Atmos. Chem. Phys. Discuss., 9, S1017–S1018, 2009
www.atmos-chem-phys-discuss.net/9/S1017/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of CLaMS, KASIMA and ECHAM5/MESSy1 simulations in the lower stratosphere using observations of Odin/SMR and ILAS/ILAS-II" *by* F. Khosrawi et al.

Anonymous Referee #1

Received and published: 24 March 2009

With this comment, I hope to provide the editor with some guidance how to solve this discussion thread.

I do agree with Dr. Mueller that the discussion about using tracer-tracer correlations to derive ozone loss does not belong into the discussion of the Khosrawi et al. paper. I recommend to the editor not to ask Khosrawi et al. to include this controversial discussion into their paper, since it would weaken rather than strengthen the paper. I missed pointing out that Dr. Tilmes' concern about my first major comment in my review did already wrongly make this connection leading the discussion away from the very point of my review.

Dr. Tilmes took my statement 'these studies show that it is difficult to untangle the effects of chemistry and dynamics on the tracer-tracer correlation shape' out of context and put it into the context of a well-defined vortex and polar ozone loss, which is not considered by Khosrawi et al. and to which I was not referring to either. My comment was rather referring to the most recent model-measurement comparison by Hegglin and Shepherd (2007) (another reference Khosrawi et al. seem not to know, although a simple search of the web of science using model-measurement comparison and O3-N2O correlation would lead directly to this work). Hegglin and Shepherd (2007) point out that 'In the middle stratosphere, where the O3-N2O correlations are not compact, the fan-shaped structure of the overall correlation pattern in both hemispheres reflects the gross effects of the Brewer-Dobson circulation together with the chemical sources and sinks of the two species.' They also state that due to sampling the effects of dynamics and chemistry in the upper stratosphere are hard to be distinguished. They infer this result from a more comprehensive satellite data set than hitherto available, and only then compare to a model. Khosrawi et al. need to take this finding into account when interpreting their results.

For the second point in the comment by Dr. Tilmes which referred to my major comment 2, I was maybe too harsh in asking to remove the Tilmes et al. and Lemmen et al. references. But I still recommend Khosrawi et al. to add a reference to Eyring et al. (2006) and to point out that different diagnostics validating ozone loss in the polar region lead to very different conclusions of the performance of the different models.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 1977, 2009.

ACPD

9, S1017–S1018, 2009

Interactive Comment