Reply to Anonymous Referee #2:

We thank the reviewer for taking the time to read the manuscript and provide critical and valuable feedback.

Overall, the paper reads very well, the figures are clear except for some difficulty in separating black and dark green lines.
We have changed the green colour to red for improved clarity.

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.
The WACCM grid becomes progressively coarser near the model top and pressure levels are the fixed, native grid WACCM operates on. A conversion to geometric altitude is made using the geopotential height parameter, output by the model. The resulting profile is not fixed in altitude as it depends on latitude and therefore needs to be interpolated onto a fixed grid. Because SOFIE has a fixed vertical resolution of approximately 2 km, we decided to use this grid.
Conversion to number density is done using the ideal gas law. More information and equations have been added the text.

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.
We thank the reviewer for pointing to this possibility. WACCM outputs data on geolocations as close as possible to SOFIE measurements and model data should ideally be as similar to the observations. We have not considered polar vortex locations from SOFIE data. Other research into MLT descent using satellite observations that are not restricted to solar occultations also observe discrepancies with the simulated descending NOx flux. Furthermore, the inferred descent rate in our study in the 80-110 km region is remarkably similar in WACCM and SOFIE, leading us to believe that the dynamics are well represented in the model. This has been discussed in the new version of the manuscript.

The title is very general. Add: in the polar mesosphere-lower thermosphere. Maybe add SOFIE and WACCM. Maybe the years too.
We have changed the title into ‘Production and transport mechanisms of NO in the polar upper mesosphere and lower thermosphere in observations and models’ and added the covered years in the abstract.

Page 1, line 11. Maybe: altitude of peak density
We have changed this to ‘altitude of peak NO density’.

Page 1, line 12. multiple linear regression
It is changed.

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?
We have checked the SOFIE data and do not find any strong change in the altitude of maximum NO throughout the 2007-2015 period. We are therefore inclined to believe this changing altitude is due to increased resolution of the observations, though cannot rule out a change due to physical variability.

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?
The AE index is the physically more correct index to use as it represent particle precipitation over the polar regions and is better related with NO variability (Hendrickx et al., 2015). WACCM uses the Kp index and when performing the MLR with Kp on both WACCM and SOFIE, a difference, similar as when using the AE index, remains between the datasets. Because the AE index is physically more correct, we decided to use the AE index rather than the Kp index.

Page 9, line 4-5. The relative increases given in the text are not presented in Figure 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?
The colour bar in Figure 10 is on a logarithmic scale, so even though the increases seem rather similar, a small change in colour can be a large difference and that is why we additionally give the percentage increase. This relative increase is a combination of a high NO background and a too low variation with geomagnetic activity in WACCM.

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.
We have changed the text in this paragraph as to not emphasise this ratio so strongly.

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.
We have implemented the suggestion.