\mu m}$. The total systematic $O(^1D)$ uncertainty was obtained by calculating the root-sum-square of all individual uncertainties. It was found to vary in the range of (37-52)% depending on the pressure level. Due to averaging, the random error of data presented below is negligible.»

Furthermore, the paragraph was moved to the end of Section 2.

Validation of the O(1D) retrievals is beyond the scope of this manuscript. Nevertheless, I would like the authors to comment on perspectives towards a future validation of these SABER retrievals.

We agree with the comment. In the revised manuscript, we added a couple of sentences in the end of Sect.4 (Discussion and Conclusion):

«The analysis of this impact should be carried out with the use of a global 3D chemical transport model of the mesosphere – lower thermosphere. Additionally, it may indicate measurable characteristics of this region that could indirectly confirm the results obtained in this article. In principle, direct evidences of $O(^1D)$ layer existence in nighttime mesopause can

be established by *in situ* measurements of $O(^1D)$ airglow at 630 nm which can be carried out, for example, as a part of future WADIS rocket sounding mission (Strelnikov et al., 2019; Grygalashvyly et al., 2019). More detailed analysis is out of this short article scopes.»

Some minor comments:

Line 9, 11: remove "the" before "values" (3 times)

Line 13: "a useful data set"

Line 14: "on the chemistry"

Line 31: replace "constant" by "a continuous"

Line 34: "via the process"

Line 54: "use the known"

Line 77: "Half a year"

Line 78: "A similar pattern"

Line 83: "There is a pronounced"

Line 84: "found similar features"

Line 84-85: I do not understand the notation "nighttime ozone chemical equilibrium boundary", in particular the term "boundary". Please clarify.

Line 86: "of the stratospheric"

Line 93: remove "the" before "values" (2 times)

Line 94: remove the word "correspondingly

"Line 98: replace "summarized" by "the total"

Line 108: "use of a global"

In the revised manuscript, all these points were corrected accordingly.