

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: 21 March 2009

I here reply (as reviewer #1) to the comment of Dr. Tilmes which questions my concern that chemical ozone loss and diabatic descent cannot be distinguished in the tracer-tracer correlation method. This is a lively discussion point throughout the history of the use of the tracer-tracer correlation method in determining chemical ozone loss, and in my eyes has never been resolved satisfactorily, and will not be solved in this discussion either.

It is crucial in scientific research that new findings are discussed in the light of knowledge derived from earlier publications, and reviewers have the duty to assess the quality of this discussion. In my assessment as reviewer I arrived at the conclusion that

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Koshrawi et al. chose a very one-sided selection of the literature, all supporting the application of the tracer-tracer correlation method, while there is a whole list of literature (as one can see from the references given in my review and below) which is ignored by the authors. Koshrawi et al. need to mention possible shortcomings of the method as pointed out in the literature, and not just attribute differences seen in their model-measurement comparison to model deficiencies.

I therefore give some more examples from the literature to demonstrate that the arguments of Dr. Tilmes are not carved in stone:

The seminal paper on tracer-tracer correlations by Plumb and Ko (JGR 97, 10145-10156, 1992), which is often cited in defence of the use of the correlation method to infer ozone depletion, in fact states that it should NEVER be applied to ozone because it is not sufficiently long-lived. While it is true that within an isolated polar vortex during polar night ozone becomes sufficiently long-lived to develop compact correlations with other species (an effect not addressed by Plumb and Ko), Sankey and Shepherd (JGR 108, 4494, 2003) showed that it takes a month or longer for this compactness to develop, and that the vortex needs to be well isolated. Koshrawi et al. apply the diagnostic globally, without regard to these issues.

Michelsen et al. (JGR 104, 26419-26436, 1999) concluded that mixing of air into the lower Antarctic vortex in the spring perturbed the O₃:N₂O correlation, enhancing and not underestimating the apparent photochemical ozone loss. That analysis was based on ATMOS, HALOE, and ER-2 correlations of multiple species. That reference, along with Michelsen et al. (JGR 103, 28347-28359, 1998), Plumb et al. (JGR 105, 10047-10062, 2000), and Ray et al. (JGR 107, 8285, 2002), provide more counter-examples to the assertion that ozone loss would only be underestimated by the O₃:N₂O correlation.

Mueller et al. (2005) claim that descent would not be strong enough to enable upper stratospheric/mesospheric air to mix with lower stratospheric air, but there are counter-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

examples to that assertion as well, e.g. Rosenfield and Schoeberl (JGR 106, 33485, 2001) and Plumb et al. (JGR 108, 8309, 2003). Also, Mueller et al. (2005) use a simplified model to simulate the impact of diabatic downwelling on the O₃:N₂O correlation which in my eyes is initialized with a wrong shape of the O₃:N₂O correlation. This shape misses the branch of the correlation lying in the upper stratosphere where the O₃:N₂O correlation is negative. Missing this part of the curve may indeed miss the mixing by diabatic descent. If this branch would be included it could potentially lead to an overestimation rather an underestimation of the ozone loss.

While Huck et al. (2007) is quoted as supporting the use of this kind of analysis, in my reading of this paper there is no independent evidence provided, just a restatement of the assertions made in Mueller et al. (2005).

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

