Comment on acp-2021-68

Anonymous Referee #1

Referee comment on "Two- and three-dimensional structures of the descent of mesospheric trace constituents after the 2013 sudden stratospheric warming elevated stratopause event" by David E. Siskind et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-68-RC1, 2021
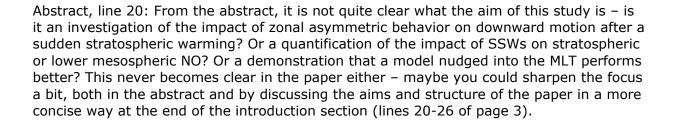

This paper investigates the downward propagation of NO after a sudden stratospheric warming / elevated stratopause event in early 2013 by analyzing results from WACCMX nudged into the MLT region compared to different observations and the NAVGM-HA data. The topic is of great relevance as sudden stratospheric warmings provide a highly variable and still not well understood source of NOx in the late-winter Northern hemisphere uppermost stratosphere and lower mesosphere. Unfortunately I found the paper rather unfocussed; it did not become entirely clear to me what the main scope and focus of the paper is. To evaluate the performance of a high-top model nudged by a high-altitude meteorological analysis compared to models nudged only up to the stratopause, in a particularly difficult dynamical situation? To analyze the dynamics of transport through the mesosphere after the SSW? To repudiate the idea that medium-energy electrons could be important for the energetic particle indirect effect? The second point appeared the most compelling and new to me as the analysis of longitudinal and latitudinal structures in NO and H2O after the warming as done in the paper provides new, highly relevant insights into downward propagation through the mesosphere in this dynamically disturbed situation. In particular it is shown that even in this model version nudged up to the mesopause, there are systematic differences in the downward transport through the mesosphere compared to the meteorological analysis which are related to (or expressed by) differences in the spatial distribution of areas of strong descent. Concerning the first point, it is shown that despite remaining differences, the WACCMX version nudged up to 90 km performs better than results from a previous model study nudging only up to the stratopause; this is hardly surprising, but important to point out. Concerning the third point, the evidence shown here is not entirely convincing to me. The authors argue that "in the absence of realistic meteorological forcing, one should be cautious about drawing firm conclusions about the role of medium-energy electrons", and I wholeheartedly agree with this statement; however, it can be turned around in the sense that "in the absence of a realistic representation of NO in the source region of the lower thermosphere, one should be cautious about drawing firm conclusions about the role of medium-energy electrons versus downward transport and mixing". In this sense, I recommend final

publication after revisions mainly to make the focus and main conclusions of the paper clearer and more robust. Suggestions and more specific concerns are listed below, as well as a list of minor comments (typos and such).

Abstract lines 8-9: this is only plausible if you assume that the sources of NO you included in your model for the MLT region are accurate. These presumably are photoionization and auroral electrons. However, from previous publications investigating MLT NO with the WACCM model, I would assume that this is not necessarily the case, as there appears to be evidence that the NO production by photoionization is too large (e.g., Siskind et al., 2019), while the parameterization for auroral electrons produces the NO peak at a rather high altitude (e.g., Smith-Jonsen et al., 2018). So it seems possible that the NO amount agrees reasonably well in a certain location and time due to a compensation of two antithetical error sources. Unfortunately the different NO formation mechanisms in the MLT – photoionization, auroral electrons, upper boundary condition -- in the model version used here are not described in the paper, so it is not possible for the reader to consider this adequately.

Abstract, line 10 – 13, "Despite the general success of WACCM in simulating mesospheric NO, …" this is a very positive way put it. A more critical assessment would be "Despite the general realistic temporal development of mesospheric NO in WACCM in the zonal averaged view, …" but this is also not quite true, as there appear to be significant differences in the downward motion in the lower mesosphere which are also observable in NO.

