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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ice injected up to the Tropopause by Deep Convection: 1) in the Austral Convective Tropics" *by* Iris-Amata Dion et al.

Anonymous Referee #2

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The study by Iris-Amata Dion and coauthors addresses a topical issue of water transport in the tropical tropopause layer during Austral convective season. The analysis makes use of satellite observations of ice water content (IWC) by MLS and precipitation (Prec) by TRMM in consideration of twice daily sampling of the former and a full diurnal coverage of the latter. Having established a reasonably high spatial correlation between IWC and Prec, the authors propose an original method for reconstructing the diurnal cycle of IWC from MLS observations, enabling a quantification of the diurnal amount of ice convectively injected into the TTL. The method is validated using partial path IWC measurements by SMILES instrument during an individual convective season. The result are reported separately at two levels representing upper troposphere (146 hPa) and tropopause level (100 hPa) as well as for different land and oceanic

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Discussion paper

zones in the southern tropics considered as most convective regions. The results of analysis indicate a much stronger convective transport of ice above continental regions compared to oceanic ones. The largest amount of injected ice is found for the land regions of Maritime continent. A major strength of this study is the synergistic approach making use of different satellite observation techniques, which enables acquiring add-on value information on the IWC diurnal cycle from the twice-daily MLS observations. The manuscript is logically structured, the description of data and methods is comprehensive and the graphical material is good quality. The conclusions are in line with what is established by the analysis, and overall the study represents a valuable contribution to the topic of convective transport of water. There are however certain shortcomings which require revision or clarification. The detailed remarks are listed below.

General remarks.

* The introduction places the study into the context of stratospheric water vapour and related problematics. With that, in my opinion, the obtained results are of limited relevance for the control of stratospheric water budget. The stratospheric entry of water is mostly driven by minimum temperature at the Cold Point Tropopause and, to a much smaller extent, by injection of ice into subsaturated environment above this level. The CPT level corresponds to 82 hPa level of MLS (as can be clearly seen in Fig. 5a), whereas the analysis is performed for 146 and 100 hPa levels. The ice water detrained below CPT may have very little or no impact on stratospheric water: even if the injected ice crystals sublimate before settling down, the amount of water vapour ascending into the stratosphere would be limited by the colder temperatures at CPT level. Moreover, large-scale convection may lead to additional cooling of CPT at diurnal and intra-diurnal time scales thereby further limiting the cross-tropopause transport of water. I believe the above considerations should be discussed in the context of the role of deep convection of stratospheric water. It would also be useful to compare the results regarding IWC obtained for 100 hPa with those for 82 hPa level.

* On the base of comparison between TRMM Prec and TRMM OPF diurnal cycles,

Prec is shown to be a good proxy of deep convection during its growing phase. Could you clarify why the OPF data as such could not be used for the IWC analysis?

Specific remarks.

l. 18-19. The purpose of the method is missing in this sentence, i.e. "We propose a method for... using..."

l. 165. The title of Sect. 3.1 should be more specific.

l. 180. The statement regarding a ubiquitous subsaturation in the Austral convective regions is surprising. Consider that the RH product of MLS may have a substantial dry bias at 82 hPa since it is based on MLS temperature profiles. The latter, in turn, do not resolve the sharp temperature minimum at CPT, which leads to underestimation of RHI at this level. While the geographical distribution of MLS RH could be fairly accurate, the quantitative statements based on these data should be avoided. A correct way to infer RHI values from MLS measurements would be to compute them from WV profiles of MLS and temperature profiles from a reanalysis data set with sufficient vertical resolution in the TTL, e.g. MERRA-2.

l.203. Since Fig. 5a shows the profiles at the native MLS pressure levels, a correct inference on the CPT pressure would be 83 hPa $\pm$ half width of MLS weighting function.

l.204-207 and Fig. 6. What is the relevance of this information in the context of Sect. 3.2 entitled "Water budget in the UT and LS"? Please clarify.

l.282-283. The section title refers to UT level, whereas the first line reads "...convection reaching the tropopause..."

Sect. 4.2. For consistency, the description of SMILES instrument should be introduced in Sect. 2. Please clarify how the full diurnal coverage is ensured with ISS platform.

Figure 1. Color scales of 1a and 1b should be the same. Figure 1c is a duplicate of 1a.

Technical corrections.

l.175. Maximal => maximum

l.213. Convectively-lifted?

l.221. Spatial correlation

l.223. Consider rephrasing the sentence ending with "...WV and IWC in the UT and Prec is analyzed".

l.237-238. Broken sentence

l.376. try "before reaching"

l.417 sentence unclear

l.879. 30 N => 30S

l.882. sentence unclear

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