

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

**Anonymous Referee #1**

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The authors of this paper have performed a long model run using the COMMA-IAP model, but generally seem to lose sight of the purpose of this model run. In the middle of a paragraph on pg. 15455 they say: "Our main goal is the reproduction of the water vapor trend in the middle atmosphere on the basis of the derived and measured trends of methane (CH<sub>4</sub>), dinitrogen oxide (N<sub>2</sub>O), and carbon dioxide (CO<sub>2</sub>) inclusive of the trend of the solar Lyman- radiation." This seems to be a reasonable goal for this paper, but if this is the main goal this statement should not be buried in the middle of a paragraph. A paper of about half the current length would probably suffice and be much more readable.

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The goal of reproducing the water vapor trend seems to be motivated in part by the desire to reproduce an observed long-term trend in NLCs, so it makes sense to look at factors that influence temperature in the polar summer mesosphere. But many of the plots in the paper (such as Figures 8-12, and 15) are only vaguely related to this purpose. On the other hand, I would have thought that a plot showing the change in relative humidity would be invaluable. The ozone variations may be important to the heating rates, but since there are no measurements of similar timescale to compare with I don't understand what the reader is supposed to make of these plots. Near the end of the paper the authors do make a passing reference to: "This model assertion was confirmed by microwave measurements of ozone in middle latitude at Lindau, Germany when comparing the observations with data of the ozone reference model established from satellite measurements 20 years ago." It is asking a lot of the reader to take a set of unnamed microwave measurements taken against an unnamed reference model as confirmation of the model results.

The discussion of water vapor beginning on pg. 15459 makes a not very complicated topic sound amazingly complex and needs to be rewritten more clearly. Statements such as "It is a common erroneous belief that water vapor enters, on global average, into the stratosphere from the troposphere" are inappropriate. Although there are studies showing increases in water vapor in the recent past, there is no physically reasonable argument given here (nor do I think there is one) as to why water vapor entering the stratosphere should have increased since pre-industrial times. While there is nothing particularly wrong about assuming a 10% dryer hygropause in 1890, I see no reason to characterize this estimate as "conservative". The increase in humidity in the troposphere is irrelevant if the dominant mechanism controlling water vapor entering the stratosphere is the temperature at the tropical tropopause, and I know of no evidence that this has either warmed or cooled.

The figure captions for Figures 6 and 7 are confusingly labeled as "trends". While the figures do show the presence of trends, what is actually plotted is mixing ratio (not,

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e.g., change in mixing ratio per year).

I don't believe the statement that the diurnal variation of the water vapor mixing ratio is not significant below 80 km is correct. The diurnal variation of water vapor is almost certainly dominated by diurnal tides and these should produce clear diurnal signals below 80 km.

If I understand correctly, the results of Figure 8 are dependent on the integration time step. If this is not the case please clarify. If it is, then, while the bistable behavior may be of mathematical interest, the publication of the ozone results in Figures 8 and 9 seems inappropriate.

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

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