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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ozone seasonality above the tropical tropopause: reconciling the Eulerian and Lagrangian perspectives of transport processes" *by* M. Abalos et al.

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The authors acknowledge the review of the anonymous referee #2 and address the minor comments below.

General comment:

"One overall comment is about the authors' focus of ozone. To highlight in-mixing, transport process would be important and ozone is not necessarily appropriate since the chemistry process should be considered even in the lower stratosphere. Some other tracer species or potential vorticity would be better to analyze. The authors

should say some words about their focus on ozone seasonality."

» Response: In this article, the apparently contrasting results regarding the ozone seasonality near the tropical tropopause are considered. There is no similar controversy in the literature associated with the annual cycle observed in other tracers. In addition to ozone, Ploeger et al. (2012) analyze the seasonality in carbon monoxide (CO) and water vapor. Their results for these tracers show a much reduced role of in-mixing, due to the smaller meridional gradients in CO as compared to ozone. The same amount of air in-mixed from the extratropics leads to very different changes in tropical mean concentrations in the various tracers. This differentiated behavior highlights the importance of analyzing each tracer individually. Consequently, while potential vorticity provides valuable information on transport processes, it is necessary to examine each tracer separately in order to assess the specific impact on tropical mean concentrations.

"I am not sure about the policy of ACP, but I feel that this paper is a kind of a discussion part of a full paper which is assumed rather longer than this. Except the shortness of this paper, I found that it is clearly written and that it gives insight for understanding some signatures of the tropical tropopause layer. Therefore I recommend that this paper is acceptable to ACP after some minor revisions based on the following comments which the authors might consider to take into account."

» Response: The authors consider that ACP is the right journal for this article, as it complements and refers to the recent publications Abalos et al. (2012, ACP) and Abalos et al. (2013, ACPD). At the same time, this paper cannot be considered as a comment to one specific paper, but rather related to a group of papers (some of which are not published in ACP).

Specific comments:

"2. Model data and method: There is no description about the vertical grid points for the chemistry climate model WACCM and the Lagrangian 1-D model. Some related information can be seen in the figures, but it would be better to give the grid point information for the vertical resolution."

» Response: The vertical resolution in WACCM is between 1.1 and 1.4 km, this has been included in the revised version of the manuscript (line 11 page 19298). The 1D-model uses an integration step of 0.5K potential temperature, and is based on ERA-Interim data which has been interpolated to isentropic levels with 10 K vertical spacing between 360 K and 500 K.

"Page 19295, line 25: α_N and α_S are used but not defined in the following. Actually there are some explanations around Page 19297 and line 1, but it is too far from Eq. 1. Also we could not find clear definition of the two parameters including actual values they used. In other way to ask, are these two parameters constant and same for the two hemispheres? They are somewhat magic numbers to explain in-mixing in the model. Similarly there is no detailed description about χ_N and χ_S, but these should be also very important to represent seasonal variation in the TTL due to in-mixing."

» Response: A definition of α_N and α_S has been included in line 6 on page 19296. χ_N and χ_S are defined in lines 5-6 of page 19296 as "mean NH and SH mid-latitude annual mean mixing ratios". We have now specified that these concentrations are taken from a HALOE based monthly climatology (see text). We further checked that using monthly varying extratropical mixing ratios in the 1D model causes no substantial change of our results and conclusions, and we mention this in the revised manuscript on page 6 line 10.

"Page 19300, line 4- : The authors stress overall similarity. I agree a part of their discussion, but do not fully. During boreal winter there is Australia monsoon, and there seems a sign in the WACCM data but not in the 1-D model data. Also the duration of the boreal summer maxima is not well reproduced in the 1-D model. I understand that it is not a discussion in this paper, but this may be related to the previous comment about the specification of α_N and α_S, and χ_N and χ_S."

» Response: As emphasized in the manuscript (lines 12-18 page 19298 and lines 8-9

page 19300), the models compared here are extremely different (WACCM is a complex coupled chemistry-climate model and the 1D model is a conceptual one-dimensional simplification of transport), and thus exact correspondence is not expected. Moreover, the magnitude represented in Figure 3 (in-mixing) is derived in a very different way for each model. While in the 1D model in-mixing is estimated as the net input from the extratropics given the rates α_N and α_S and the meridional gradients χ - χ_N and χ – χ_S, in WACCM it is obtained averaging the TEM net horizontal transport in the tropics. The key point of this comparison is that, despite these huge differences, the most relevant features are clearly observed in both models: 1) large in-mixing due to the summer monsoon near the tropopause and 2) reduced seasonality in the tropical lower stratosphere, with somewhat larger in-mixing in boreal winter and spring.

"Figure 4: It is somewhat difficult for me to understand the detailed messages from this Figure. Some explanations are written in the figure caption, but for easy reading and clarity more detailed descriptions would be preferable in the main text."

» Response: We have changed Section 3.3 in the revised version of the manuscript in order to express more clearly the messages from Figures 4a and 4b.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 19291, 2013.