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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impacts of the January 2005 solar particle event on noctilucent clouds and water at the polar summer mesopause" *by* H. Winkler et al.

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We thank the anonymous reviewers, and Benjamin Murray for their constructive comments. The modified manuscript comes as a supplement file.

Reply to the specific comments of reviewer # 1 :

1) P. 1159, lines 20-22. The reviewer is confused about the prescribed vertical winds. If they are using a two-dimensional atmospheric and chemistry transport

model why do the authors not use vertical winds from that model for the sake of self-consistency? Please explain.

2) P. 1159, lines 19-20. Similarly, does the two-dimensional model calculate Kz and if so why is it not used here? Please explain and if the authors are choosing the Kz of Luebken (1992) please indicate the value explicitly and also indicate its altitude dependence.

Response: A one-dimensional model has been used for this study. The confusion may arise from our statement that this model is initialised with results for polar summer conditions from a two-dimensional atmospheric chemistry and transport model. This means that the starting values of the trace gas distributions are taken from the two-dimensional model. We have modified the description, it now reads (Sec.6.1): "For the studies presented here, a one-dimensional model of the altitude range 75–91 km at latitude 75° S is used. It is initialised with trace gas profiles for polar summer conditions from a two-dimensional atmospheric chemistry and transport model (Winkler et al. 2009), and it has the same photochemistry routines." *The two-dimensional model does a good job in terms of the large-scale zonal-mean chemistry but its resolution is rather coarse (vertical ∼3km), and the polar summer mesopause temperature is too high (∼170 K). This should correspond to too small vertical winds and adiabatic cooling. Therefore we have decided not to use the dynamical parameters of the model. The use of independent temperature, wind and diffusion parameters is of course not consistent but this would also have been the case for the combination of calculated dynamical model parameters and MLS temperature. These dynamical issues are interesting but outside the scope of our study.*
Concerning the K_z profile: A wrong reference was give, instead of Lübken (1992) it has to be Lübken (1997). This has been corrected, and the text now reads: "Molecular diffusion is neglected, and for K_z the eddy diffusion coefficient profile for polar summer conditions from Table 3 in Lübken (1997) is used."

3) P. 1161, lines 4-6. The authors should know that the MLS temperatures have been used in a global high-altitude data assimilation system, which accounts for the changing geolocation of the MLS (and SABER) measurements over time throughout the meso- sphere. A sentence or two with a reference to this work would be appropriate here [Eckermann et al., JASTP, 71, 531, 2009]. The authors should consider these results and perhaps compare to what they are using. The output ïňĄles are available through anonymous ftp at map.nrl.navy.mil and then "cd pub/nrl/aim9c". The January 2005 time period is available.

Response: This is an interesting data set. I have done some comparisons, but decided not to include a detailed discussion in the article. A few lines of text have been added, though not in Sec. 6.2 as suggested, but in Sec. 7. (Results) when the MLS uncertainties and the bias with respect to SABER was mentioned (in response to your comment 6):
"MLS temperatures in combination with SABER data have been used in the assimilation system NOGAPS-ALPHA (Hoppel et al, 2008; Eckermann et al, 2009). A comparison of the MLS temperatures used in our study with the corresponding assimilated temperatures (not shown) does not reveal a clear pattern. The MLS temperatures tend to be smaller (typically 1–3 K) than the assimilated ones. However, during the temperature maximum on 21–23 January, the NOGAP-ALPHA temperatures are smaller (1–4 K) than the MLS temperatures. The differences might partly be due to fact that the temperatures used here are MLS data version 3.3 whereas the assimilation system used version 2.2. No final conclusion can be drawn from this comparison without further investigation."

4) P. 1161, lines 12-13. A tidal amplitude of 2 K in the polar summer mesopause region is small compare to observations in the northern hemisphere. An amplitude of 4 K is more reasonable [Singer et al., Adv. Space Res., 31, 2055, 2003; Stevens et

al., JGR, 115, D18209, doi:10.1029/2009JD013225, 2010]. Please comment on how the results change with a tidal amplitude that is twice as large.