Abstract, line 16: differences in the GW forcing are certainly to blame for a lot of problems in modelling middle atmosphere dynamics. However: what is your statement based on that the differences are "small"? Small compared to what? Maybe just leave out the "small". Also: is it possible that differences in wave-wave interaction between planetary-scale and gravity waves play a role here as well? The distribution of NO as shown in Figure 5 seems reminiscent of a planetary wave 1 forming between February 15 and March 1, both in WACCM and NAVGEM, though with a slightly different tilt.

Abstract, line 20: From the abstract, it is not quite clear what the aim of this study is – is it an investigation of the impact of zonal asymmetric behavior on downward motion after a sudden stratospheric warming? Or a quantification of the impact of SSWs on stratospheric or lower mesospheric NO? Or a demonstration that a model nudged into the MLT performs better? This never becomes clear in the paper either – maybe you could sharpen the focus a bit, both in the abstract and by discussing the aims and structure of the paper in a more concise way at the end of the introduction section (lines 20-26 of page 3).

Page 2, line 6: you could refer to the last WMO report (scientific assessment of ozone depletion, 2018) here – the impact of EPP on stratospheric NO and ozone is discussed there in the "polar ozone" chapter.

Page 2, line 15: There are quite a few references you could cite here using SCIAMACHY NO data to investigate the impact of electron precipitation on the mesospheric NO budget, the most concise are probably Bender et al., ACP, 2019 and Sinnhuber et al., JGR, 2016.

Pages 3-4, description of WACCMX: I am missing important information here to understand the performance of NO and transport in WACCMX specifically in the MLT region. For example, what is the vertical resolution in the upper mesosphere and around the mesopause? Is it already one-fourth of the scale height? From which pressure level on? By which mechanism is NO formed – is the same parameterization for auroral ionization used as described in Smith-Jonsen et al 2018? Is the upper boundary condition for NO used (probably not for WACCMX)? Is the same parameterization for EUV photoionization rates used as described in Siskind et al., 2019 (based on Solomon and Qian 2005), or has there been an update? When were the model runs started, and what were they initialized with? This last point is mentioned later on ("H2O was initialized by a December climatology") but not really clarified – is the model run started on December 1 of the winter? Or mid-December, or end of December? Are model results output on satellite footprints? Please clarify these points in Section 2.1.

Page 5, line 5: is the model also sampled at the satellite overpasses, or is this strictly a zonal average at the latitudes of the observations? How are model data output – as zonal

averages, or as fields of the full model grid at some specific time(s) of day? Please clarify (possibly in Sec 2.1)

Page 5, line 7: "are transported downward …" in the absence of chemical loss, that is. Considering the lifetime of NO in the high-latitude winter this is probably a justified zero-order assumption (and you do discuss this point later on), but you should make a statement about it here.

Page 5, line 8: Looks like 0.2-0.3 hPa in SOFIE, 0.1-0.2 hPa in WACCM to me. However, it is quite difficult to read this from the contour figures. Why not provide profile plots for January 1 and February 15?

Page 5, line 8: please provide date and doy for each period you discuss here; that would make the comparison with the figure much easier to follow. "Middle of February" presumably is around doy 45?

Page 5, line 10: "could differ" erase the "could" – they do differ significantly.

Page 5, lines 14-15, "… it is unclear to what extent these higher altitude differences are relevant to the present study". Well as you do not include MEE, your NO presumably is formed by auroral electrons and EUV photoionization in these higher altitudes, and transported or mixed down to 0.01 hPa. That values there agree reasonably well with observations could argue a compensation of the too-low thermospheric values by more efficient mixing in the lower thermosphere. Your argument here appears to be that you only investigate the transport from 0.01 hPa into the upper stratosphere / lower mesosphere, and this is therefore not relevant. That is a fair point, but you should anyway

