Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-1188-RC1, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Production and transport mechanisms of NO in observations and models" *by* Koen Hendrickx et al.

Anonymous Referee #2

Received and published: 26 February 2018

General:

The authors presents a detailed comparison of NO in the Antarctic MLT region between observations and simulations. They use SOFIE and WACCM data to study the magnitude of the NO reservoir, dependence on solar and geomagnetic drivers, and the descent through the mesopause. Similarities and differences and the reasons behind them are noted and discussed, and suggestions are given for further improvement and work.

Overall, the paper reads very well, the figures are clear except for some difficulty in separating black and dark green lines. The methods are sound and well described, the results and conclusions are supported by the data. The topic is clearly in the scope of ACP. Recommendation: publish after my concerns below are addressed.

One concern: altitude resolution and unit conversions. When describing the model and simulations, the authors mention that they interpolate the data to 2-km altitude grid. I think the WACCM grid is coarser than that in the MLT, so should not the observations be interpolated to the WACCM grid? Also, WACCM operates in pressure levels rather than altitudes. How was this conversion made? Also WACCM provides mixing ratios, but some results are shown as NO concentrations. How was this conversion made? The authors should provide some more details.

Another concern: differences in polar vortex dynamics. Since the polar NO is very much dependent on the polar vortex, I wonder what kind of differences are there between the reality and its representation in WACCM. SOFIE observations, as solar occultations, are very restricted in latitude. Thus sampling WACCM at the measurement locations could introduce artefacts if there is a SOFIE-WACCM difference in the shape or size of the vortex. Have the authors considered this possibility? I think that the problem, if any, could be largest during solstice times when lower latitudes are covered.

Specific:

The title is very general. Add: in the polar mesosphere-lower thermosphere. Maybe add SOFIE and WACCM. Maybe the years too.

Page 1, line 11. Maybe: altitude of peak density

Page 1, line 12. multiple linear regression

Page 5, line 23-27. Why are the observations giving different altitudes of maximum NO in the past and now? Is it due to instruments improving (e.g. better resolution) or the maximum altitude really changing? If the latter, then why the change?

Page 6, equation (1). Is AE the correct geomagnetic index to use? Why? In WACCM, auroral precipitation is driven by the Kp index, shouldn't that be used for the model at least? Is it possible that differences between Kp and AE could introduce an artefact?

Page 9, line 4-5. The relative increases given in the text are not presented in Fig-

ure 10, instead absolute values are shown. To me, the maximum absolute increases seem rather similar, so the difference in relative change is due to differences in the background?

Page 10, line 25. The N(2D)/N(4S) ratio is important, but I think it is in perhaps emphasised too much in general. There are other important factors, such as temperature and atomic oxygen. Model deficiencies in these could play a big role.

Page 12, line 29. "not parameterised chemistry in the D-region". Suggestion: excluded D-region ion chemistry. Or: too simplified parameterisation of D-region ion chemistry.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2017-1188, 2018.