Response to the comments on the paper by Referee 2

Greenhouse gas effects on the solar cycle response of water vapour and noctilucent clouds

By Ashique Vellalassery[1], Gerd Baumgarten[1], Mykhaylo Grygalashvyly[1], and Franz-Josef Lübken[1]

Dear Referee,

Thank you very much for your comments on the paper and constructive recommendations. We have tried to follow your suggestions and take almost all of your remarks into consideration. In the following, we mention point by point how the manuscript has been changed according to your suggestions.

1. Lines 14-16: Potentially solar activity can also affect the circulation and transport of the H2O and CH4 from the troposphere.
   Yes, you are right. However, in our MIMAS model, we use atmospheric dynamics for all runs corresponding to 1976, so the effects of solar activity on the circulation and transport of H_2O and CH_4 from the troposphere are not included. Since the effect of the solar cycle on dynamics in the atmosphere is still under debate, we have not included changes in the dynamics to have a better separation of the influences of temperature and Lyman alpha (see the reply to the comment 20).

2. Lines 27-28: In absolute or relative values?
   The decrease in Lyman alpha variability in the late phase is given in absolute values. We have deleted the sentence that partially duplicates lines 22-24 (see the response to reviewer 1).

3. Lines 33-34: Little explanation of the mechanism would be nice.
   These lines are deleted (see the response to reviewer 1). The mechanism is described in more detail in section 3.3.

4. Line 50: Which trends? Any trends?

   We mean the trends in background temperatures and H_2O concentrations. We have added this to line 47 (revised manuscript).

   "NLCs have been proposed as indicators of trends in background temperature and H_2O concentrations"

5. Lines 72-78: Has it already been performed and described somewhere, or it is planned for the current manuscript? These lines are confusing because after that they mentioned the goals of the paper. Some rewriting would be nice to distinguish between the introduction and motivation.

   We have changed the sentence to make it clearer (Lines 70-72 of the revised manuscript). The model runs without microphysics were performed in the current study.

   "Therefore, for this study, simulations are performed with and without microphysics using the same background conditions, resulting in a H_2O profile with and without NLC".

6. Line 101: What is the lower atmosphere here?

   The lower atmosphere here means up to an altitude of 45 km. We have clarified this in line 99 (revised manuscript).

   "*The LIMA model in this study is nudged to reanalysis data NOAA-CIRES (National Oceanic and Atmospheric Administration-Cooperative Institute for Research in Environmental Sciences 20CR; Compo et al., 2011) up to an altitude of 45 km"*

7. Line 103: Does it mean that GWD forcing is fixed and does not depend on the solar cycle?

   Yes, the GWD is fixed and does not vary during the solar cycle (see the discussion in comment 1).

8. Figure 1: CH4 looks strange. It has no source, no effects.

   Figure 1 has been updated. Now we show that in our runs the water vapour is parameterized as a function of CH_4.

9. Lines 115-116: But there is H_2O photolysis. Why only CH_4?

   Photolysis is taken into account in MIMAS and causes changes in the H_2O concentration. The increase in CH_4 affects the H_2O input at the lower boundary of the model (see also

Lübken et al., 2018 (section 2)). The sentence has been modified for clarity (lines 108-114 of the revised manuscript).

"Below the MIMAS lower boundary two effects determine the mixing ratio of H_2O in the mesosphere: (i) transport of H_2O from the troposphere and (ii) oxidation of methane (CH_4). The oxidation of each CH_4 molecule produces two H_2O molecules. Methane is nearly completely converted to H_2O in the stratosphere by photochemical processes (e.g., Lübken et al., 2018). MIMAS assumes that transport from the troposphere is constant. The increase in H_2O is primarily through (ii) i.e. due to the increase in CH_4 concentration (Lübken et al., 2018)".

10. Line 118: 40 million is enough? Any justification? What is the source and properties of the dust particles?

    We believe that 40 million is enough. We investigated this in earlier studies in more detail. We added a description of the source of dust particles and included three references for more details (lines 118-120)

    "Dust particles are formed from meteors evaporating in the atmosphere (for more details, see Berger and von Zahn, 2002; von Zahn and Berger, 2003, Killiani, 2014)".

11. Line 137: Only H2O? CH4 is not photolyzed?

    Yes, you are right, MIMAS only considers H_2O. CH_4 is oxidized nearly completely to H_2O in the stratosphere.

12. Lines 139-140: HAMMONIA is not the original source for the photolysis code. Would it be TUV or something else? What is taken from Lean et al. (1997)? The description of the used spectral solar irradiance is not clear. Obviously, there is an inconsistency between H2O photolysis in the two used models.

    We have modified the sentence to make it more clear (lines 141-143 of the revised manuscript)

    "The parametrization schemes are completely discussed in Berger, 2008 (see Section 2.2). Variations of these bands are parametrized based on Lyα values according to Lean et al. (1997)."

