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Atmospheric Chemistry and Physics Discussions

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

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Response to the comments on the paper by Referee #1

Long-term trends of the concentration of the minor constituents in the mesosphere - a model study

By M. Grygalashvyly, G. R. Sonnemann, P. Hartogh

Dear Referee,

Thank you for your comments on the paper and constructive recommendations. We have tried to follow your suggestions and have utilized most of them. Additionally, we should mention that the paper has now been edited by a native English speaker. We

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Discussion Paper

have changed and corrected the different errors and weaknesses that were mentioned by you and the second referee.

We have changed the structure of paper, particularly section 1, according to your recommendation. We deleted the chapter regarding chemical aspects and inserted only the introduction for the catalytic ozone destructing cycles into the "Discussion" chapter. We believe that this is necessary because the reason for the ozone decrease differs in the upper mesosphere/mesopause region versus the lower mesosphere/stratopause region. We deleted all discussions related to the NOx increase due to the rise of nitrous oxide. We also deleted Figures 9 and 10. Although a great number of papers deal with the complicated nonlinear response of the chemistry within the mesopause region, the effects are not commonly known in the scientific community. Thus, we had to explain the scientific background, which enlarged the paper and departed from the main subject of the manuscript. On the other hand, we must mention that such effects can occur. Both the occurrence of subharmonics and chaos, which is suppressed by large time steps, and the bi-stable behavior (trigger solution), which also occurs for longer integration time steps, are nonlinear effects. In our long-term calculation such a behavior appears. We state this effect, but on its own it is an entire subject of scientific research to investigate its behavior related to its dependence on time step and different control parameters, such as the water vapor mixing ratio or the ratio of daytime to nighttime hours, etc. This is not possible in the frame of our long-term calculations. The discussion of water vapor on page 15459 in the chapter entitled "Chemical aspects" was changed as mentioned above, and some clumsy passages were deleted. This also concerns the title of the paper. The captions for Figures 6 and 7 were changed to a more appropriate manner, and the word "trend" has been replaced by the term "long-term behavior". You are right, a certain diurnal variation truly occurs below 80 km, which is dominated by diurnal tides; however, this variation is relatively small in high summery latitudes, which is what we considered here (maximum of 10 % at 80 km and less than 10 % at 70 km in high latitudes, but there are regions in middle and low latitudes close to the sub-solar point marked by essentially larger amplitudes due

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to stronger tidal winds and larger gradients of the water vapor mixing ratio). We have now specified the following assertion. The reason is that the tides shift the air parcel upward or downward only by 1or 2 km. The vertical water vapor gradient is not so large that this could entail a distinct diurnal variation. The diurnal variation due to the meridional transport that is triggered by the tidal waves lies in the same order of magnitude. According to the long life time of water vapor below 80 km, the diurnal variation due to the photochemistry also ranges in the order of few percent depending on height.

We need lower boundary values for the model among others also for water vapor. We cannot model this, and we suppose that no current model can reliably model the complicated behavior at the hygropause in pre-industrial time. The assumption of a 10 % dryer lower boundary at the hygropause is, of course, a rather arbitrary value. The only argument supporting this results from the fact that the stratosphere was dryer and that the globally averaged water vapor flux is directed downward. This statement results simply from the fact that the methane flux entering the stratosphere is some orders larger than the hydrogen escape flux. Methane will be oxidized in the middle atmosphere to water vapor. The difference between H-atoms bound on methane entering the stratosphere and the part that leaves the Earth's atmosphere penetrates as water vapor into the troposphere again. This water vapor flux depends, of course, on latitude, season, etc. In the tropics where upward moving air enters into the stratosphere, the values of the water vapor mixing ratio may have been unchanged, but, outside this area where stratospheric air enters the troposphere, the relations should be different. We have now changed some misleading formulations.

With respect,

M. Grygalashvyly, G. R. Sonnemann and P. Hartogh.

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

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