

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.

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The main point of Alan Plumb's comment regarding the paper under discussion is that he disagrees with the statement in the sentence (p. 1986, lines 11-12): "This method helps to separate O₃ variability due to latitudinal transport from photochemical changes." This criticism has been raised already more than 15 years ago (Plumb and Ko, 1992) and was discussed by e.g. Proffitt et al. (1992). A detailed discussion concerning Alan Plumb's criticism raised here on how mixing affects the TT correlations can be found in Proffitt et al. (2003). Besides the Proffitt et al. (1992) and Proffitt et al. (2003) papers there are several other published papers that have established and successfully defended that TT relationships are a useful tool for separating the effects of chemistry from the effects of atmospheric dynamics.

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Most importantly, however, we cannot see any contradiction between the statements in the comment and our paper. We emphasize that we do not attempt here to quantify chemical ozone loss in terms of mixing ratio as it is done in the classical TT method. The point of the paper in discussion is to provide and use monthly averages of N₂O and O₃ binned by potential temperature from satellite observations as a tool for model evaluation. The method used here is a strongly modified form of the classical TT method that indeed *helps* separating transport from chemical changes. Thus, the effects of transport and chemistry become visible in the families of curves of monthly averaged N₂O and O₃.

Further, we would like to point out that the method of Proffitt et al. (2003) has so far only applied by Khosrawi et al. (2004,2006,2008) and up to now there is no known study or published paper providing evidence that this method does not help to separate photochemical from dynamical changes or should not be used as a tool for the evaluation of atmospheric photochemical models.

Other parts of Alan Plumb's comment are regarding the original TT method that, in contrast to the method employed here, does attempt to quantify chemical ozone loss. We agree with Alan Plumb that this discussion is somewhat peripheral to the paper under discussion and we agree with referee 1 that this issue cannot be resolved in this discussion and should also not be part of this paper.

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