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Reply on RC1

Peter Dalin et al.

Author comment on "What caused the frequent and widespread occurrences of noctilucent clouds at middle latitudes in 2020?" by Peter Dalin et al., Ann. Geophys. Discuss., <https://doi.org/10.5194/angeo-2021-28-AC1>, 2021

Dear Editor,

We thank two Reviewers for their useful comments and suggestions, which improved our manuscript. In the revised version of the manuscript we have carefully addressed Reviewers comments and suggestions. Our detailed replies are provided below.

Reviewer 1.

Review of "What caused the frequent and widespread occurrences of noctilucent clouds at middle latitudes in 2020 " by Dalin et al.

General Comments:

This paper describes mid-latitude NLCs, with an emphasis on the enhanced number of NLCs seen during the Northern summer of 2020. The Authors attribute the active 2020 season to volcanic enhancement of water vapor in 2015, with a time lag of 5 years for the water vapor to be transported from the lower stratosphere to the mesopause.

I recently reviewed this paper when it was submitted to JASTP, and note that the paper was rejected at that time (March 2021). In reading the paper now, I see that the Authors have done very little to improve the manuscript, and in particular, they have ignored most of the comments I made in my review for JASTP. As such, my review below is largely a restatement of concerns from the JASTP submission. It is my opinion that Authors approach here was inconsiderate of other peoples time.

Indeed, we have ignored most of the comments that Reviewer 1 made in the JASTP review since most of these comment are irrelevant or wrong. Please see our replies below.

1) The Authors argue that transport from the lower stratosphere to the polar mesopause requires roughly 5 years, an assertion that is not supported by the paper. There is an

attempt to quote some previous studies that addressed transport from the troposphere to the stratosphere, and within the stratosphere and mesosphere, but the Authors did not derive a convincing story from these previous works. They alternately quote stratosphere - mesopause transport times as 2 years and 5 years. As this is the key aspect of their argument, it is mandatory that they get this part of the story correct, and support it clearly with recent references. If the Authors can make clear and convincing arguments regarding H₂O transport and lifetime, then I believe this study will constitute a significant new result. In comparing the JASTP submission and the present paper, I see that they completely ignored my comments in this regard.

This story is simple and complex simultaneously. The story is simple because there are simply no studies at all considering volcanic H₂O transport through the mesosphere to the mesopause region. The story is complex because there are a few papers considering transport of volcanic H₂O in the stratosphere. We have added some references, which we could find, on this issue in the Discussion of the revised manuscript.

As up to "They alternately quote stratosphere -mesopause transport times as 2 years and 5 years", Reviewer 1 is wrong. We do quote the transport time of 2 years when discussing the H₂O transport from the upper stratosphere/ lower mesosphere to the mesopause, and the transport time of 5 years when discussing the H₂O transport from the lower stratosphere to the mesopause. All is consistent and there is no error in our logic. Please read carefully the Discussion of our paper.

2) The study is based on a correlation analysis, where the visual NLC record is compared to time series of temperature, water vapor, solar Lyman-alpha, and volcanic emissions. As such, it would be very useful to have a figure where the NLCs are plotted along with T and H₂O, and another figure where NLCs are shown along with Lyman-alpha and volcanic emissions. Currently, all of these time series are in different figures and it is difficult to see if there are correlations or not.

We disagree with this comment. When plotting NLCs as a function of temperature, water vapor, solar Lyman-alpha and volcanic emissions, all is clear to the reader to demonstrate the original dependence of NLC time series on the analyzed data sets. Note that we demonstrate analyzed parameters for various altitudes and latitudes, and it would be complicated to the reader to look at multidimensional figures having many data sets at different altitudes and latitudes.

Also, we should note that it is not the purpose of the present paper to establish "if there are correlations or not". We would do it by implementing a robust correlation analysis estimating correlation coefficients and their 95% confidence intervals and summarizing all coefficients in a table. But again, it is not the purpose of the present paper.

Specific Comments:

1. Line 54: The statement "meaning that the summer mesopause at middle latitudes has become more wet" is redundant.

We have deleted the second part of this statement in line 53.

