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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A meridional structure of static stability and ozone vertical gradient around the tropopause in the Southern Hemisphere extratropics" *by* Y. Tomikawa and T. Yamanouchi

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The authors greatly acknowledge the referee's careful reading and valuable comments. We have revised our manuscript as much as possible following the reviewer's suggestion. Our final response is described as follows. The referee's comments are presented in Italics.

Response to major comments:

Referee comment: 1. Shortwave ozone heating:

It is true that the shortwave ozone heating is proportional to sunshine hour. However,

no shortwave calculations are presented that show that the difference between the seasonal variations of sunshine hours in Fig. 2 is large enough to matter in principal. Presumably, there is not enough ozone around the tropopause (for all seasons) to produce appreciable shortwave heating contributions to the temperature structure. Maybe this is what the authors intend to show? I think the authors need to state what the initial hypothesis is, e.g. why they think that shortwave ozone heating could contribute to TIL formation in polar summer (if that was the initial motivation).

More importantly, it seems that the longwave heating due to ozone has a much greater potential to affect the temperature structure around the tropopause. This component of the full radiative effect due to ozone is not discussed in the manuscript (note: Randel et al. (2007) find that even at midlatitudes the longwave heating dominates the full radiative heating due to ozone). The fact that this more important radiative contribution due to ozone is not discussed confuses the author's point about the shortwave ozone effect.

Author response: Randel et al. (2007) have shown that a rapid increase of ozone concentration across the tropopause induced significant heating above the tropopause through longwave (65%) and shortwave (35%) absorptions at midlatitudes. Although the shortwave contribution was smaller than the longwave, it is not small enough to be neglected. In addition, the sunshine hour shows the largest seasonal variation in the polar region. If the shortwave absorption due to ozone contributes to the TIL formation, it should be observed in the seasonal variation of the TIL in the polar region. Thus the polar region is the most appropriate place to investigate the contribution of shortwave absorption due to ozone. We added further statement on the work of Randel et al. (2007) and the motivation to focus on the shortwave absorption due to ozone in the polar region in section 1.

As the referee pointed out, the longwave absorption due to ozone may have a significant contribution to the formation of TIL. Thus we revised the manuscript to clearly mention that this manuscript dealt with only the shortwave ozone heating and longwave ozone heating may significantly contribute to the formation of TIL.

Referee comment: 2. Ozone transport from the subvortex region.

I agree that Fig. 5 allows for the interpretation that ozone-depleted air is transported (mixed?) from the subvortex region to mid-latitudes. However, the evidence presented by the authors (cross sections of ozone vertical gradient derived from a few stations) is certainly not enough to claim that they "clearly demonstrated" this transport. In fact, the authors remark, that they are not able to distinguish the above transport pathway from air transported from the tropical upper troposphere. Given that the residual mass transport is from the tropics to midlatitudes, isn't this second transport pathway the more likely one?

The discussion here is, again, very speculative. Furthermore, it is not clear what the authors are ultimately trying to show in this part of the analysis. In other words, how is this part related to the rest of the paper (see also 3.)?

Author response: See our response to the next comment.

Referee comment: 3. Storyline.

It is not clear to me what the overall storyline should be? For the most part the authors focus on the SH TIL structure and its seasonal cycle. But the discussion around Fig. 5 focuses on lower stratospheric ozone transport. Overall, the manuscript reads somewhat like bits and pieces from a lab notebook. I think the authors need to do a better job at focusing the material around a storyline (or two if necessary).

Author response: As the referee pointed out, section 3.3 included much speculation and dealt with a different topic from other sections. We decided to delete this section from the manuscript following the referee's suggestion.

Response to minor comments:

Referee comment: - One of the motivations to use the ozone tropopause instead of

the thermal tropopause, is that the latter becomes ill-defined in polar winter. In other words, stratification does not show an abrupt enough transition at the tropopause in polar winter. Given that the TIL represents a layer of anomalously strong stratification just above the tropopause, it is trivial that the TIL disappears due to the ill-defined thermal tropopause in polar winter. There is somewhat of a chicken-and-egg flavor here.

Author response: The ozone tropopause based coordinate demonstrated that a TIL disappears but a clear transition of static stability between the troposphere and stratosphere still exists even in the Antarctic winter. This is not a trivial feature, because the ozone tropopause is defined regardless of the thermal structure.

Referee comment: p19176 L13: this sounds like somehow the polar-night jet is formed due to the TIL - should be reworded

Author response: "is formed" was replaced by "exists" following the referee's suggestion.

Referee comment: L16: "referred to as ozone vertical gradient" can be removed

Author response: This sentence was removed.

Referee comment: p19177 L7/8: one of the advantages of the thermal tropopause is that it can be applied globally (as opposed to the PV tropopause for example)

Author response: We added a statement on the advantage of the thermal tropopause in this sentence following the referee's suggestion.

Referee comment: L20/21: there's also an asymmetry in the N2 structure between cyclones and anticyclones, such that even if they occupied the same area, the stability maximum in anticyclones would dominate

Author response: We deleted "and occupy larger area than cyclones" following the referee's suggestion.

Referee comment: p19178 L22: "radiative role of ozone" - only shortwave contributions are presently discussed

Author response: “radiative role of ozone” was replaced by “a role of shortwave absorption due to ozone” following the referee’s suggestion.

Referee comment: p19179: it should be discussed somewhere here, that the ozone tropopause is less well established than the thermal tropopause, meaning the precise values to be used for the thresholds involved are less well tested

Author response: As the referee pointed out, the definition of ozone tropopause has not been well tested compared to the thermal tropopause. On the other hand, several papers (Bethan et al., 1996; Wirth, 2000; Ravetta and Ancellet, 2003; Sivakumar et al., 2006; Tomikawa et al., 2009) dealing with the ozone tropopause have been published. We added both statements in section 2.

Referee comment: p19181 L10: true, but there are so few stations in the SH, that the missing longitudinal structure could still be an issue

Author response: We added some statement on the importance of longitudinal structure in the manuscript following the referee’s suggestion.

Referee comment: p19182 L10: Eq. 1 is exact for log-pressure coordinates

Author response: We added a statement on the log-pressure height in section 3.2 following the referee’s suggestion.

Referee comment: L17: "meridional circulation" - does this refer to the residual circulation? If yes, than this does not represent a conservative process.

Author response: We removed section 3.3. See the response to major comment 3.

Referee comment: L19-21: motivation doesn’t work: N2 is generally not conserved for adiabatic motion, neither is the vertical ozone gradient, so the connection between N2 and the ozone gradient is not very clear, other than from a radiative perspective

Author response: We removed section 3.3. See the response to major comment 3.

Referee comment: p19184 L14: I don’t find this study as "comprehensive" as the authors claim here. It certainly is the first study (as far as I can tell) quantifying certain properties related to the TIL from SH soundings. However, all mechanisms discussed are presented in a speculative way without support from detailed analysis.

Author response: We deleted “comprehensive” following the referee’s suggestion.

References:

Bethan et al., 1996: A comparison of ozone and thermal tropopause heights and the impact of tropopause definition on quantifying the ozone content of the troposphere, Quart. J. Roy. Meteorol. Soc., 122, 929–944.

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Tomikawa et al., 2009: Characteristics of tropopause and tropopause inversion layer in the polar region, SOLA, 5, 141–144.

Wirth, 2000: Thermal versus dynamical tropopause in upper-tropospheric balanced flow anomalies, Quart. J. Roy. Meteorol. Soc., 226, 299-317.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 19175, 2010.