

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

Anonymous Referee #2

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The authors analyze the UTLS vertical structure of temperature and ozone from 10 SH mid-latitude and polar ozonesonde stations. They discuss the seasonal cycle and latitudinal structure in ozone tropopause based coordinates (relative to the ozone tropopause), concentrating on the tropopause inversion layer (TIL). Shortwave heating due to ozone is ruled out as a formation mechanism of the SH polar summer TIL. Similarities between the seasonal cycles of the vertical gradient of ozone and static stability are discussed.

Overall this is a useful analysis, especially given the scarcity of routine ground-based observations in the SH. The manuscript is generally well-written and should be of in-

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terest to readers of ACP. I have a few concerns as outlined below (some major, some minor) that should be addressed before publication. The major comments concern the mostly speculative nature of most of the discussion by the authors.

Major Comments:

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.

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

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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.)?

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).

Minor Comments:

- 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.

p19176

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

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

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L7/8: one of the advantages of the thermal tropopause is that it can be applied globally

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(as opposed to the PV tropopause for example)

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

p19178

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

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

p19181

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

p19182

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

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

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

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.

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