Dear Editor,

We would like to say many thanks the Referee for second review of our manuscript and providing valuable recommendations. We took into account all the remarks of Referee and, to the best of our ability, implemented the corresponding changes in the manuscript.

In the following, we address the comments point by point and show how the manuscript has been changed according to the comments. Below we use a certain color notation: comments by Referee are in red, our responses are in black, and the changes in the manuscript are in blue (placed inside the quotation marks).

**Response to the comments on the paper by Referee 2**

1. The paragraph at lines 390-401 is somewhat hard to follow. However, among other things, it seems to be saying that the summer/winter differences in O in MLT midlatitudes is due to photochemistry. There is ample evidence that this difference is driven primarily by the seasonal flip in the global-scale circulation. For example, see Wang et al. (2023a,b) and references therein. Wang, J. C., Yue, J., Wang, W., Qian, L., Wu, Q., & Wang, N. (2022). The lower thermospheric winter-to-summer meridional circulation: 1. Driving mechanism. Journal of Geophysical Research: Space Physics, 127, e2022JA030948. https://doi.org/10.1029/2022JA030948 Wang, J. C., Yue, J., Wang, W., Qian, L., Jones, M., Jr., & Wang, N. (2023). The lower thermospheric winter-to-summer meridional circulation: 2. Impact on atomic oxygen. Journal of Geophysical Research: Space Physics, 128, e2023JA031684. https://doi.org/10.1029/2023JA031684

In the revised manuscript, we have improved this paragraph to make it clearer (see lines 408-427 in Discussion):

« As noted, Figs. 5-6 represent an interesting peculiarity. At the middle latitudes summer $z_{{HO_2}_{sh}^{eq}}$ and $z_{OH_{sh}^{eq}}$ are remarkably higher than winter ones. For example, in February $z_{{HO_2}_{sh}^{eq}}$ at 60ºN is ~ 84 km, whereas the one at 60ºS is ~ 74 km. Recently, Kulikov et al. (2023b) found such a feature in the evolution of nighttime ozone chemical equilibrium boundary (Fig. 5 there), derived from SABER/TIMED data. The study showed that the boundary closely follows the transition zone that separates strong and weak diurnal oscillations of O and H (see Figs. 1-3 and 13 in Kulikov et al. (2023b)). Above the zone the behavior of components is dynamically driven and seasonality is the result of change in global-scale circulation, vertical advection being the primary factor according to Wang et al. (2023). In the transition zone and below O and H concentrations change by orders of magnitude during the night driven by photochemical processes. Kulikov et al. (2023b) studied the photochemistry at these altitudes and its seasonal dependence. It was shown analytically that nighttime O decreases with the characteristic time scale $\tau_O = O/|dO/dt|$ proportional to the $O/H$ value at the beginning of the night (see Eq. (13) there). At the same time, according to the distributions derived from SABER measurements $O/H$ during summer daytime (and thus also at the beginning of the night) at the middle latitudes is remarkably less than the one during winter daytime (see Fig. 14 there). Consequently, summer values of nighttime $\tau_O$ below ~ 84 km are significantly shorter than winter ones, so summer O during the night decreases much faster than in winter. In our case lifetimes of $HO_2$ and OH are proportional mainly to $\frac{1}{O}$ (see Eqs. (11) and (19)), so, following the approach described in Section 2, the summer rise of $z_{HO_2{}_{sh}^{eq}}$ and $z_{OH_{sh}^{eq}}$ at the middle latitudes can be explained by the season difference in O diurnal photochemical evolution at these altitudes.»

Note we consider here the O diurnal evolution at ~74-84 km, where there are deep photochemical oscillations of O caused by the diurnal variations of solar radiation, when the difference between daytime and nighttime O values can reach several orders of magnitude. At this time, Wang et al. (2022, 2023) consider mainly the overlying region, where dynamical processes dominate. In particular, Wang et al. (2023) pointed «that the vertical advection is the dominant mechanism in redistributing O at altitudes between 84 and 103 km» (see Abstract there) or «Local vertical advection associated with the lower-thermospheric winter-to-summer meridional circulation is found to be the primary driver for redistributing O between 84 and 114 km» (see Conclusions there). The fact is duly noted in the new version of the paragraph.

2. Readers might find the ex post facto revision of critOH (lines 402-415) confusing. Why not introduce the correction at the point of the original derivation around line 340?

In the revised manuscript, this paragraph was moved to Section 6 immediately below the original derivation of $Crit_{OH}$ (see lines 351-363).

3. As far as I could see, the revised text has no mention of the presence of a supplemental document and does not refer to any specific figures from the supplement. Are these figures helpful in interpreting the results? Is this document even necessary?

The Supplement was organized due to requirements by other Referees (# 1 and 3). In the revised manuscript, we connected the figures from Supplement with the text of manuscript,

lines 234-235: «The complete figures for $HO_2$ sources and sinks for every month (all 12 panels) are given in Supplement (Figs. S3-S11)»

lines 257-259: «The complete figures for OH sources and sinks for every month (all 12 panels) are given in Supplement (Figs. S12-S24).»

lines 372-373: «(see Figs. S1-S2 and S25-S26 in Supplement)» line 390: «(see Fig. S27 in Supplement)»

line 407: «(see Fig. S28 in Supplement)»

Thank you for taking your time to review our manuscript.

With respect,

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