Response: The simulations were repeated with a diurnal amplitude of 4 K (and unchanged 2 K semidiurnal amplitude). In Fig. 1 the resulting diurnal icy particle distribution is shown in comparison with the case of a 2 K diurnal amplitude. For 4 K, the abundance of NLCs during the first maximum is a bit smaller, and larger during the second maximum in the evening. However, the differences are not very big, and the general pattern remains the same. Figure 2 shows that increasing the diurnal amplitude to 4 K does not significantly change the response of the icy particles to the SPE. These figures are not shown in the article, but a few words have been added to the text (Sec.6.2): "The amplitudes of both diurnal and semi-diurnal variation are 2 K. These values have been chosen to reproduce the diurnal temperature variations of the Forbes and Gillette (1982) model at 60° S at the December solstice, see Figure 3 in Jensen et al. (1989). As disscused in Jensen et al (1989), the model of Bjarnason (1987) predicts larger tidal amplitudes of $\sim$4 K for 65° N during summer. This is in agreement with temperature observation at the polar summer mesopause in the northern hemisphere (Singer et al., 2003; Stevens et al., 2010). We have performed additional model simulations with a diurnal amplitude of 4 K, and a semi-diurnal amplitude of 2 K. The results of these simulations (not shown) do not differ significantly from the results assuming a 2 K diurnal amplitude, and would not change the conclusions drawn here."

5) PP. 1163-1164. There needs to be more discussion here on how the model is compared to the data throughout this section of the paper. For example, is the model sampled only at the local times of the observations and what exactly are the local times included in the observations presented (see Technical Corrections below)?
Response: See Technical Corrections 2+3

Technical Corrections:
1) P. 1154, Lines 16-17. “the effects can clearly” should be “can the effects clearly”.
Response: Changed

2) P. 1158, Lines 7-9 and Lines 15-17. The authors indicate the local time of the measurements used but do not indicate how the zonally averaged NLC occurrence rates or temperatures are averaged. Do the authors average observations from both nodes of the satellite orbit (all local times) together? Please be explicit.
3) P. 1159, Lines 6-7. As indicated above, please explicitly indicate how the water vapor observations were averaged together.

Response: At the beginning of Sec. 7 it is now explained: “All MLS data shown in this section are daily mean zonally averaged values. Averaged are all MLS measurements in the latitude band 70–80° S during one day. Used are MLS profiles from both the acsending (03:15–05:15 p.m.) and the descending node (10:05 p.m.–00:00). Similarly, the SCIAMACHY and MIPAS data presented are averages of all measurements in the latitude band 70–80° S during one day. All of these measurements fall in the time periods 11:30 p.m.–03:00 a.m. and 05:00–8.30 a.m.”
Some lines later, before the first comparison of model with measurements, it was inserted: “The daily satellite data used here are averages of all observations in limited time windows corresponding to the satellites' overpass times. For the purpose of comparison, the model output has been averaged over these time windows. For the comparison with MLS, the model was averaged over 03:15–05:15 p.m. and 10:05 p.m.–00:00. For SCIMACHY and MIPAS comparison, the model was averaged over 11:30 p.m.–03:00 a.m. and 05:00–8.30 a.m.”

4) P. 1158, lines 17-18. Please indicate exactly how geopotential height is converted to geometric altitude with an equation or a reference that has the relationship explicitly written out.

Response: In that paragraph it was added: “The geometric altitude z and the geopotential height H are related as

$$z = \frac{H}{1 - H/R_e} \tag{5}$$

where R_e is the radius of the Earth (6378.1370 km). Equation (5) follows from the definition of the geopotential height, e.g. Andrews et al. (1987), and the inverse-square law of gravity.”

5) P. 1161, line 19. Please explicitly state the radius value in the “smallest radius bin”. The reader needs to know the approximate sizes of the particles that are nucleating and the approximate size to which they grow so the authors should state this here as well.

Response: Behind “smallest radius bin” *it was inserted:* “(3–3.8 nm)”. *A few lines later, when the results of the two model versions are compared, it was added:* “The particle radii (not shown) are largest in the altitude range 80–83 km. At 81.5 km, the mean particle radius varies between 30 nm and 80 nm, being largest during the second ice mass maximum in the evening. The fraction of particles with radius $>$120 nm is always smaller than 1%. All of this applies to both model versions.”

