Atmos. Chem. Phys. Discuss., 7, S8588–S8591, 2008
www.atmos-chem-phys-discuss.net/7/S8588/2008/

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.

M. Grygalashvyly et al.

Received and published: 21 January 2008

Response to the comments by Referee #2

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 2,

Thank you very much for your comments on the paper. We have tried to follow your suggestions and have taken your remarks into consideration.

We have applied some of your recommendations and corrections concerning the lan-

guage and style. The paper has been edited by a native English speaker. We thank you for the corrections of our grammatical errors and misprints. We have changed and corrected the various errors and weaknesses that you and the other referee mentioned.

Your main point of criticism concerns the statistical analysis. It is certainly a weakness in our presentation, and a lack of lucidity will prevail if the method of investigation does not become clear. Therefore, we have changed this part. We use a deterministic model that calculates climatological means. The dynamic part of the model calculates annual variations of the temperature, pressure, and wind components, which does not change during the long-term calculation but the annual variation repeats every year because the model does not operate interactively. Establishing a trustworthy interactive global model for the MLT-region is a very intricate task because the thermal structure of the mesopause region depends specifically on the chemistry but also on the complete wave activity (i.e., gravity and planetary waves), which cannot be modeled in the framework of this model. Currently, all models are tuned by gravity waves in the way that the seasonal behavior particular of the mesopause region meets the observations. This is state of the art. Presently, no model can truly calculate the thermal structure of this domain for the pre-industrial time, and we did not want to burden the calculations with speculative dynamic fields. Thus, we also cannot give an answer regarding the change of the Brewer-Dobson circulation, and we would very much doubt if presenting such results. We have used estimated mixing ratios published by other groups for a few anthropogenic increasing species (i.e., methane, nitrous oxide, and carbon dioxide) for the pre-industrial time and current measurements. Using these values, we carry out calculation under the condition of a nearly exponential increase in mean growth rates. The average exponential increase of these species is employed then in the chemical model to calculate the concentrations of other minor constituents during the 120 years that were considered. The calculated values are not subjected to any statistical scatter; they are no measurements. Hence, a statistical analysis makes no sense. The annual variation of the water vapor mixing ratio measured in ALOMAR, which is shown by Fig. 3, is only used to demonstrate the fact that the calculated water vapor mixing ratios

ACPD

7, S8588–S8591, 2008

show a similar behavior as the measurements. The model calculates climatological means, whereas the measurements relate to an individual year.

According to your recommendation, we included a short discussion of the model. We neglected to introduce the model in the first version and referred only to some publications in order to reduce the expanse of the paper, but providing an introduction in the paper itself may make the full text more intelligible for the reader. We deleted the section regarding chemical aspects. The other sections about reproduction of the Lyman-alpha radiation and the discussion of the lower boundary (hygropause) of the model relates to the necessary requirements to solve the considered problem, but they do not belong in the introduction. Only a few chemical aspects are now discussed in the Discussion section at the appropriate place. We added some new references, but we think that the introduction of the catalytic cycles is necessary in order to assist the reader's understanding of the fact that the mechanism of the ozone decrease differs in the upper versus the lower mesosphere. We also deleted the passages related to discussion of the effects due to the increase N2O and, consequently, the change in NOx. The same is valid for the ozone measurements in Lindau because these results are not published thus far; hence, we cannot provide an appropriate reference.

Based on your criticism we have changed the word "trend" to the expression "long-term behavior". This also affects the title of the paper.

We deleted Figs. 9 and 10 because the explanation of this complicated state of affairs requires much space. The strange nonlinear response of the photochemical system close to the mesopause region was investigated in a large body of publications. In order to obtain nonlinear effects, the numerical integration of the model should not be too diffusive, meaning one needs a transport scheme marked by small numerical diffusion, such as the Walcek-scheme that we used. The height resolution should be on the order of 1 km or smaller. To obtain trustworthy effects, such as a period doubling or chaos, the integration time has to be much smaller than what is used for long-term calculations. Therefore, we did not find these effects in this model run. However,

ACPD

7, S8588–S8591, 2008

we can get apparently a trigger solution even for larger time steps, as suggested by Yang and Brasseur (1994). The solution depends on the H-flux determined by the vertical wind changing from grid point to grid point. Obviously, the system responds sensitively to small changes in the control parameters. This behavior is well-known in nonlinear dynamics. For a concrete case, we cannot say whether the system operates in a bi-stable or in a single mode. We only found sudden steps of the mixing ratios when changing system conditions slightly. The deleted figures also displayed such a response. As we took all presented figures from the same long-term run, further figures also show the behavior for the other minor constituents such as OH. This is why we have included a discussion about this nonlinear behavior. Water vapor is only faintly attacked by this effect as the photolysis does not depend on it.

The results presented in the paper are taken from only one model run, and all figures are consequently consistent. Figure 5b shows the data for the 1st of April. This date was mentioned for the other figures, but we forgot to include it for this one. Comparing the vertical section in Fig. 4 on 1st April with the vertical section in Fig. 5b at 67.5° N, one will find the same height distribution; this is clear if employing the same data set to produce the figures. Figure 4, as also Figure 5b, shows for 1st April at 67.5° N in maximum, which is placed between approximately 85 km and 90 km, the values of relative deviation of 55-60%. Figure 4 shows 95% (as it was mentioned in your comment) in the middle of February (not in March). The maximum value that Figure 5b shows at high latitudes amounts to 60-65% (not 70% and not at 67.5° N).

With respect,

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

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

ACPD

7, S8588–S8591, 2008

Interactive Comment