13. Lines 157-159: Maybe it depends on the location. Some anti-correlation for the 'late' period is visible in satellite data (see 10.5194/acp-21-201-2021).

Yes, you are right. We have added some text to this in lines 161-163 (revised manuscript).
"Certainly, at low and middle latitudes, without NLCs one can detect anticorrelation. For example, in H_2O satellite data averaged over the tropics (30◦ N-30◦ S), an anti-correlation is observed for the "late" period (Karagodin-Doyennel et al. 2021)"

14. Lines 161-162: Do the authors mean temperature effect from solar variability or from GHG or from GWD via circulation?

    Our sentence has a general statement without clarification of the temperature variability source. In our numerical experiments, we consider only the variability of temperature due to solar variability and the GHG effect.

15. Line 223: It is important to emphasize the novelty of the results in Fig. 4 compared to the mentioned literature.

    We have modified the sentence to emphasize the novelty of the results in Fig.4. (lines 228-230 of the revised manuscript)

    "The results in Figure 4 illustrate the freeze-drying effect described above and also indicate that the effects of NLC on H_2O are not present below ~79 km and above ~97 km. This is the novelty of the results in Figure 4".

16. Line 253: to atmospheric absorption by which species. Should be another maximum lower down due to ozone absorption.

    We mean atmospheric absorption by molecular oxygen and water vapour. We added this in line 259 (revised manuscript).

    "Temperature differences decrease as altitude decreases because the intensity of solar radiation decreases due to atmospheric absorption by molecular oxygen and water vapour"

17. Line 275: 'increase' or 'production' is missing

    Line 282 (revised version) modified by including your suggestion.

    "Photolysis of H_2O by Lyα radiation molecules mainly produce atomic hydrogen (H) and hydroxyl (OH) in the upper atmosphere (~90 %) and with less extent to $O(^1D)$ with molecular hydrogen (~10 %)".

18. Lines 390-391: Is LIMA really an atmospheric transport model? In lines 97-98, I see 'which calculates winds and temperature'.

LIMA is an atmospheric dynamical model. We have corrected this in line 399 (revised manuscript).

"In this study, we used our ice particle model MIMAS along with atmospheric dynamics model LIMA to investigate the response of H_2O to the solar cycle from 1992-2018".

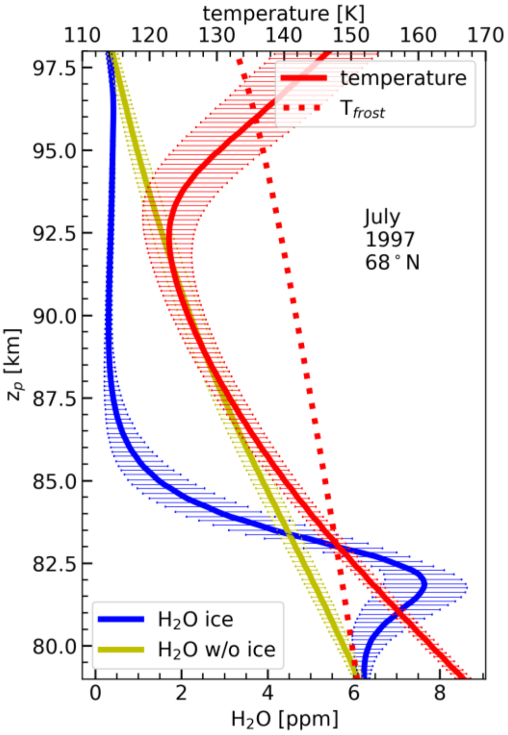
19. Lines 423-425: Why is this not the case for HALOE data?

Unfortunately, we can only speculate about this. However, what we saw from our model simulation is that the solar cycle response in the vertically averaged data can be increased if we change the altitude of averaging.

20. Conclusions: It would be nice to discuss limitations (e.g., fixed GWD) and uncertainties in the applied models. I would also expect some discussion about the statistical significance of the results related to the internal variability of the model based on first principles.

We have studied in detail the model setup and the internal variability (see also answer to Q10). Please find in the figure below an example of the mean and the standard deviation of the July mean data. The dataset includes per altitude 120 x 6 x 4 x 31 = 89280 data points. So the standard error of the mean is about 1/300 of the standard deviation. In conclusion, given the current model combination, we believe that uncertainties in the internal variability (or to low sampling thereof) of the model do not affect our conclusions. We discussed the limitation of using constant dynamics and GWD in lines 441-443 (revised manuscript).

"It should be noted that our results have limitations as they use constant dynamics for all years. We are looking forward to a new gravity wave resolving model to investigate the effects on changing dynamics due to changing GHGs and solar activity".



All typos were corrected .

Other changes are related to the recommendations and demands of other referee.

Thank you for taking the time to review our manuscript.

With respect.

Ashique Vellalassery, Gerd Baumgarten, Mykhaylo Grygalashvyly, and Franz-Josef Lübken