This paper examines the descent of nitric oxide (NO) and water vapour in the northern high latitudes during the stratospheric warming (SSW) of 2013, using a version of WACCM-X nudged to the high-altitude NAVGEM analyses, extending to the mesopause. Results are compared to older simulations with WACCM driven by the MERRA reanalyses extending only to the stratopause region.

The paper shows that when constrained by reanalyses extending higher, the model better reproduces the evolution of NO seen in satellite observations, if not in all the details at least in the mean total transport into the stratosphere.

The paper is relatively clear and well-written, although it is a bit long. It is worthy of publication in ACP after a couple of major comments and minor comments are properly addressed.

We thank the reviewer for their comments. The first part of the paper has been substantively rewritten as a response. A number of new citations have been added as we discuss below. Also, and importantly, a new figure (Figure 2) is added to track more clearly how the NO from the upper mesosphere ends up in the lower mesosphere. Based upon this we conclude that the tongue of descending NO is peeled off the bottomside of the NO layer that sits above 80-85 km and that "upper mesospheric" rather than "MLT" more properly describes that. This is consistent with similar conclusions reached by Randall et al., (2001) and that citation is now added. More specifics on this issue are offered below.

Note, as part of our overall response, there are a number of new references that we have added. For ease of reference, these are listed below; our specific responses follow this list.

1. Duderstadt, K. A., C.-L. Huang, Spence, H.E., Smith, S., Blake, J. B., Crew, A. B., Johnson,A. T., Klumpar, D. M., Marsh, D. R., Sample, J. G., Shumko, M., Vitt, F. M., Estimating the impacts of radiation belt electrons on atmospheric chemistry using FIREBIRD II and Van Allen Probes observations, J. Geophys. Res., https://doi.org/10.1029/2020JD033098, 2021.

2. Funke, B., Lopez-Puertas, M., Holt, L., Randall, C. E., Stiller, G. P. and von Clarmann, T., Hemispheric distributions and interannual variability of NO$_y$ produced by energetic particle precipation in 2002-2012, J. Geophys. Res., 119, 13,565-13,582., doi:10.1002/2014JD022423.

3. Harvey, V.L., Randall, C. E., Hitchman, M. H. Breakdown of potential vorticity-based equivalent latitude as a vortex-centered coordinate in the polar winter mesosphere, J. Geophys. Res., 114, D22015, doi:10.1029/2009JD012681.

4. Hendrickx, K, Megner, L., Marsh, D. R., and Smith-Johnson, C., Production and transport mechanisms of NO in the polar upper mesosphere and lower thermosphere in observations and models, Atmos. Chem. Phys., 18, 9075-9089, 10.5149/acp-18-09750-2018, 2018

5. Perot, L., and Y. J. Orsolini, Impact of the major SSWs of February 2018 and January 2019 on the middle atmospheric nitric oxide abundance, J. Atm. Solar. Terr. Phys, 2021, in press.

6. Randall, C. E., Siskind, D. E., Bevilacqua, R. M., Stratospheric NOx enhancementsin the southern hemisphere vortex in winter/spring of 2000, Geophys. Res. Lett., 28, 2385-2388, 2001.

7. Sinnhuber, M., Friedrich, F., Bender., S. The response of mesospheric NO to geomagnetic forcing in 2002-2012 as seen by SCIAMACHY, J. Geophys. Res., 121, 3603-3620, doi:10.1002/2015JA022284, 2016.

Response to Major comments

1) I wonder the role of downward transport from the NO main reservoir into the mesosphere near 90-100 km that is glimpsed from the observations and whether the model captures it. Orsolini et al. (2017) or Limpasuvan et al (2016) showed a short-duration downward transport, diagnosed in the TEM formalism, driven by transient planetary wave activity following the onset. That westward planetary wave forcing driving the downward motion was able to overwhelm the eastward gravity wave drag. That 2017-paper used WACCM nudged to MERRA and I wonder if there is a similar w* signature in the simulations discussed in this paper. The implication is that this descent might still be underestimated in the model simulations, in a region where the constraint from NAVGEM is relaxed.

It would hence be of interest to see w* higher than 0.01 hPa in Fig 7 (like in Fig 1), from the time of onset and onwards. The authors argue that what happens at these higher altitudes does not influence the descent lower down, based on examination of Fig 1. However, it seems that SOFIE indicates higher values of NO than WACCM-X at 0.001 hPa and high mixing ratios do seem to migrate downwards (Fig 1, in second part of January). I understand that there might be a bias due to initialisation, but it might be possible to rescale values to track the rate of descent. It may also be that the descent is confined in longitudinal sectors, constrained by the presence of the planetary waves, and not entirely captured by a zonal-mean like Fig 1, as the authors have also diagnosed during other events in other publications. In other words, can it be confirmed that a strong transient downward transport near 90-100km is not occurring during this 2013 event shortly after the onset in the simulation or, if it occurs, that it does not impact NO much lower down?

