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Interactive Comment

Interactive comment on "Cold trap dehydration in the Tropical Tropopause Layer characterized by SOWER chilled-mirror hygrometer network data in the Tropical Pacific" *by* F. Hasebe et al.

Anonymous Referee #2

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Summary:

In this paper the authors attempt to determine whether frost point hygrometer measurements of water vapor in the tropical tropopause layer (TTL) of the western Pacific are consistent with the cold-trap hypothesis of Holton and Gettelman [2001]. This hypothesis attributes the dehydration of air parcels in the TTL to exposure to cold temperatures resulting from large-scale adiabatic ascent along the parcel paths as opposed to processes such as deep convection. The authors take observations of water vapor mixing ratio (OMR) from balloon soundings of the Soundings of Ozone and Water in the Equatorial Region (SOWER) project and compare these to estimates of the minimum

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saturation mixing (SMRmin) ratio along seven-day isentropic back trajectory bundles initiated at each of the sounding profiles.

Before proceeding to their statistical analysis of the results of the OMR/ SMRmin comparison, the authors present two case studies of soundings at Biak, Indonesia [1.2 S, 136.1 E]. Both showed layers of supersaturation coinciding with enhanced backscatter from ice particles from the Biak lidar, and one also showed a prominent feature at 455 nm with the COBALD backscatter sonde. In this latter case, RHice reached as high as 179% in the cirrus layer.

The statistical analysis is based on the argument that individual values of OMR should not exceed the homogeneous nucleation threshold of 1.6 X SMRmin. Conducting their analysis at six potential temperature levels ranging from 350 K to 400 K, they find that at 370 K and 380 K this relationship appears to hold, while at 360 and 365 K, many OMR values exceed the homogeneous nucleation threshold. They thus argue that a significant fraction of the air parcels at these two lower levels may still be undergoing dehydration at the time of observation, while at the two higher levels, the dehydration was essentially complete. At 400 K, the majority of the air parcels show OMR< SMRmin which they suggest is a consequence of diabatic heating; a predominance of the same at 350 K they also attribute to descent driven by adiabatic cooling.

Overall comments:

This paper draws upon the SOWER program's unique set of water vapor sounding data from the equatorial Pacific to address the question of TTL dehydration in a straightforward and direct manner, and the authors do an extremely careful job of preparing the sounding data to remove instrumental noise and to characterize the error structure of the measurements. For this they should be applauded, and we can place a high degree of confidence on the mixing ratio values they use in their analysis. They also provide clear, albeit anecdotal, evidence of high supersaturation within layers of ice particles in the TTL.

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The main thrust of the paper however is to establish via statistical analysis the relationship between observed mixing ratios and air parcel temperatures upstream, but the results are not clear cut. While, the scatter diagrams in Fig. 14 certainly suggest a relationship involving homogeneous nucleation at 370 and 380 K in particular, the authors appear to shy away from making a clear statement of how the results taken as a whole are consistent with the cold-trap hypothesis per se. While I think there are questions about the reliability of the trajectory calculations that should have been addressed in the paper, the lack of a clear set of conclusions I think can be attributed to the absence of a statement of a set of testable hypotheses at the beginning of the paper. Instead, the concept of the 'efficiency' of dehydration is introduced early but never defined, and it is unclear how the evidence will be used to assess this efficiency, much less discriminate between cold-trapping via homogeneous nucleation and other processes affecting the water vapor mixing ratios. Instead, other processes such as sedimentation of ice particles and radiative heating and cooling are introduced in the breech. For example sedimentation of ice particles from higher levels is invoked as one possible explanation for the presence of OMR values exceeding the homogeneous threshold at the 360 and 365 K levels. Another explanation offered for the same observation is that dehydration was ongoing at those levels.

Another question that is not adequately addressed is how adequate are the data and the analysis approach for making such discriminations? I think this question is most important for the trajectory-based upstream histories of air parcel saturation mixing ratio. As the authors show in Fig. 4, both the ERA40 and ECMWF operational analyses are cold-biased relative to the SOWER soundings by up to 2 K in the middle of the TTL (350-360K). The impact of these biases on the SMRmin estimates is not discussed in the text, and if the SMRmin estimates are in fact too dry below 365 K, then the agreement with the observations might well have been better. The other important aspect is confidence in the wind fields underpinning the trajectories. Fig. 4 shows substantial biases in the zonal wind especially. The bottom line is that a more careful accounting of the errors in SMRmin needs to be provided before the strong conclusions

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can be drawn from Fig. 14. While the caption for Fig. 14 does includes a statement about the SMR error bars, this is no substitute for a careful consideration of all the sources of error in tropical trajectory calculations.

Another potential source of error in SMR values was the choice to calculate trajectories adiabatically. For example, while the magnitude of the clear-sky radiative heating in the TTL is less than 1 K/day, air parcels in the western Pacific TTL are likely to spend a considerable fraction of time over extensive cloud decks. This and other potential sources of error in the trajectory calculations should have been addressed.

More specific comments:

1. I don't understand why a long passage on the statistical nature of dehydration is relegated to an appendix. The points raised here are very germane and deserve to be in the main body of the text. Appendices should be used in my opinion to discuss technical issues that may be critical to the methodology, but not to the scientific argument itself. The authors should consider incorporating the material in Appendix B into Section 4, although this may require some trimming.

2. Many of the figures are too small and difficult to read. This is particularly true of Figs. 5 and 14 as a whole and for the legends in Figs. 7, 10 and 11. Fig. 14 is the most important figure in the paper, and it is particularly hard to distinguish the symbols of the individual stations amidst the forest of error bars. If nothing else, the panels in this figure should be doubled in size.

3. The text will require copy-editing to correct grammatical errors and various malapropisms. A common example of the former is the insertion of the definite article where none is required (e.g., page 25837, line 1: '...efficiency of the "cold-trap" dehydration...'). An example of the latter is the word 'conveniently' in line 12 of page 25836.

Explanation of ratings:

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1. Scientific Significance: I think the paper has the potential for an Excellent rating if the arguments could be stated more clearly as I have discussed.

2. Scientific Quality: Again, there is the potential for this being an Excellent paper if trajectory errors were more thoroughly taken into account.

3. Presentation Quality: Two problems were mentioned: (a) the figures are of high quality but some are too small, and (b) use of the English language.

Recommendation:

This is an important contribution to a topic of very high interest and should be published. To address my overall comments, I would recommend the authors (1) revise the text to address the cold-trap hypothesis more squarely in the context of other processes potentially affecting the relationship between OMR and SMRmin and (2) provide a more thorough exposition of the errors in the estimates of SMRmin due to errors in analysis temperatures and wind fields as well as the neglect of diabatic heating or cooling along the trajectories.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 25833, 2012.