

***Interactive comment on “Long-term trends of the concentration of the minor constituents in the mesosphere – a model study” by M. Grygalashvyly et al.***

**Anonymous Referee #2**

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Review of "Long-term trends of the concentration of the minor constituents in the mesosphere; a model study", by M. Grygalashvyly, G. R. Sonnemann, and P. Hartogh.

This paper describes numerical simulations of increasing water vapor in the atmosphere due in part to increasing methane concentrations, and its effects on chemistry near the mesopause. Unfortunately the paper has significant issues that need to be addressed. In particular the paper lacks statistical analysis of the data presented, and so the reader is unclear if the long-term differences presented are significant.

The paper is quite difficult to follow, and needs to be radically restructured. Sections 1 through 6 are almost entirely introductory material, and much of the material can

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be replaced by a few references. For example, the discussion of odd-hydrogen driven catalytic loss of ozone could be replaced with a reference to Brasseur and Solomon's Aeronomy textbook. Also, what is the relevance of the discussion of the D-region and NO (pg. 15462) to the current study? The paper would be improved if a much-abbreviated version of these 6 sections formed a revised Introduction section. The description of the model and experimental setup up is minimal and distributed among these sections. It is not until the end of section 5 that the authors disclose that the model is non-interactive. There are other model caveats, and I would suggest adding a short section on the model and experimental setup.

"Trend" usually refers to the change per unit time derived from a regression analysis. While the title implies it is a study of trends, no trends are calculated. What is shown in Figures 4, 5, 8, and 11 is the difference of two years (1997 and 1880). The reader is left to wonder if the differences are due to dynamical variability or an underlying trend. At a minimum, several years should be averaged to ascertain the variance in water vapor, and a difference of means presented. Picking two solar minimums is not sufficient. The difference of means should be accompanied by a plot showing the statistical significance of the difference (usually tested using the Student t-test). Similarly, Figures 13 to 15 show differences of individual years and no indication as to if they are significant.

In Figure 4, there is some discussion of the decrease during October (suggested to be related to Lyman-alpha changes), but no discussion of the very large increase in March. It is stated that the absolute values of water vapor are very small, which indicates the changes are not statistically significant. There appears to be an inconsistency between Figures 4 and 5. Figure 4 shows >95% changes in March at 67.5N, but Figure 5 indicates at most a 70% change. Also, the caption appears to be wrong for Figure 4, since it states it is a difference between 1997 and 1880, whereas the text states 1890.

Figure 9 presents two time-series of ozone diurnal mean (zonal mean?) ozone separated by 5 degrees in latitude. This could be just one model grid-spacing, although the

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horizontal grid-spacing is not specified. The differences in the two series are extreme, and if correct, call into question any of the water vapor / ozone relationships discussed in the paper. For example, the solar cycle response is out of phase for the two time series, and a decadal mean of the traces would have opposite slopes. How can HOx increases during solar maximum decrease ozone at 2.5S and increase it at 7.5S? This bizarre model behavior is not explained at all in the text, except for a reference to a "bistable response". Similarly the discussion of Figure 10 is not adequate - "Our results may hint of a bistable behavior but, on the other hand, the integration of the system is too coarse to get a definitive answer."

The abstracts indicates the effects of rising N<sub>2</sub>O will be presented. However, trends for N<sub>2</sub>O derived from the model are missing from the analysis. There is no indication whatsoever in this paper that they have an important effect on mesospheric chemistry. The abstract and paper should remove references to N<sub>2</sub>O.

The results presented here are necessarily unrealistic to some extent since the model used is non-interactive. The magnitude of the discrepancy needs to be discussed. For example, how do changes stratospheric and mesospheric temperature resulting from carbon dioxide and ozone (presumably not included in the model) affect the simulations? How does the change in the Brewer Dobson circulation affect the rate at which methane is converted to water vapor (see e.g. Austin and Li, GRL, 2006)?

minor comments:

There are numerous grammatical and punctuation errors in the manuscript. I have listed some below.

15454/1: dinitrogen oxide is usually referred to as nitrous oxide

15455/3: typo "El Nio"

15455/8: "we observed ... since the beginning of industrialization." This implies the authors actually observed the increase since the late 1800s. Suggest re-wording and

giving a reference.

15455/13: "assess the shares..." - it is not clear what is meant here. Are the authors describing attribution?

15455/21: The text is misleading, in that it implies the model used here is fully coupled.

15456/12: growing -> growth

15456/14: "and the now" - delete "the"

15456/17: "rise" - is this supposed to be rice?

15459/19: punctuation needed here

15460/20: end of sentence missing

15463/10: it is better to not make judgments on what is the most important increase, since this is very subjective.

15463/20: "no NLC have been observed" - reference?

15464/4: It appears as though the 6 ppm water vapour contour in summer is about 10 km higher in the model than in the observations. Although it is difficult to verify this since the data are not plotted with the same contour intervals. Therefore, I'm not convinced the water vapor mixing ratios are "quantitatively reproduced".

15464/17: "In the region..." Punctuation needed and suggest replacing "in high summery latitudes" with "at high latitudes during summertime"

15465/13: "germs" - nuclei?

The captions for Figures 6, 7, 9 and 12 should be modified, since trends are not shown, but time series.

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 15453, 2007.

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