discuss this point in a bit more detail here, and make more clear that this does not mean that thermospheric NO production in WACCMX is generally well reproduced. One feature I am missing in the discussion here is the apparent MLT upwelling in early January around the SSW. This seems to be strongest around day 8-10 in SOFIE, around days 10-15 in WACCMX, and the strengths of the upwelling appears to be different as well. In SOFIE, the 200 ppb isoline moves up from 0.01 hPa on January 1 to 0.004 hPa on January 10; in WACCMX, the 200 ppb isoline moves up from 0.01 hPa on January 1 to 0.002 hPa on (probably) January 12. The 200 ppb isoline in WACCMX thus covers a larger vertical area in a shorter period of time to reach 0.1-0.2 hPa around day 40. So downward transport after the event throughout the upper and mid-mesosphere appears to be faster in WACCMX.

Page 5, lines 18-25 "Overall, the good agreement between calculated and observed NO at 0.1-0.2 hPa … Our results therefore suggest that for this specific period …, an additional odd nitrogen source … is not required. " As I pointed out above, if you look closely at NO in the source regions of the lower thermosphere, and at the temporal evolution of NO before it reaches the lower mesosphere, there are quite a few differences to the observations; too many to draw firm conclusions about the different sources of NO I think, even for early 2013. Ofcourse you can speculate about this, but the evidence does not appear compelling.

Page 6, lines 3 to 16, Figure 2: I'm not quite sure what the purpose of this comparison is in respect to the aims of the paper. If the main purpose is to compare against the Orsolini et al results, you should include their data in your figure. However, if the purpose is to show that WACCMX performs better than WACCM as used by Orsolini et al, you should include observations as a benchmark as well.

Page 6, line 27 "remains at lower pressure" of about 0.2 hPa.

Page 6, lines 27-28: "…have descended another scale height …" it is not quite clear what the reference here is – March 1 or February 15? The feature is now at 0.6 hPa in NAVGEM respectively 0.7 hPa SOFIE, at 0.4 hPa in WACCMX. This is less than a scale height (one

order of magnitude in pressure) even in SOFIE, certainly much less in WACCMX. Please clarify.

Section 4: This is really an interesting analysis.

Page 7, line 34, discussion of Figure 5: just a note – what I see on March 1, very clearly in NAVGEM, less pronounced in WACCM, is a planetary wave-1 structure with a zone of large-scale descent presumably related to the polar vortex displaced to Greenland.

Page 8, line 26: for better readability, and to ensure the reader can follow you without having to read lots of other papers, you could provide equation 4 of Siskind et al 2010 and equation 3.5.2c of Andrews et al 1987 here as well.

Page 9, lines 15-16, "A truly comprehensive examination of the causes of these differences is beyond the scope of the present study"; I accept that it is unlikely that you will clear this question within this study, but it seems to me that you could go one step further in evaluating your statement that the reason for the differences are more likely due to the representation of GWs than due to planetary-scale waves. E.g., you could test whether the planetary-scale waves really are represented consistently in WACCMX and NAVGEM. Figure 6 (lower right panel) seems to confirm this assumption, but this is only one latitude; e.g., you could easily calculate amplitudes and phases of planetary waves 1, 2, … for this date for a wider latitude range.

Page 10, line 7 "shows good agreement" …I wanted to suggest to compare mean/median peak values and peak altitudes, however considering the strong longitudinal variation as shown in Figure 9 this is probably not very meaningful. Maybe you could clarify somehow

that it is a qualitative agreement of peak values and altitudes in those profiles which show enhanced values.

Page 10, lines 13-14: actually the peak of SOFIE and ACE as shown in Figure 8 seems to lie around 0.3 hPa (ACE) respectively between 0.3 hPa and 0.4 hPa (SOFIE), definitely not below 0.4 hPa.

Page 10, lines 26-27: As the relationship between equivalent latitudes and enhanced NO / low H_2O emphasizes downwelling of upper mesosphere air in the polar vortex, it is quite puzzling that this appears to be better reproduced at the higher altitudes than at the lower altitudes – I would assume the vortex to be better represented lower down. Question – equivalent latitudes here have been calculated from NAVGEM data? Could you calculate those from WACCMX? That is, is it possible that the vortex itself is shifted in WACCMX compared to NAVGEM? Or is it larger, or is the edge more diffuse?