2. Line 150: This definition is unnecessary, given the audience.

We disagree with this statement. We do not actually understand what kind of

audience does Reviewer 1 mean? We address our paper to a broad scientific audience and in our opinion this definition is necessary. We keep this definition unchanged.

3. Line 163: Please state the time frame for the MLS results, i.e., are they 1 or 2 day means?

In our opinion, this information is unnecessary since in the previous lines 137 and 138 we have clearly indicated that “we analyze nighttime temperature and nighttime water vapor measurements”. In the lines 164-167 we also provide this information. We believe that it is well understood to the reader that these are “1 day mean”.

4. Page 5; Fig 1: It looks like the Gaussian fit ignores features like the cold periods around day 170 & 195. Do cold times like these correlate to more NLC sightings? It would be interesting to add a plot of the NLC sightings underneath the T time series.

First. The Gaussian fit does not ignore any feature in the estimated points. The Gaussian fit is a mathematical object that cannot judge what points should be or should not be ignored. The Gaussian fit is estimated using a least-squares method and it might go above or below any estimated point. We can provide all the data in a digital form to Reviewer 1 in order to judge if “the Gaussian fit ignores features like the cold period...”

Second. The cold times themselves cannot answer the question if “do cold times like these correlate to more NLC sightings?”. This question is complicated and is based on at least three following factors:

a) Actual amount of water vapor should be taken into account. In other words, the frost point temperature should be calculated and should be compared to an actual MLS temperature. We can do it, no problem, but this is beyond the scope of the present paper.

b) The frost point temperature should be estimated in a rather small volume of the mesopause as closest as possible to an actual volume of the mesopause filled with NLCs. It is impossible to do in the present study since we integrate over a very large volume of the mesopause of 5 degrees in latitude and over 360 degrees in longitude to calculate mean nighttime values of the temperature and water vapor concentration.

c) At the same time, based on our own previous results dealing with comparing MLS temperatures and water vapor data and actual NLC occurrences (see Dalin et al., 2011), we can provide a rule of thumb: temperature at the mesopause should be lower than the frost point temperature of 145-147 K, i.e., this is the temperature threshold below which actual NLCs exist. It is important to note that this experimentally derived temperature threshold coincides well with a theoretical temperature threshold of 147 K for the saturation value equal to 1 at 83 km altitude as estimated by Berger and von Zahn (2002). Please see the following papers:

Dalin P., Pertsev N., Dubietis A., Zalcik M., Zadorozhny A., Connors M. et al. (2011), JASTP, 73, 2097-2109.

Berger U., and von Zahn U. (2002), JGR, 107, A11, 1366.

5. Line 187: This statement makes no sense: "...with 42 NLC cases in 2020 in the past 30 years...", please clarify.

We have corrected this sentence in the revised manuscript.

6. Line 296: There are other relevant references that you should mention here like Hartogh et al. (2010); Hervig and Siskind (2006), and Lübken et al. (2018). In looking at these papers you will see that there is some disagreement on what the H₂O - solar response actually might be.

Yes, we are aware of these papers and are aware of "some disagreements on the H₂O- solar response". Here we talk about a general and well-known physics concerning the anticorrelation between Lyman alpha and water vapor in the upper mesosphere and the mesopause, which is also mentioned in Hartogh et al. (2010); Hervig and Siskind (2006). Here we do not cite Lübken et al. (2018) because this paper is not related to this issue. We have added a sentence describing "some disagreements" in the revised manuscript.

7. Figure 8: it would be appropriate to only show results form 2004 - present, as this is the time period you analyze for NLCs, T, and H₂O.

We disagree with this comment. In our opinion, it is very interesting to the reader to look at all available Lyman alpha data since there was the lowest solar activity in the last 60 years occurred in the summers of 2019 and 2020. It is not a problem to take a closer look at the period 2004 - present. We keep Figure 8 unchanged.

8. Line 349: The link goes to a missing page.

We have replaced the old link with a new one:

https://disc.gsfc.nasa.gov/datasets/MSVOLSO2L4_4/summary?keywords=MSVOLSO2L4_3

9. Figure 9: I think the curve in Fig 9c is supposed to be from subtracting the red and black curves in Fig 9b. It appears, however, that there is still a strong linear component in Fig 9c, that seems like it should be absent since the black curve is basically a straight line. Please comment and/or check the results.

