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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Mesospheric nitric oxide model from SCIAMACHY data" *by* Stefan Bender et al.

Anonymous Referee #2

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General comments

In this paper, Bender et al. describe the empirical model for NO that they have developed, based on SCIAMACHY limb measurements of mesospheric NO. This model relates the daily zonal means of NO number densities to the Lyman-α and AE indices, as proxies for the solar irradiance and geomagnetic activity, respectively.

Such a model is very useful, both to provide an estimate of the NO concentrations and to indicate regions where certain processes dominate. It is a good complement to other existing empirical models for NO in the MLT. These models can be used to validate or constrain atmospheric models, or as a tool to help compare different observational data sets with each other.

I recommend the publication of this article in ACP after consideration of the few revisions suggested below. My only significant comment is about the way the non-linear model is defined. The methodological choices could be described in a more accurate way. The comparison with the linear version of the model, as it is presented in the paper, is not convincing enough.

Specific comments

p.1, l.18-20: Variations in solar and geomagnetic activity do not affect the atmosphere only above 100 km. This is what one can understand when reading these few lines. Please reword this paragraph to make it clearer.

p.2, l.4-5: SSW events do not always result in a strong downward transport of air from the mesosphere to the upper stratosphere. The formation of an elevated stratopause is generally needed for that to be observed.

p.2, l.10: Odin/SMR NO measurements are actually available from 35 to 115 km (Pérot et al., 2014). As explained by Kiviranta et al. (2018), only a part of the available altitude range has been used to develop SANOMA model.

p.3, l.11: NO is produced by particle precipitation at auroral latitudes under sunlit conditions too.

p.3, l.14-15 and l.19: There is a mistake in the dates of the SNOE mission. According to Marsh et al. (2004), the instrument was operational for only two years, from 1998 to 2000.

p.4, l.22-23 and l.30: The Lyman-α index is a proxy for solar irradiance and the AE index is a proxy for geomagnetic activity, but both exhibit long- and short-term variations. Please reword your description of the proxies.

p.4-5, Sec. 2.2: Please indicate the source for the proxy data used in your study.

p.5, l.1: I agree with referee 1 about the fact that "questioned" seems too strong. Hendrickx et al. (I guess that you meant 2015, and not 2017) showed that the AE index correlates better with NO concentrations measured by SOFIE. One cannot draw a conclusion from one study based on one instrument. As mentioned in Kiviranta et al. (2018), the Kp index correlates better than the AE index with Odin/SMR NO observations.

p.5, l.25: "We then omit the harmonic parts in the model." Why? Please explain this choice.

p.9, l.10-11: "At high southern and low latitudes, the improvement over the linear model is less evident." How do you explain that the improvement is clearer in the NH than in the SH?

Fig. 1-2 - following my previous comment: For high southern and low latitudes, it is difficult to see any clear difference between the linear and non-linear models in the residuals but, looking at the upper panels, it actually seems that the linear model reproduce better the seasonal variations of the data. That would mean that the non-linear model is not better in all regions. That could be due to the fact that, in your non-linear version of the model, you do not take into account seasonal variations which are not related to EPP. Please comment about that.

p.10, l.1-2: How do you explain the observed decrease in the Lyman-α parameter distribution between 80 and 90 km?

p.10, l.2-3: "The Lyman-α coefficients are all negative below 65 km." Please mention that the coefficients are negative at high northern latitudes too.

p.11, l.5: "The amplitude also increases with decreasing altitude." This is not always true. At high northern latitudes, the amplitude actually decreases with decreasing altitude between 75 and 90 km. Please make the description of this figure more accurate and comment about this observed distribution (in Sec. 5.4).

p.12, l.16-17: "We observe negative Lyman-α coefficients [...] at high northern latitudes above 80 km." How do you explain that such a pattern is observed only in the NH?

p.13, l.2: "Production rate" I agree with referee 1 on this point. AE coefficients do not represent the NO production rate, but rather the NO response to changes in the AE index.

p.13, l.17-18: "the increasing photochemical lifetime at low solar zenith angles." This sentence is unclear. According to Sinnhuber et al. (2016, Fig.7b), the photochemical lifetime of NO is lower for low solar zenith angles than for high SZAs. Did you mean "the increasing photochemical lifetime with decreasing altitude, at low SZAs"? In any case, this does not explain why the annual variation of your lifetime parameter increases from 75 to 90 km in the highest northern latitude bin.

Technical corrections

Fig.1 and 2: These figures are not easy to read, especially because the dots representing the data are hidden by the error bars. Maybe you could represent the error bars in a different way (other colour for example) in order to make the plots clearer.

p.9, l.17-19: This sentence is unclear (too long). Please reword.

p.18, l.1: "Versick, S" has been written twice.

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