

Atmos. Chem. Phys. Discuss., referee comment RC2  
<https://doi.org/10.5194/acp-2021-68-RC2>, 2021  
© Author(s) 2021. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## Comment on acp-2021-68

Anonymous Referee #2

---

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-RC2>, 2021

---

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.

### 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?

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?

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 realise that enough simulated years might not be available.

#### 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.

P2, line 31: In fact, the 2018 event was not an elevated stratopause event, but the 2019 event was.

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<sub>2</sub>O but also other species) could be addressed in this section (it is referred to only later in the paper).

P7, line 12 : the sentence is a bit confusing since the H2O meridional gradient is negative, with drier air at the pole.

P9, line 7: the sentence is a bit unclear: "the 0.1-0.2 hpa equatorward flow is seen in NAVGEM-HA moving downward...".

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 (?)

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

P13, line 30: A word about the potential causes of the stronger equatorward dispersion of NO in WACCM-X would be useful.

Figure 4 is missing a color scale

Figure 7: could a streamfunction help visualization of the different circulation cells ?

Typos/English

P4, line 4 : Sassi (2020)

P4, line 21: short term

P4, line 31: A similar figure

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

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

P10, line 15: too low

P13, line 20: is not present (?)