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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 #1

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This paper uses Odin SMR NO observations taken between 2004 and 2016 in the upper mesosphere and lower thermosphere to build an empirical model of NO number densities in the altitude range 85 -115 km. The model is compared to the previous NOEM empirical model which builds on SNOE NO observations taken between 1998 and 2000,

The presented empirical model is very useful for constraining or validating atmospheric models and improves upon previous empirical models by considering a longer observational period that covers the entire solar cycle. It is also very useful as transfer function allowing for indirect intercomparison of thermospheric NO observations with disparate

temporal sampling. Such indirect intercomparisons of the SANOMA model (and hence SMR) to satellite observations from ACE, MIPAS, SCIAMACHY and SOFIE are also presented.

The paper is well written and I have only minor comments specified below:

p2 l26: "Additionally, the amount of NO influences the thermal balance of the MLT, especially at ultra-vialett wavelengths (Richards et al., 1982)". The cited paper discusses the role of NO in modulating the thermospheric heating efficiency by infrared 5.3 um cooling (not at ultra-violet wavelengths).

p3 l25: "since night-time NO is expected to be higher than day-time NO". It should be mentioned that the day-night ratio of NO depends on latitude and altitude (see, e.g. Bermejo-Pantaleon et al., 2011)

p3 l27: typo: based -> based on

p4 l25: "SANOMA will express NO in number density to accommodate chemical models" Why is number density over altitude better suited to accommodate chemical models than vmr over pressure level?

p8 l 6: the PC's mentioned here are not included in Eq. 1. It would be clearer to note that the PCs were replaced by polynomial fits to the geophysical parameters Kp, delta, log(F10.7) in Marsh et al., 2004, represented in Eq 1 by f1,f2, and f3.

p10 l9: Have auto-correlations been considered in the regression? This would be important since explained variances might be overestimated otherwise (see, e.g. Hendrickx et al., GRL, 44, 2017)

p 11 l11: "R-sqared" coefficient of determination

p11 l8: Why aren't the mean differences between SANOMA and SMR zero in a given bin? This would be expected from a simple multilinear regression.

p13 l10: "SANOMA includes no dynamics". Since SANOMA is an empirical model it does not include any physical process.

p13 l11: "This could also be due to a lower signal-to-noise ratio in the measurements at lower altitudes." Isn't the measurement error already accounted for in Eq 5?

p14 Table 1: Isn't ACE measuring at altitudes < 70 km as well?

p14 Table 1: Same for MIPAS. Also, the MIPAS latitude range should be 90S - 90N.

p 15 Fig 12: I'm puzzled about the NOEM results, showing minimum polar summer NO concentrations in 2012-2014, i.e., well after the solar cycle minimum (2009-2010). How is this possible if NOEM uses F10.7 and Kp (both having lower values in solar min)?

p18 l5 : The given reference is correct for MIPAS but for the NO data version mentioned here Bermejo-Pantaleon et al, 2011 would be more adequate. It is unfortunate that this study uses data version v5r 620 since it only covers the years 2010-2012. The newer version v5r 622 (available, for instance, at http://mesospheo.fmi.fi/data_service.html) covers the entire period 2005-2012.

p19 l2: "with a minimum of -60% at 115 km around 70 deg". It looks more like a minimum of -30 to -40% at 115 km in Fig. 22.

p21 Table 2: caption: "the mean of the mean percent difference". Do you mean the mean percent difference averaged over the respective latitude bands? Further: "between the various instruments and a) NOEM, b) SMRNOEM, and c) SANOMA". Shouldn't it be the other way round, i.e., model - instrument?

p21 l4: Since SMR measures both day and night time NO, a positive difference compared to the daytime measuring instrument SCIAMACHY was expected, but SANOMA shows a positive difference in comparison to SOFIE and ACE as well". SOFIE and ACE are solar occultation instruments. A diurnal sampling bias can therefore no be excluded when comparing to instruments that measure at both day and nighttime (like SMR or MIPAS).

p21 l12-15: I wouldn't say that MIPAS measurements should be treated with care, particularly because MIPAS shows small absolute difference with respect to SANOMA (and hence SMR), as well as high R-squared values (Table 1). It is likely that the apparently different behavior of MIPAS compared to other datasets is related to the shorth time coverage of the MIPAS data version used here (2010-2012). It would be very interesting to check this by using the newer version v5r 622 (2005-2012).

p22 l5-7: see above.

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