6) P. 1164, lines 17-18. Please quote the MLS temperature uncertainty here explicitly.

Response: The text was changed to: “In the following, results from model runs with MLS temperatures minus 2 K are shown. 2 K is well inside the uncertainties of the MLS data in the considered altitude range, The precision of individiual MLS temperature profiles is about ±2.5 K, and there is a modelled bias uncertainty of (2 ± 3) K

(Schwartz et al., 2008). The fact that our model results agree better with the NLC observations if the MLS temperatures are lowerd by 2 K does not necessarily mean that the MLS temperatures are too high. It might be that the simple one-dimensional model using zonally averaged daily mean temperatures overestimates the icy particle sublimation as it neglects variations of the three-dimensional temperature field, and three-dimensional transport. [...] Compared to temperature data from the SABER (Sounding of the Atmosphere using Broadband Emission Radiometry) instrument (Mlynczak and Russel, 1995) the MLS temperature has a negative bias uncertainty of up to 9 K (Schwartz et al., 2008)."

7) P. 1165, lines 19-20. Please indicate the mismatch in mesopause altitude between MIPAS and MLS explicitly here.

Response: The text was changed to: "It has to be taken into account that the MLS mesopause altitude is about 4 km higher than the MIPAS mesopause altitude (García-Comas et al.,2011). This might cause the difference between MIPAS and MLS water."

8) Some of the references are not in alphabetical order. Please take care to review the reference list.

Response: Should be better now

Reply to the specific comments of reviewer # 2 :

General comments
... The conclusion that sublimation of NLCs leads to signiïňĄcant changes in the water vapor distribution in the upper mesosphere is certainly plausible, but it is not

substantiated by the results shown in the paper. It could be that the major reason for this is the signiïňĄcantly degraded vertical resolution of the MLS satellite instrument that was the prime measurement data source used for this study.

Response: Our statements on the water comparison might have been a bit too positive. This has been modified, in particular in the summary and conclusions section.

SpeciïňĄc Comments

Page 1152, line 6: The sentence about sublimation is not proven in the opinion of this reviewer.
Page 1152, line 10: Suggest adding "much" in front of the word "stronger"

Response: That part of the abstract was modified.: "The model calculations indicate that the sublimation of noctilucent clouds leads to significant changes of the modelled water distribution in the mesopause region. These model predictions are compared with H_2O measurements from the MLS and the MIPAS/Envisat satellite instruments. In general, the modelled effect of water redistribution is stronger than the observed one."

Page 1152, line 26: The sentence beginning with "Because" assumes that the particles have non-icy cores however, this is not yet proven. Suggest changing the sentence to read "Because the ice cores are likely to be non-ice, the term..."

Response: This is true. The sentence was changed to "Because the particle cores are likely to be non-ice, the term "icy particles"..."

Sections 2, 3 and 4 give excellent descriptions of the physical, chemical and charge coupled processes.

Response: Thank you.

Page 1158, line 6: Make the word "time" plural
Page 1158, line 7: Change "is" to "are"
Page 1158, line 14: Make the word "time" plural
Page 1158, line 15: Change "is" to "are"
Page 1158, line 20: Delete "in detail"
Page 1158, line 22: Change "pictured" to "observed"
Page 1159, line 6: Make the word "time" plural and change "is" to "are"
Page 1160, line 5: Insert the words "...instruments is several to many km..."
Page 1160, line 7: Insert "would" after "data"
Page 1160, line 11: Change sentence to read "...justifies use of a fixed H2O value at..."
Response: Changed

Page 1163, line 14: The AIMOS model used to calculate the ionization rates in Figure 2 should be referenced. Also, "ionization" is misspelled in the text and on the figure.

Response: The reference to the AIMOS model is given at the beginning of Sec. 6.3. In the figure caption, a reference to that Section was added. Concering "ionisation" vs "ionization": Well, I'm not a native English speaker, and maybe I'm wrong, but I thought "ionisation" would be the British English form. "s" is used throughout the whole text. Actually, the only exception is AIMOS (Atmospheric Ionization Module Osnabrück) because this is a "name". Also "modelled" instead of "modeled" is used.