As we now discuss in the context of the new Figure 2, the tongue of the descending NO that is the focus of the paper does not come from 0.001 hPa. We do acknowledge in the text (page 6) that there are differences between WACCMX and SOFIE at the higher altitudes, but it is our assessment that those are not relevant to the present study. We did look at specific longitudes to confirm this (not shown). Certainly the large reservoir of NO that persistently resides in the 85-100 km region could well be considered “lower thermospheric”; however, the details are beyond the scope of the present study. We did add a citation to Hendrickx et al. (2018) who looked into some of this. As a consequence, subsequent to the discussion of Figure 2, we replaced all mention of “lower thermosphere” or “MLT” in favor of “upper mesosphere” to more rigorously describe the NO we are studying (cf. throughout Section 5).

2) Some more details about how the equivalent latitude is calculated would be useful and would fit in Section 2. Is the PV from WACCM-X used or from NAVGEM, and do you use spatial filtering first to remove small-scale PV anomalies (linked to GW drags) which are common at these altitudes? Discussion added in Section 5 with citation added to Harvey et al (2009).

3) Some climatological validation of WACCM-X-NAVGEM NO climatology against SOFIE at the top level shown in Fig 1, near 0.001 hPa, based on several years of available simulations and observations, would be highly valuable to support with confidence that the WACCM-X distribution of NO is realistic at the top of the mesosphere. Given the dependence upon geomagnetic activity and EPP, I realize that enough simulated years might not be available.

As noted above, we have concluded that those differences have no immediate consequence or relevance for the tongue of descending NO that is the subject of the paper.

Minor comments

P2, line 17: You could add Odin/SMR to the list of satellite instruments, since data from SMR is referred to later in the paper, when describing the descent of NO during SSWs. In fact, Odin/SMR has also been used to describe the descent of dry air through the mesosphere during SSWs. done

P2, line 31: In fact, the 2018 event was not an elevated stratopause event, but the 2019 event was. Corrected- we removed the Wang et al., reference and added Perot and Orsolini’s recent paper

P3, line 33: Some details about the initialisation would be helpful: how is the model initialised at levels above where the NAVGEM data is used; initialisation of chemical species (in particular NO and H_2O but also other species) could be addressed in this section (it is referred to only later in the paper). Text added in Section 2.1

P7, line 12 : the sentence is a bit confusing since the H_2O meridional gradient is negative, with drier air at the pole. Sorry, no- in WACCM the lowest value of H_2O is sub-polar. From 70 to the pole, the air gets wetter- a positive meridional gradient.

P9, line 7: the sentence is a bit unclear: “the 0.1-0.2 hpa equatorward flow is seen in NAVGEM-HA moving downward…”. Hopefully clarified “at 0.1-0.2 hpa, equatorward flow is seen…”

P9, line 26: a “zero wind line” might not be the most appropriate term. What is meant is a line where w* goes to zero. This expression is mostly used in connection with zonal wind, and w* is not an actual wind. “encounters a level where w*goes to zero…” or something like that (?)

Yes, corrected as suggested (both at the beginning of section 5 and a couple of places at the end of Section 4)- it now reads “zero isoline” or “zero line in the descent” or “where w* goes to zero”

P10, line 12: measure at the same latitude or sample the same latitude (not are at the same latitude). Corrected: “sampling”

P13, line 30: A word about the potential causes of the stronger equatorward dispersion of NO in WACCM-X would be useful. We added reference to Figure 8. Also “spreads” was not the best word- is “advected” since that is what the TEM circulation characterizes.

Figure 4 is missing a color scale. Added

Figure 7: could a streamfunction help visualization of the different circulation cells ? Perhaps; hopefully some of our clarifications make it easier to follow.

Typos/English

P4, line 4 : Sassi (2020) Woops- sorry, there were supposed to be two Sassi references, one to a 2018 paper and one to a 2021 paper. Hopefully now clarified/corrected.

P4, line 21: short term corrected

P4, line 31: A similar figure Corrected

P5, line 8 : check consistency about use of hPa and hpa (also other places in the paper) OK

P7, line 2: use daily rather than diurnally to be consistent with captions Done.

P10, line 15: too low Corrected

P13, line 20: is not present (?) “does not present itself”. Corrected.