In lines 319-320, we have clearly indicated that "After the subtraction of the solar term from water vapor measurements using equation 1, we arrive at H₂O residuals shown in Fig. 9c." Besides, in the caption to Fig. 9c, it is clear stated the following:

"H₂O mixing ratio residuals after the subtraction of the solar term and the regression constant C_{01} from water vapor measurements (see text)." It means that the time term of Equation 1 WAS NOT subtracted from the water vapor data. We have clarified this issue, although it was already clearly stated in the text.

10. Line 322: Please state here the pressure and latitude for the H₂O used in the analysis.

We disagree with this minor correction. We have already TWICE clearly stated the pressure and latitude for the H₂O in the top caption of Figure 9 as well as to

the caption of Fig. 9b. In our opinion, Reviewer 1 has inattentively reviewed our paper. We keep this issue unchanged.

11. Line 337: This statement gets to the heart of your work, and needs a reference to support it. In fact, the amount of water injected into the stratosphere by volcanoes seems to be highly variable, and is still debated in the literature. Since you ignored this comment last time, I list below some relevant references. You need to look at these and other papers, and summarize them in your article.

LeGrande, A., Tsigaridis, K. & Bauer, S. Role of atmospheric chemistry in the climate impacts of stratospheric volcanic injections. *Nature Geosci* 9, 652–655 (2016).
<https://doi.org/10.1038/ngeo2771>.

Joshi, M. M. & Jones, G. S. The climatic effects of the direct injection of water vapour into the stratosphere by large volcanic eruptions. *Atmos. Chem. Phys.* 9, 6109–6118 (2009).

Glaze, L. S., Baloga, S. M. & Wilson, L. Transport of atmospheric water vapor by volcanic eruption columns. *J. Geophys. Res.* 102, 6099 (1997).

We are aware of these and other relevant references. Although the paper by LeGrande et al. (2016) is a very interesting research, it deals with chemical effects of volcanic water vapor on the evolution of stratospheric global-mean aerosol optical depth following a volcanic eruption within 3 months. Thus, this paper is not appropriate for our study and we do not cite it.

In the literature, we could find just a few studies on volcanic water vapor amount being injected into the atmosphere. Kern et al. (2017) have used passive differential optical absorption spectroscopy (DOAS) measurements to study the plume of the Sabancaya volcano eruption in 2016. The authors have obtained an H₂O emission rate of 250,000 ± 75,000 ton/day. Sioris et al. (2016), analyzing Aura/MLS satellite water vapor data for the 2015 Calbuco eruption, have inferred the mass of stratospheric water vapor from this eruption of about 2 Mton. Schwartz et al. (2013) used Aura/MLS water vapor measurements to demonstrate that the 2008 Kasatochi eruption injected water vapor into the lower stratosphere at midlatitudes: about 10–13 ppmv was measured at 100 hPa (about 16 km) and more than 9 ppmv was registered at 82.5 hPa (about 18 km). These values highly exceed mean H₂O values of 4.5 ppmv and 4.3 ppmv at 100 hPa and 82.5 hPa, respectively, at northern midlatitudes. Pitari and Mancini (2002) have performed model simulations of the Mt. Pinatubo eruption in 1991 and have demonstrated that it injected 37.5 Mton of water vapor as well as the majority of ice particles into the stratosphere. Sigurdsson and Carey (1992) have estimated that about 2 × 10¹² kg (about 2000 Mton) of magmatic water vapor was transported to the stratosphere by the 1815 Tambora eruption. Note that the entire stratosphere contains only about 10¹² kg (about 1000 Mton) of water vapor (Glaze et al., 1997). Thomas (1991) estimated about 100–200 Mton of water vapor injected into the atmosphere by the Krakatoa eruption in 1883. In general, Glaze et al. (1997) have noted the following: “A large eruption column rising in a wet atmosphere could inject 4 × 10⁹ kg (4 Mton) of water vapor per hour. Twenty-four hours of more or less continuous activity could deposit a mass of water vapor equivalent to the yearly contribution by methane oxidation, 100 midlatitude thunderstorms, or 7% of the total stratospheric loading.” Joshi and Jones (2009) have made model simulations demonstrating that some large volcanic eruptions can deposit a climatically significant amount of water in the stratosphere. At the same time, it is important to note that Kern et al. (2017) have concluded: “Despite its abundance, there are a number of challenges

associated with accurately determining volcanic H₂O emission rates, and measurements of H₂O output are therefore relatively seldom reported.”

