

Interactive comment on “The annual cycle in lower stratospheric temperatures revisited” by S. Fueglistaler et al.

Anonymous Referee #3

Received and published: 2 January 2011

General Remarks

This is a very nice paper, straightforwardly and succinctly presented. It makes the important point that the tropical-extratropical temperature “see-saw” noted by Yulaeva et al. (1994) in MSU4 temperatures is not as directly controlled by the residual circulation as was suggested by those authors, and that in general seasonal variations in tropical ozone as well as latitudinal variations in static stability will introduce non-compensation in the seasonal cycle of lower stratospheric tropical and extratropical temperature. The paper convincingly demonstrates that the striking result found by Yulaeva et al. was to some extent an accident (“artefact” seems perhaps too strong, since there is still significant compensation) of the MSU4 weighting function, which averages over layers with different characteristics in the tropics.

At one level, this paper is a refinement of Yulaeva et al. Perhaps more significantly, though, it means that one cannot blindly use tropical-extratropical compensation as a diagnostic of circulation changes, which is important given the current interest in inferring changes in tropical lower stratospheric upwelling from the observed temperature record. I can see no methodological flaws in the approach (with the possible exception of point 4 below) or weaknesses in presentation and therefore recommend publication after the following comments are addressed.

Specific Remarks

1. p. 26825, lines 7-8: You say here that the radiative effects of water vapor “are certainly smaller” than those of ozone. Can you provide a reference for this? It seems like a surprising statement, given all the attention paid to Susan Solomon’s recent Science paper, especially when one considers the strong seasonal cycle in tropical lower stratospheric water vapor.
2. p. 26825, lines 9-11: The validity of the Newtonian cooling approximation was recently evaluated by Hitchcock et al. (JAS 2010) using a comprehensive GCM with interactive ozone chemistry. They found that the approximation broke down badly in the tropical lower stratosphere (see their Fig. 1d). This could provide another mechanism for non-compensation, as well as adding uncertainty to your diagnostic approach.
3. Inspired by YHW94, Pawson et al. (BAMS 2000) used the annual cycle in tropical and extratropical lower stratospheric as a diagnostic of model behavior. They chose 100 hPa rather than attempting to simulate the MSU4 weighting function. They nevertheless found nearly exact compensation of the annual cycles in ERA-15 temperature, and in most (but not all) models. Was this also an accident of the choice of 100 hPa? Your Fig. 7e suggests that 100 hPa is a very sensitive level to choose. It might be interesting to show the results at 100 hPa just for comparison with that paper, since it is so widely cited. However I note that the fact that some models in Pawson et al. did not show compensation suggests that compensation cannot be fundamental, consistent

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with the overall conclusion here.

4. p. 26825, lines 17-21: One concern with using reanalyses is that they do not necessarily respect the thermodynamic balance that you are using as a diagnostic. In particular, reanalysis temperatures are not constrained by the meridional circulation to the same extent that they are in the real atmosphere (and, in particular, are not constrained in the same way by wave drag). Have you examined the extent to which lower stratospheric global mean w bar star vanishes in ERA-Interim? If it doesn't vanish, then this could introduce a source of non-compensation, could it not? Can you exclude the possibility that this could explain your results? (I would feel more comfortable if you got the same results using a chemistry-climate model.)

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 26813, 2010.

ACPD

10, C11839–C11841,
2011

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