Page 1164, line 17: Delete the minus sign in front of the 2 K and replace "are" with "is"

Response: Changed

Page 1164, line 19: The paper suggests two reasons for data and model disagreement, i.e. satellite data uncertainties and deïňĄciencies in the model. A third possibility is that the measurements on the limb give the line-of-sight mean temperature which generally is always higher than the ice temperature. See the paper by Hervig and Gordley, J. Geophys. Res, VOL. 115, D15208, 9 PP., 2010 doi:10.1029/2010JD013918.

Response: This is a good advice. After "It might be that the simple one-dimensional model ... overestimates the icy particle sublimation ... as it neglects .... and three-dimensional transport." it was added: "On the other hand, Hervig and Gordley (2010) have pointed out that for limb measurements, the retrieved temperature is a line-of-sight mean which is indeed generally higher than the ice temperature."

Page 1164, line 23: Insert "modeled" in front of "water" in the second sentence.
Page 1164,line 26: Delete "Complementarily"

Response: Done

Page 1164, line 26: This sentence states that the water abundance above 82 km is small because of water uptake by ice particles but it does not consider photolysis which is the main reason for the decline in water with altitude. Maybe there is a point being missed here regarding what the authors are intending to say that can be clariïňĄed.

Response: Sure, the photolysis off H_2O works continously, and the sentence could be misleading. It was replaced by: "At higher altitudes the uptake of water by the icy particles reduces the water concentration, and due to the sublimation of the icy particles during the SPE, the water abundance increases, e.g., from ∼0.5 to ∼5.5 ppm at 86 km."

Page 1165, line 1: Figure 8 does not support the case for the cause of changes in water vapor after the SPE. MLS shows no increase at the 84km level during days 21- 24 that is any greater than the increase that is seen for example around day 10. The small and broad increase in MLS data after about day 21 remains an increase through day 38 unlike the model which shows a rather sharp decline after day 30. Also, why is the low altitude model increase peak at around 84 km occurring before the 90 km peak by about 1.5 days? The model H2O time series without the vertical averaging shows these peaks occurring at the same time. The vertical resolution of the MLS data coupled with the model uncertainties make it very difïňĄcult to draw a solid conclusion about the effect of the SPE on water vapor caused by NLC sublimation.
Page 1165, line 4: Insert "MLS" in front of "water"

Response: Indeed, the agreement between smoothed model result and MLS in Fig. 8 is not very good, and we did not want to claim this. I have removed the word "somewhat" before "stronger effect", and inserted a "small" before "decrease", so that the text now states: "The smoothed model result still shows a stronger effect than the MLS data. There is a moderate increase of the MLS water mixing ratio during 19–22 January at higher altitudes and a small decrease below the icy particle layer." *The water peaks at 84 and at 90 km do not occur at the same time in the unsmoothed model plot. Maybe the contour plot makes it a bit difficult to see. Figure 3 clearly shows the lag.*

Page 1165, line 15: The authors are speculating about the accuracy of the MLS water versus the MIPAS water. It would be helpful to include a deïňĄnitive statement about the MLS water accuracy based on a MLS validation paper that could be referenced.

Response: The text was changed to: "In the following, results from model runs with MLS temperatures minus 2 K are shown. 2 K is well inside the uncertainties of the MLS data in the considered altitude range. The precision of individiual MLS temperature profiles is about ±2.5 K, and there is a modelled bias uncertainty of (2 ± 3) K (Schwartz et al., 2008). The fact that our model results agree better with the NLC observations if the MLS temperatures are lowerd by 2 K does not necessarily mean that the MLS temperatures are too high. It might be that the simple one-dimensional model using zonally averaged daily mean temperatures overestimates the icy particle sublimation as it neglects variations of the three-dimensional temperature field, and three-dimensional transport. [...] Compared to temperature data from the SABER (Sounding of the Atmosphere using Broadband Emission Radiometry) instrument (Mlynczak and Russel, 1995) the MLS temperature has a negative bias uncertainty of up to 9 K (Schwartz et al., 2008)."

Page 1165, line 27: The first complete sentence says that the H2O decrease at lower altitudes shown by the model is not present in the MIPAS data. However both the model and the MIPAS data show an increase at lower altitudes not a decrease.