Summarizing the issue on the H₂O volcanic water vapor amount in the atmosphere, there are no reliable continuous measurements of the amount of volcanic water vapor injected into the stratosphere and there are no estimations of volcanic H₂O being transported through the mesosphere to the mesopause region. Considine et al. (2001) have performed model simulation of atmospheric effects after the Pinatubo eruption in June 1991 and have shown that it takes about 4 years for volcanic water vapor to propagate into the upper stratosphere and lower mesosphere, resulting in peak increases of about 6% in early 1995.

We have provided this information in the revised manuscript.

12. Line 357: It seems like the SO₂ plume mass would be relevant to consider as well as the altitude. These data are in the data base that you used, and it would be useful to plot the plume mass results in addition to altitudes (perhaps as an addition to Fig 10), as this would help the reader interpret the volcanic impact. For example, is it possible that there were massive injections that did not get very high, or vice versa?

During the manuscript preparation, we have analyzed both altitudes and SO₂ masses of volcanic plumes. We could not find a reasonable correlation between H₂O in the mesopause and stratospheric SO₂ mass for 2020, but we find a good correlation between H₂O and SO₂ altitudes for 2019 and 2020 as clearly demonstrated in Figure 11. The relation between the amount of volcanic SO₂ and survived volcanic H₂O molecules in the mesosphere and mesopause is completely unknown. There are no scientific papers investigating this important issue. Thus, we are allowed to investigate the influence of SO₂ altitude as a proxy for volcanic activity, which does demonstrate a positive correlation with H₂O in the mesopause for 2019 and 2020.

In the revised manuscript, we have added a couple of sentences describing SO₂ mass analysis during the manuscript preparation.

13. Figures 10 & 11: You need to plot the volcanic height / emission time series along with the time series of water vapor and NLCs. This will allow the reader to visually assess the correlations, or lack thereof.

We disagree with this comment. The correlation analysis between the volcanic height and water vapor is clearly present in Figure 11 and the whole section 4.4 is dedicated to this topic.

We do not investigate the correlation between the volcanic height and NLC activity since there are FOUR different NLC databases shown in Figure 3 (which NLC database should we correlate with volcanic activity?). The only common feature for all these databases is the increased NLC activity in 2020, which is a topic of the present paper.

14. Line 420: This is a rather long sentence.

We have shortened this sentence in the revised manuscript.

15. Line 464: Temperatures of 175K are above the frost point, and this discussion is therefore confusing. It would be more relevant to analyze the 0.0046 hPa results, which is closer to where NLCs form. Please comment on this.

We have recalculated the S values at 0.0046 hPa, with the temperature drop from 153 to 149 K and water vapor increase from 5.0 to 5.6 hPa. The results are about the same and presented in the revised manuscript.

16. Line 467: Here you state that both temperature and water contributed to the increased NLC activity in 2020. Yet in the abstract and conclusions, you state that the 2020 NLCs were due mostly to increased H₂O. Please check these statements and be sure they are consistent.

We disagree with this comment. In the Abstract (lines 55-57) and Conclusions (544-546) we have clearly stated: "A combination of lower mesopause temperature and water vapor concentration maximum at middle latitudes is the main reason for frequent and widespread occurrences of NLCs seen around the globe at middle latitudes in the summer of 2020."

Thus, we are consistent and keep all these unchanged.

17. Line 482: You cannot state that plume height had a positive impact on H₂O. Your correlation analysis does allow you to state that plume heights are positively correlated with enhanced H₂O, with a time lag. Please re-word this statement.

We have re-worded this sentence in the revised manuscript.

18. Paragraph starting on Line 494: Here you discuss transport times for gases emitted in the troposphere to reach the stratosphere (~5 years). However, your hypothesis is to only consider volcanos that injected material directly into the stratosphere. Below you state that the transport time from the stratosphere to the upper mesosphere is ~2 years. If this is true then your entire study has a major problem. This is because your analysis considered only volcanic plumes that penetrated into the stratosphere, and that the time lag to reach the NLC region is therefore 2 years and not 5 years.

