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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Spatio-temporal variations of HNO_3 total column from 9 years of IASI measurements – A driver study" *by* Gaétane Ronsmans et al.

S. Solomon (Referee)

solos@mit.edu

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Review of the paper by Ronsmans et al.

General comment: This paper presents and analyzes 9 years of total HNO3 column abundances from IASI measurements. The paper does a great job of presenting the data and discussing its key features. It will be a valuable resource for other researchers and merits publication. I do have a few comments and suggestions that I hope the authors will consider, presented below.

1) There is a lot of discussion of the apparent onset of denitrification and its association

with a 'threshold' T<195K. I think the paper would be stronger if this were accompanied by a discussion of the uncertainties in the temperatures used to identify this relationship (i.e., ERA-interim). Three questions arise a) Could it really be 194K within uncertainties? Why or why not? How sharp is this threshold within uncertainties? b) what about the influence of small scale waves that may not be resolved by the reanalysis? c) would there be value in simply doing a scatter plot of local HNO3 versus local temperature in winter, as opposed to the current approach of binning by eqlat?

2) Figures, 2, 3, and 4 are key results of this study, showing very well the seasonal cycles in the two hemispheres. A minor comment on Fig 2 is that heavy tics are needed for January, so one can see the exact mapping in time more readily. A substantive comment is that the time lag for recovery of the HNO3 column in the southern hemisphere is striking. The paper has some good discussion on this but I wonder if more could be done. In particular, if the mechanism replacing the HNO3 is mainly via transport, then HNO3 and O3 would show very similar post-final warming increases – do they? This could be shown, and would make the paper more useful. Further, I suspect that the very late recovery of HNO3 may have more to do with chemistry, in particular the fact that perpetual sunlight means NO3 is photolyzed effectively throughout the polar summer so that little of it can end up in N2O5 and hence to HNO3. This may do a better job explaining the ramp from Mar-May (e.g., in Fig 4) than other explanations, and could be probed fairly simply using chemical kinetic equations considering length of night, temperature, and ozone near 20-25 km. Zonal means should suffice, despite the potential for vortex meandering, which is likely to be small in summer.

3) The authors often (not always, but often) refer to low HNO3 columns as synonymous with denitrification, but what about, for example, high ClONO2 columns associated with the collar implying that NOy may be in that form at times? Similarly, the low HNO3 during mid-summer seen in Figure 4 and discussed above may have to do in part with perpetual sunlight meaning high NO and NO2 amounts, with less in HNO3 but not necessarily less total NOy. Some further discussion is merited.

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