Response: This is meant for 21-22 January as in the previous sentence. To make this clear it now states: "The water decrease at lower altitudes at the same time ..." *The effect is not very large, but it is by no means an increase. For instance, at 81 km the smoothed model water H_2O decreases from 5.3 ppm to 4.9 ppm between January 20th and 22nd. MIPAS does basically not change at that altitude.*

Page 1166, line 6: Make "abundance" plural and change "is" to "are"
Page 1166, line 8: Insert "is" in front of "negligible"
Page 1166, line 28: Insert "model calculations indicate that" after "Additionally"
Page 1167. line 2: Delete "respectively"

Page 1167. line 3: Insert “on NLC occurrence” after “depletion”
Page 1167, line 7: Insert “model indicates” in front of “are”

Response: Changed

Page 1167. line 11: The statement that the modeled water vapor generally agrees with the observations is not substantiated in the paper
Page 1167, line 13: Replace “somewhat” with “much”

Response: The end of the story has been rewritten. It now reads: ”Even if the vertical resolution of the MLS and MIPAS water measurements are taken into account, there are significant differences between the observed and the modelled water distribution. The model predicts stronger gradients and more pronounced changes during the SPE than observed by the satellite instruments. This might be attributed to the simple one-dimensional model approach which in particular neglects horizontal transport processes.”

Reply to the comment of Benjamin Murray on nucleation mechanism:

Preliminary remark: There is already a reply to the comments of Mr Murray. Here only the changes made to the manuscript are listed:

1. Introduction:
The part concerning homogeneous nucleation was rewritten, and now reads: “The temperature measurements of Lübken et al.(2009) indicate that homogeneous ice nucleation can be possible at the polar summer mesopause, see also Murray and Jensen (2010). Condensation nuclei are believed to significantly facilitate the formation of NLC particles, e.g. Gumbel and Megner (2009); Megner and Gumbel (2009).”

2. Description of icy partile growth
The confusing hexagonal/cubic part was repaired: “To the authors best knowledge, there are no direct measurements of the water saturation pressure over ice of cubic structure in the range of the polar summer mesopause temperatures. For hexagonal ice there is a useful fit by Murphy and Koop (2005) for p_∞ in Pascal valid for temperatures down to 110 K:

$$\begin{aligned} p_\infty &= \exp(9.550426 - 5723.265/T \\ &+ 3.53068\ \ln(T) - 0.00728332\ T) \end{aligned} \tag{3}$$

This saturation pressure is used in the NLC model of Bardeen et al. (2010), and similar values based on the expression $\ln(p_\infty) = 28.548 - 6077.4/T$ ($\pm 5\%$ difference with respect to Eq. 3 for T=110–150 K) have frequently been used in NLC research, e.g. Jensen and Thomas (1988); Berger and von Zahn (2002); Chu et al (2003). Following these studies, here the saturation pressure corresponding to hexagonal ice is used.”
... and information on the surface tension has been added: “Eq. (4) is a combination of the temperature dependent ice-vapour surface tension (Hale and Plumer, 1974) in the numerator, and a factor accounting for the decrease of the surface tension with radius $(1 + 2\delta/r)^{-1}$ (Tolman, 1949). δ is an emperical factor for very small water or ice particles for which the value $\delta = 1.5 \times 10^{-10} m$ of Turco et al. (1982) is used.”

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/12/C2135/2012/acpd-12-C2135-2012-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 1151, 2012.