It seems that Reviewer 1 has inattentively read this paragraph. Indeed, in lines 505-508 we clearly indicate the following:

"Thus, it takes about 4–6 years for inert trace gases to reach the polar atmosphere at 30-55 km altitude from the tropical troposphere. Then it takes them two more years to rise throughout the mesosphere and reaching the mesopause region at 85-87 km altitude where NLC ice particles start to form."

Thus, TWO years refers to the transport time within the MESOSPHERE and MESOPAUSE, not "from the stratosphere to the upper mesosphere".

We do consider only volcanic plumes higher than 10 km, i.e., penetrating the LOWER STRATOSPHERE (please see again and carefully Figure 10). That is why we get a time lag of 5 years (not 2 years), which is needed to transport water vapor from the lower stratosphere to the mesopause region.

Thus, there is no problem in these transport time estimations and we keep this paragraph almost unchanged.

19. Line 497: It is irrelevant here to consider the transport times of long lived tracers (e.g., HF). This is because tropospheric water vapor is very short lived, with lifetimes of less than 10 days (please read van der Ent and Tuinenburg, 2017; Hodnebrog et al., 2019). As a result, a water vapor plume in the troposphere will never impact the stratosphere, as it will enter the hydrologic cycle and be dispersed long before it can travel to the middle atmosphere. Furthermore, the bulk of tropospheric H₂O that enters the

stratosphere does so via the tropical tropopause, where the entry value is limited by cloud formation.

We poorly understand this logic of Reviewer 1. As we have pointed out many times, we have considered only volcanic plumes higher than 10 km, i.e., NOT in the TROPOSPHERE!

We do NOT consider the transport, hydrologic cycle and lifetime of water vapor in the troposphere. Thus this comment is totally irrelevant to our study.

As far as “the bulk of tropospheric H₂O that enters the stratosphere does so via the tropical tropopause, where the entry value is limited by cloud formation” is concerned, this is partly true since it is also well known that middle latitude volcanoes can directly transport water vapor into the midlatitude stratosphere. For example, Sioris et al. (2016), analyzing Aura/MLS satellite water vapor data for the 2015 Calbuco eruption, have inferred the mass of stratospheric water vapor from this eruption of about 2 Mton. Schwartz et al. (2013) used Aura/MLS water vapor measurements to demonstrate that the 2008 Kasatochi eruption injected water vapor into the lower stratosphere more than 9 ppmv to 18 km altitude at northern midlatitudes.

Summarizing, all is correct in this paragraph and we keep it almost unchanged.

20. Line 507: The relationship between Krakatoa and the first reported NLCs is NOT a well-known fact, but rather a common speculation. Please revise this statement accordingly.

We strongly disagree with this comment. The relation between Krakatoa and the first reported NLC is well established in the NLC scientific community. To support this, we cite the following fundamentals works:

Gadsden and Schröder (1989), Noctilucent clouds:

Page 125: “After the eruption of Krakatoa in the Sunda Straits in 1883, notable twilight phenomena were seen all around the world (cf. Förster 1906; Kießling 1885; Pernter 1889 and the report by the Krakatoa committee of the Royal Society edited by Symmons 1888). It was soon conjectured that noctilucent clouds might be caused by volcanic eruptions because they appeared with increased frequency after other volcanic eruptions (see Table 10.1).”

Page 126: “The two periods of increased noctilucent cloud activity (1885-1890 and 1963-1968) followed the two great volcanic eruptions of Krakatoa in 1883 and Agung in 1963. These eruptions were of the so-called Plinian type (cf. Ertel 1967) the events are violently explosive because plugged vents cause the buildup of large pressure with the conversion of magmatic water to steam. The efflux from Plinian-type eruptions is rich in water vapour, other gases and pumice.”

Thomas G.E., Olivero J.J., Jensen E.J., Schroeder W., and Toon O.B. (1989), Nature:

Page 490: “In view of this, it is remarkable that, despite the fact that observations of NLCs have been made almost every year since their discovery in

1885, none were observed before this date, notwithstanding the attention of a number of skilled observers, located at appropriate latitudes, who were sufficiently familiar with twilight phenomena... Alternatively, however, the discovery of NLCs may have been prompted by the Krakatoa volcanic eruption two years before, which would have caused volcanic injection of water into the stratosphere, followed by slow upward transport to the summertime mesopause."

