Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-39-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "An empirical model of nitric oxide in the upper mesosphere and lower thermosphere based on 12 years of Odin-SMR measurements" *by* Joonas Kiviranta et al.

Anonymous Referee #2

Received and published: 4 March 2018

The authors present a new model of lower thermospheric NO, based on observations by the SMR instrument. The purpose is to provide the scientific community with a new, improved proxy for thermosphere NO and the authors throughly compare their regression model to most available observations since 1998 (for some reason HALOE has been left out of the comparison). The model presented in the paper is potentially very useful for thermosphere upper boundary conditions in high top models, but I have concerns on the presentation of how the model was built which I will explain below. In my opinion these clarifications are vital for others to be able to 1) evaluate and 2) apply this model, and thus I am recommending a major revision so the authors can clarify these points. Clearly the model compares nicely with independent observations, but

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these clarifications are still needed. If we are to use the model (and I think many people will be interested!), we need to know what went in. Particularly as the approach is so different from other linear regression models in the literature. My comments below may sound critical, but remember that the purpose is to clarify your work so that we can use it with confidence.

How was is the regression model built? We have the equation:

NO(lamda, h, t) = Kp(t)*c1 + dec(t)*c2 + log(F10.7(t))*c3 + com1(Kp(t),dec(t))*c4 + com2(Kp(t),dec(t))*c5 + C

Firstly it is not clear how the authors formed com1 and com2. The paper says that they were a result of iteration, but there is no physical explanation for them or clear explanation of the iteration process. They do, however, introduce autocorrelation with all except term #3. I am worried that there are two terms which both have a linear (lagged) Kp term as well as linear/sin terms depending on solar declination in addition to the linear terms in #1 and #2 - without any explanation.

The final number of terms in #4 and #5 also depend on solar declination in a way that is not explained clearly. Why for summer solstice conditions are there a maximum number of lagged days (11)? Would it not be natural to assume a lag is needed for winter conditions when the lifetime of NO is larger and thus there might be build-up? Why was the Kp lag not built into the first term of the regression (and same for solar declination)? What is the physical meaning of having these extra Kp & dec dependent terms?

The actual regression coefficients "c1, c2,. . .c5" are not given (First 3 were plotted in Figure 8, I didn't notice remarks on c4 and c5) and thus the equation can not be used. Please note that many readers will not have access to Matlab statistics toolbox used for the analysis.

Other comments: Figure 2: This is plot of the F10.7 time series, clearly taken directly from the NOAA website. Although the reference to the website is given, please plot the data yourself. This is a very simple figure with the actual monthly means and a running mean. The future prediction (red line) is not necessary for this paper.

The indices you use here are not "space weather" indices, but rather geomagnetic and solar indices. The term space weather has a very specific meaning relating to impacts on technology (and these indices can be used for those), but as we are looking at impacts on the Earth's atmosphere we should talk about geomagnetic and solar indices.

The AE index (Auroral Electrojet) is mentioned in the same section, but this is not used anywhere after that, only Kp is mentioned.

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