

***Interactive comment on* “Technical note: A stratospheric climatology for O₃, H₂O and CH₄ derived from HALOE measurements” by J.-U. Grooß and J. M. Russell III**

Anonymous Referee #1

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General comments

This paper presents a climatology of ozone, methane and water vapor based on observations from the HALOE instrument on board the Upper Atmosphere Research Satellite (UARS) over more than a decade from 1991 to 2002. This is a very important work which serves the needs of the modeling community. The presented climatologies derived from a single stable data source are important for the use as input in general circulation models (GCMs) and chemical transport models (CTMs). In addition they are also very important for the chemistry evaluation of coupled chemistry-climate mod-

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els (CCMs) and CTMs. The easy access of the data in two different formats (ASCII and netcdf) along with a detailed description is highly commendable. The paper is clearly structured. However, a detailed description on how the derived climatologies agree with previous work is missing and is clearly needed. Therefore, the text needs some rewriting. I would recommend publication of the manuscript after addressing the comments below.

Specific comments

p.2974, Introduction: The relevance of long-term observations as input for radiative transfer calculations is stated in the introduction. However, long-term observations are also important for the chemistry evaluation of coupled chemistry-climate models (CCMs) and CTMs. This needs to be better stated in the introduction. So far, the introduction only focuses on models that are using the derived climatologies as input, thus highlighting the effect on realistic simulation of radiation. It is true that both approaches (off-line and online) need long-term observations, but on page 2974 this gets all mixed up. Please rewrite and clearly separate the two different approaches.

In addition to the climatology, it would be desirable if the authors could provide the monthly mean data of the entire period (even with gaps) as electronic supplement. If the observations are only used as input for global models this might not be necessary. However, as soon as the observations are used for the evaluation of CCMs, the monthly mean data set would add sufficiently more information. For a CCM it is not only important to reproduce the observed climatological state, but also the interannual variability. This is only a wish and the authors might have their reasons not to provide the data.

p. 2975, line 14-p.2976, line 4: Part of this paragraph is already a description of the method and should be moved to the section 'Method'.

p.2976, line 5ff: Rather than learning a lot about climatologies that are used in model simulations, I would prefer to get a review on how the derived climatologies agree with

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previous work. The Randel et al. papers are cited only to review the water vapor trend, but nothing is said how they compare to the results presented here. How do the presented climatologies compare to the one derived in the UARS Reference Atmosphere Project (SPARC reference climatologies) apart that here a longer time period is used? How do HALOE results compare with other instruments (e.g. SAGE (Taha et al., JGR, 2004); MIPAS, GOME etc.)? I would recommend including an extra section. Please include more references. Are you able to quantify the differences between the different instruments and analysis techniques?

p.2976, line 5-8: paragraph on models, line 8-13: paragraph on previous HALOE climatologies, line 14-21: paragraph on models. Please sort and move a review on available HALOE climatologies in a separate section or at least separate paragraph.

p.2976, line 11: How does the Randel et al. 1997 climatology compares to the results presented in this paper? E.g., if you would use the same period (1991 to March 1997) would your results be the same?

p.2979, line 18-24: This statement seems to be rather unjustified to me. We all know that ozone did not remain constant in the past and will likely not stay constant in the future. At least a reference on the evolution of ozone during the observed period 1991 to 2002 and references on what CCMs actually predict for the evolution of future stratospheric ozone (Austin et al., 2003 and WMO, 2003) are needed.

p.2979, line 24p.2980, line 4: Similar: Concerning the use of the derived water vapor and methane climatology until 2015 needs some more discussion on how methane and water vapor is predicted to unfold over the next ten years. The discussion on the water vapor trend is not clear. For example, the sentence 'However, it has been reported not to continue over . . .' is ignored as in the next sentence 'This behavior . . .' refers to an increase of water vapor. Keeping the ozone, water vapor and methane distribution fixed for the input of model simulations over a time period of 25 years does not sound like a good advice to me. As long as you cannot justify this statement, I would recommend

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deleting it.

p.2980, 16-17: Clearly, in addition to the use of the presented climatologies as input for GCMs and CTMs, they are also suitable for the chemistry evaluation of CCMs and CTMs. This should be said in the conclusions, as it extends the community that might use the presented climatologies. Again, I would recommend deleting the statement that the presented climatologies are well suitable as input for model simulations until 2015.

I would recommend extending the header in the electronic supplement (e.g. include reference, time period, instrument, latitudes etc.).

Technical corrections

p.2967, line 10: replace 'th' by 'the'

p.2978, line 18: change 'if the compiled' to 'of the compiled'

p.2985 and p.2988, Figure caption 3 and 6: change 'March, June, September and December' to 'January, April, July, and October'.

p.2975, line 9: replace 'waver' by 'water'

p. 2978, line 6, 'ratioing'

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