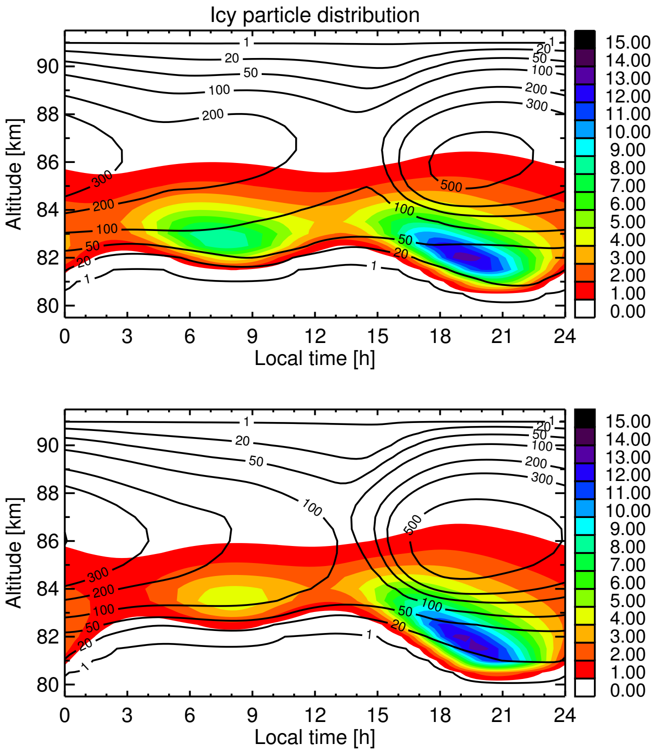

Fig. 1. Modelled icy particles at 75 the period of one day (14 January). Upper panel: Simulation with 2\,K diurnal amplitude. Lower panel: Simulation with 4\,K diurnal amplitude

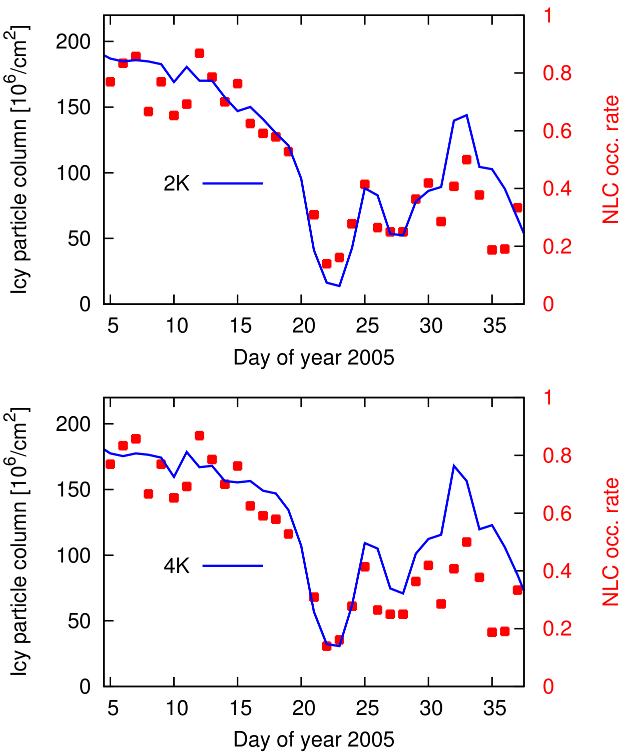

Fig. 2. Total column of icy particles. Upper panel: Simulation with 2\,K diurnal amplitude. Lower panel: Simulation with 4\,K diurnal amplitude

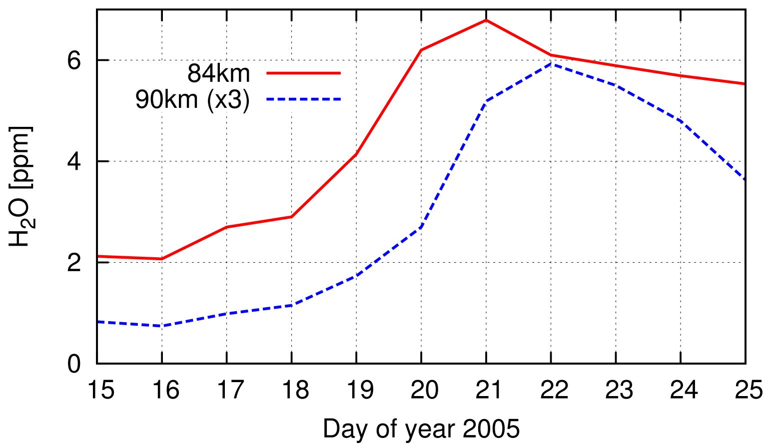

Fig. 3. Modelled H$_2$O [ppm] at two altitudes, the solid red line shows water at 84\,km, and the dashed blue line water at 90\,km increased by a factor of three.