

## ***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**

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### **General comments:**

This study aims at validating transport and chemistry of three different atmospheric models using N<sub>2</sub>O and O<sub>3</sub> data obtained from ILAS, ILAS II, and Odin/Osiris. Diagnostics applied include a method by Proffitt et al. (2003) using monthly averaged N<sub>2</sub>O and O<sub>3</sub> values in tracer-tracer coordinates, and the annual evolution of mean O<sub>3</sub> values derived over different latitude regions.

There is definitely great interest in the chemistry-climate model (CCM) community to test CCMs with different diagnostics in order to gain confidence in their predictions of climate change and effects on the ozone layer. The O<sub>3</sub>-N<sub>2</sub>O correlation in particu-

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lar has long been a tool to validate chemistry-transport and chemistry-climate models (Bregman et al., 2000; Proffitt et al., 2003; Sankey and Shepherd, 2003).

However, Koshrawi et al. seem to be unaware of some recent findings published in the literature on the use of tracer-tracer correlations, which ask for caution when applying this diagnostic. These studies show that it is difficult to untangle the effects of chemistry and dynamics on the tracer-tracer correlation shape. As a consequence, the conclusions Koshrawi et al. drew from their results are likely to be too simplified and in some cases less insightful than what the authors present them to be. This criticism is explained in more detail below, and I can't recommend publication of this study in ACP without major revisions, which must include a discussion of the limitations of the diagnostic and revised conclusions.

### Specific comments:

**(1) P1978 L22:** This conclusion is drawn based on the assumption that at 500K, the tracer-tracer correlations is influenced by chemistry only. However, this is not a valid assumption. Diabatic descent has been shown by many studies to influence tracer distributions down to 500K (Rosenfield et al. 1994; Manney et al. 1995a, 1995b; Schoeberl et al. 1995). This affects also the O<sub>3</sub>-N<sub>2</sub>O correlation, and the diagnostic is therefore not able to tell us if the differences between model and measurements are due to either chemistry or dynamics, but are usually an effect of both. Also, both the influence of diabatic descent and ozone loss can vary substantially from year to year. Comparing data sets obtained during different years could therefore lead to differences between model and measurements which are explained by natural variability, not model deficiencies.

**(2) P1979 L15-19 / P2000 L27-P2001 L1:** More care should be taken when using diagnostics of chemical ozone loss in the polar regions for model validation with observations. The method used by Tilmes et al. and Lemmen et al. to derive ozone loss based on tracer-tracer correlations is likely to lead to an overestimation of chemical

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ozone loss (Huck et al., 2007). A better reference would be Eyring et al. (2007) in which the authors applied different metrics to validate the model ozone loss in different CCMs.

**(3) P1988 L5-10 / P1979 L23 / P1991 L12-17:** A recent model-measurement comparison has been carried out by Hegglin and Shepherd (2007) and showed that the N<sub>2</sub>O-O<sub>3</sub> correlation is not compact in the upper stratosphere. One conclusion from their study was that sampling can strongly affect the derived slopes of the tracer-tracer correlations. It might therefore not be appropriate to extend the method of Proffitt et al. to the upper stratosphere, nor to take single profile measurements as reference curves for mid-latitude, polar, or tropical air masses as done by Koshrawi et al (2008). Please discuss this issue.

**(4) P1986 L20-25:** See specific comment (1) and references therein. The two processes likely cannot be separated by moving up by 150K in potential temperature. For example, the authors claim that the inflection point in Fig. 5 (polar regions) around 190 ppbv N<sub>2</sub>O is a sign of ozone destruction. This inflection point is also seen in Fig. 4 and even to some extent in Fig. 3, but these figures show mid-latitude and tropical air masses, so we do not expect ozone loss in these regions. A more plausible explanation for the observed change in correlation slopes is given in Hegglin and Shepherd (2007). Their evaluations show that tracer-tracer correlations (in the lower stratosphere just below 500K) exhibit different seasonal cycles when looking at midlatitudes and high latitudes. While absolute slope values increase during NH winter at midlatitudes (due to increased transport of ozone rich air from the tropics), slope values at high latitudes decrease (due to the transport barrier of the polar vortex inhibiting the transport of younger ozone rich air to high latitudes and an increasing strength in diabatic descent which brings older air masses to lower altitudes).

**(5) P1992 L22-24:** Strong mixing leads to a flattening, not a steepening of tracer correlations (Plumb and Ko 1992). Rather, I suspect that something is wrong with the ODIN data (see also specific comment 9).

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**(5) P1993 L1-5:** Rather than not discussing you might want to not show these data points.

**(6) P1994 L19:** It is not clear to me which data you are using: ILAS or ILAS-II or a combination of both? If you use ILAS, then the difference could just arise from the fact that ODIN/SMR and ILAS did measure in different years.

**(7) P1995 L 14:** See my specific comment (2).

**(8) Figures 8 and 9:** These figures and/or captions are not clear to me. Do you calculate the differences between the model ozone or the ILAS measurements and Odin/SMR? Please improve the wording. Do you mingle the ILAS data obtained during ILAS and ILAS-II although they seem not to measure during the same months nor years (P1985 L11)? I don't think you could do this. The differences you see might have nothing to do with the model performance or accuracy of the measurements, but only reflect interannual/interseasonal variability.

**(9) Figures 2, 3, and 4:** There seems to be a problem with either the binning applied to the data or the ODIN/SMR data in the tropical region themselves, where O<sub>3</sub> values up to 6 ppmv at N<sub>2</sub>O values of 330 ppbv are found. First, N<sub>2</sub>O values are expected to decrease with increasing height in the tropics (e.g. Hegglin and Shepherd, 2007). Second, the tropospheric N<sub>2</sub>O levels during the observation period of ODIN/SMR are expected to lie below 320 ppbv (IPCC, 2007).

#### Technical comments:

**(1) P1981 L26; P1982 L4; P1983 L12:** Please be more concise about which time periods you use for the different models as you did for CLaMS. It would not make sense to compare 2003 November data with 2002 November data since the diabatic descent rates have considerable interannual variability.

**(2) P1985 L25-P1986 L10:** Please provide numbers for these comparisons.

**(3) P1992 L21-22:** Please improve language.

- (4) **P1993 L1**: change 'led' to 'lead'
- (5) **P1994 L19**: change 'this' to 'these'
- (6) **P1996 L11**: delete 'a' between 'thus' and 'less'
- (7) **P2001 L5**: change 'signicantly' to 'significantly'

### References:

(Only references which are not used in the Koshrawi et al. manuscript are listed)

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**ACPD**

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