Page 491: "The first observation of NLCs was made by Backhouse on 8 June 1885 in Bad Kissingen, Germany...In fact, we believe that the very first sighting was probably not due to the slow emergence of NLCs above the sky background, but that the upper atmosphere probably received an added 'boost' of condensation nuclei and/or water vapour from the Krakatoa volcanic eruption in 1883. The two-year time delay is consistent with the time needed to transport material from the stratosphere to the upper mesosphere²⁸, and has been verified by recent model calculations¹⁰."

Page 492: "We conclude the following: (1) the first manifestation of the noctilucent cloud phenomenon occurred in the summer of 1885 in northern Europe; (2) before 1885 there was insufficient water vapour routinely available at the mesopause to form visible clouds; (3) the NLC sightings during the period 1885-1895 were probably made possible in part by the large stratospheric injection of water vapour that resulted from the Krakatoa eruption in 1883."

Thomas (1991), Review of Geophysics:

Page 555: "Fogle and Haurwitz pointed out that increase of of NLC activity followed the two great eruptions of the modern era, Krakatoa in 1883 and Mt. Agung in 1963. They argued that violent (plinian) eruptions, such as Krakatoa, should increase the upper level concentration of water because of the penetration to high altitude of the water-rich effluent."

Page 567-568: "We have recently suggested that noctilucent clouds made their appearance about a century ago, not as the discovery of an existing phenomenon but as an emergence of a previously undetectable cloud into sudden visibility [Thomas et al., 1989]. We postulated that the very brilliant NLC display of 1885 was a result of an injection of water vapor and dust from the Krakatoa volcanic eruption of 1883. Previous to that time, NLC would not have been routinely visible because the pre-1885 water vapor concentration was insufficient to produce visible cloud layers."

Thus, we do not believe in the unsupported speculation of Reviewer 1, but we trust in the cited fundamental works. We keep this issue unchanged in our paper.

21. Line 509: This discussion is confusing. You state that the Krakatoa plume reached 40-50 km altitude, which is near the stratopause. The next statement is that the troposphere - mesopause transport times should therefore be 6-8 years, but these ideas do not seem to be connected as you imply. Please clarify. You also seem to be mixing up transport from the troposphere to the mesopause (~5 years?) and from the stratosphere to the mesopause (~2 years?). The stratosphere - mesopause times were stated above as ~2 years (line 494), but now you say 6-8 or 5 years? Please review this discussion and make sure you have it correct.

Reviewer 1 has inattentively read this paragraph. We are correct in our

estimations of the transport times in this paragraph. Ones again, the total transport time from the troposphere to the mesopause (based partly on the transport time of 4-6 years for inert gases from the troposphere up to 30-55 km altitude and on the 2 year delay for water vapor from 40-50 km to the mesopause for the Krakatoa eruption) is about 6-8 years. The two years delay refers to the transport time within the MESOSPHERE and MESOPAUSE, as we have clearly indicated in the manuscript.

Please also read carefully the important information which we have provided below:

“The NLC maximum occurred 4 years after the great volcanic eruption in 1991. This time shift corresponds to our result of a 5-year time delay, taking into account the fact that the great Pinatubo eruption produced a high volcanic plume reaching 25 km altitude...”

22. Line 538: Looking at Figure 5a - 5b there are 3-4 K decreases in T during 2016-2020. This seems significant, and in fact you state above that this cooling had a large impact on S. Please revise this statement.

We have corrected this statement in the revised manuscript.

23. Line 541: the phrase “meaning that the summer mesopause at middle latitudes has become more wet” is redundant and can be deleted.

We have deleted the second part of this sentence.

24. Line 548: You can fairly state that the increase in volcanic activity is correlated with (or consistent with) the high H₂O, but you cannot prove that it explains it.

We agree with this comment and have corrected this sentence as suggested.

25. Line 557: This is generally acknowledged as being due to rising CH₄.

Yes, we are well aware of this atmospheric process. However we do not consider methane in our study and would like to avoid an additional discussion (speculation) about it.