Atmos. Chem. Phys. Discuss., 3, S526–S528, 2003
www.atmos-chem-phys.org/acpd/3/S526/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

Received and published: 11 May 2003

This a very interesting paper presenting a new mechanism for stabilization of very thin cirrus layers near the tropical tropopause. The paper is clearly written, and the stabilization mechanism is demonstrated convincingly. I have a few comments on the arguments against alternative mechanisms for formation of thin cirrus layers and the general discussion in the paper.

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.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

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.

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.

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.

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?

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.

ACPD

3, S526–S528, 2003

Interactive Comment

Minor comment:

It would be easier on the viewer if the lines in Figure 1 were identified. As it is, you need to read the caption to sort out which curve is which.

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

ACPD

3, S526–S528, 2003

Interactive Comment