Page 12, Lines 28 and following, derivation of "geometric estimate" based on SOFIE and ACE: the procedere here is not entirely clear from your description. As I understood it, you just draw a line by hand which provides an approximate average of ACE and SOFIE NO values and equivalent latitude coverage extrapolated to 90° in this pressure level (so assuming homogeneous descent in the vortex), and then integrate this line. This appears to be rather imprecise considering the large differences, e.g., between SOFIE and ACE, and your description of non-isotropic descent above. As a first-order approximation it might be justified to do that. However, you could make more clear the limitations of this approach, and derive an error range based on the observed variability of NO as well as by excluding the high-latitude area not covered by data.

Page 12, lines 34-35: … do not spread to such low equivalent latitudes as suggested by the model … I would say that the relationship between equivalent latitudes and enhanced NO values in the model is not as clear as in the observations; which again raises the question whether the vortex in WACCMX was formed somehow differently than in reality, see my comment above (Page 10, lines 26-27)

Page 13, lines 22-23: "However, the global totals are quite close. Certainly the immediate conclusion one draws is …." I don't really follow your conclusion here; if you want to make a bold statement like this, you would have to a) derive a robust error range of the observational (geometric) estimate, and b) show that NO agrees well in the source regions of the lower thermosphere and in the temporal behavior from before the event onset to the lower mesosphere. Considering b), there are problems with both cases as discussed above in my comments to page 5. Your second sentence "Certainly the immediate conclusion is that it is hard to argue …." is also formulated in a rather indirect way; that could be put clearer. Why not just state "We conclude that WACCMX/NAVGEM-HA represents the NOx descent to the lower mesosphere reasonably well after this event, and no additional source of NOx is needed to reproduce the total amount of EPP NOx during this time."

Page 13, line 26: see my comment to gravity waves versus planetary wave forcing (page 9, lines 15-16)

Page 14, line 7-10: See my comments to page 5 and to page 13, lines 22-23; I don't really follow this conclusion; I think if you want to draw a robust conclusion on the MEE versus transport issue, you have to show that NO in the source regions in the lower thermosphere is well reproduced. I agree with your last sentence, that "in the absence of realistic meteorological forcing, one should be cautious about drawing firm conclusions about the role of medium-energy electrons "; but this sentence can be turned round as well to "in the absence of a realistic representation of NO in the source region of the lower thermosphere, one should be cautious about drawing firm conclusions about the role of medium-energy electrons versus downward transport and mixing".

Minor comments regarding typos and such

Page 2, line 22: Full stop missing.

Page 2, line 23: the best comparison for this is provided in my opinion in the full MIPAS NOy timeseries as shown in Funke et al., 2014

Page 3, line 16: double full stop.

Page 4, line 31: "A similar" not "As similar"

Page 5, line 4: please format "1E-5".

Page 5, line 11: "proior"?

Page 5, line 28: "much less" better "much smaller"?

Page 5, line 32: "much less" better "much weaker"?

Page 10, line 15: "too low"

Page 10, line 15: please format "1e-14"

Page 12, line 10 "… that could to used …." Should be "… that could be used …."

Page 12, line 33: "… can be compared the WACCMX data …." Should be "… can be compared to the WACCMX data …"

Page 13, lines 19-20 just strike out "as discussed above, the difference is immaterial"

Figure 11: Note x-axis labels of the lower panels are overlapping into the lower panels. Also the panels on the right-hand side seem to be shifted vertically compared to the left-hand panels. Please correct.

Table 1: Please clarify in the caption of Table 1 that "observed" refers to the last column "geom. est.", and that this is an estimate which carries a large uncertainty.