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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ultrathin Tropical Tropopause Clouds (UTTCs): II. Stabilization mechanisms" *by* B. P. Luo et al.

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In the first paragraph of section 3, it is suggested that since the clouds were always observed within a few hundred meters of the tropopause on each of five flights, they must be maintained at that altitude. However, the aircraft measurements do not provide any information about the lifetime or evolution of the clouds. The clouds observed on each of the five flights were probably completely independent, unrelated cloud layers. If the lifetimes of individual cloud layers are less than a day or so, then it is quite plausible that they are moving down through their lifetimes. We agree that the clouds observed during the five flights were probably unrelated. However, if they were not stabilized they would sediment by about 400 m per day, and the frost point would have to move down together with them, which is unlikely. We have added the aspect related to the frost point.

The only acknowledgment that wind shear is a consideration is in the Introduction:

"(unless, may be, subject to very high vertical wind shear)". No real argument is given for why wind shear could not produce very thin cloud layers. It would be more satisfying if wind shear measurements were shown or discussed for the flights where UTTCs were observed. In the second but last paragraph of Section 3 we added the sentence ą§Though extreme wind shear could help forming thin layers initially, the survival of the particles requires Sice = 1 while they sediment by 400 m/day, which is unlikely.ąÍ Also, unfortunately no wind shear measurements were made during APE-THESEO.

I do not understand the relevance of the AgI ice nucleation experiments (Pruppacher and Klett reference). It seems to me that the real question is whether there are as many as 5-10/L ice nuclei active at Sice=1.1. Perhaps a more relevant argument would be to refer to recent low-temperature laboratory measurements suggesting that insoluble particles (such as dust or soot) only lower the threshold for ice nucleation to about Sice=1.3. We gratefully accept the suggestion to refer to the recent low-temperature measurements.

The authors should mention the uncertainty in the APE-THESEO ice saturation ratio measurements. I expect the uncertainty is at least 15% due to the combination of temperature and water vapor concentration uncertainties. This issue is important since the low supersaturations are used both as an argument against heterogeneous nucleation and for the stabilization mechanism. Basically we state the uncertainties in the caption of Figure 1. The 1 K temperature uncertainty corresponds to Ďb 15 % uncertainty in Sice.

The paper leaves open a few questions about the simulated UTTCs: (1) Would the process have worked if you had used a polydispersed sized distribution for your initial cloud layer? (2) What controls the thickness of the UTTC? (3) Would the stabilization mechanism work with higher supersaturation above the cloud? (4) How large does the vertical wind speed gradient need to be to maintain the layer with moderate temperature fluctuations?

(1)We added the statement that poly-disperse distributions do not change the results. (2) That is a good points. The modelled cloud is thinner than the measured ones. The gradient of Sice profile has only little effect on the thickness if the UTTCs. The measured UTTCs with larger vertical thickness may due to small scale turbulence which is not considered in the column modelling. This sentence is added in the paragraph following Equation (4). (3) and (4) We discussed the stabilization more in depth at the end of section 5.

The ECMWF fields should be discussed in a section before "Conclusions". Also, why is the 27 February case shown instead of the 24 February case discussed above and shown in Figure 1. This shift leads to the suspicion that the correspondence between ECMWF vertical motions and the cloud location were not nearly as convincing for the earlier day.

We added a figure similar to Figure 5 for 24 February 1999 with ECMWF and lidar data. The correlation between UTTSc and upwelling motion was good. The discussion section focused on the strong upwelling motion over an large area. We think that this order gives a comfortable reading.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 1579, 2003.