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Comment on egusphere-2022-1309

Anonymous Referee #3

Referee comment on "Tropospheric NO_2 vertical profiles over South Korea and their relation to oxidant chemistry: implications for geostationary satellite retrievals and the observation of NO_2 diurnal variation from space" by Laura Hyesung Yang et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1309-RC3, 2023

In this manuscript, the authors report on a study comparing air mass factors for GEMS tropospheric NO2 satellite retrievals over Korea based on measured and modelled NO2 profiles. The measurements are 63 vertical NO2 profiles from the KORUS-AQ campaign, while the modelled profiles are from a dedicated version of the GEOS-Chem model. A particular emphasis is on the evaluation of diurnal changes in the vertical NO2 profiles and thus the air mass factors. The main conclusions from the paper are that the NO2 AMFs over Seoul significantly increase over the course of the day and that the new version of the GEOS-Chem model predicts NO2 profiles, which lead to AMFs, which are in good agreement with those based on observed profiles.

The topic of the manuscript is relevant for the atmospheric community, as the diurnal variability of the AMF needs to be understood and modelled well in order to make use of the exciting new observations from geostationary orbit, of which the Korean GEMS instrument is the first to be in operation. The manuscript is overall well written and contains clear figures illustrating the main findings. There is, however, a number of points, which the authors need to address before the manuscript can be accepted for publication.

General Comments

Throughout the manuscript, the wording is in my opinion not as accurate as it should be. There are many occasions where it is said that the "GEOS-Chem calculated AMFs are …" while I think the formulation should be "AMFs based on GEOS-Chem profiles …". Some more examples from the abstract and summary:

"the ability of the GEOS-Chem CTM to compute the AMF" – GEOS-Chem computes the NO2 profiles, not the AMFs.

"The KORUS-AQ vertical profiles indicate that 95% of the tropospheric NO2 VCD detected from space over the Seoul Metropolitan Area (SMA) originates from the PBL" – The VCD (if fit is correct) does not depend on how it is detected. What is meant here is probably what the authors earlier called „cumulative SCD".

„This leads to a large diurnal variability of NO2 detected from space" – first of all, I'm not sure if a variation of 20% is large for NO2, and second, it is not clear how this is the result of most of the NO2 being located in the PBL.

I think all authors should read the text carefully again and make sure that the wording is precise.

One of the main points of the paper is the improved GEOS-Chem version, which supposedly provides better NO2 profiles for AMF calculations of Asia. However, while the agreement of the new model version with some observed trace gases is improved, this is not obvious for NO2. I therefore suggest to add the AMFs calculated by the standard version of GEOS-Chem in Figure 4 and Table 1 to demonstrate the improvement if there is any or else explicitly discuss the lack in improvement.

Throughout the manuscript it is emphasised, how the new version of the model performs better over Asia, and that GEOS-Chem profiles result in AMFs, which are very close to those derived from measured profiles. While I do not question this, I think it should be stated very clearly, that the KORUS-AQ profiles were all taken during one season and in a very limited number of places, and it is not at all clear if the nice results shown here can be reproduced for another location in another season. To some degree, this model version has been tuned for KORUS-AQ, and I expect problems when it is applied under different conditions.

Somewhere in the manuscript, it should be stated, that all AMF calculations were made for a Rayleigh atmosphere without aerosols (at least I assume that this is the case).

Detailed comments

Line 61: "GEMS is the first geostationary instrument" => GEMS is the first geostationary instrument to measure tropospheric NO2

Line 70: “tropospheric NO2 VCDs vary with the time of day as driven by .. mixing depth“ => I do not think that mixing depht is a driver fort he VCD

Line 187: “we find in GEOS-Chem..:” Isn’t that a matter of the emission inventory, and not the CTM?

Line 352: “GEOS-Chem reproduces closely the observed diurnal variation of the scattering correction factor” => GEOS-Chem reproduces the profile, not the scattering factor

Line 363: “surface type” => while this is formally OK, the driver for the difference is the NO2 profile shape, determined by the presence or absence of local sources.

Line 393: “they are not an obvious source of error when comparing model and observed NO2 profiles“ => this probably depends on whether the scene in GEOS-Chem is cloudy or not

Line 394: „the afternoon formation of fair-weather cumuli would decrease the sensitivity of the satellite measurement to the PBL and therefore alias the observed diurnal variation of NO2.“ => yes, but only if no cloud correction is applied in the retrieval.

Line 401: „to better understand how the vertical distribution of NO2 affects the air mass factors (AMFs) for satellite retrievals of tropospheric NO2 vertical column density“ => I think that the effects of NO2 vertical distribution on AMFs are well understood. Isn’t the point here to look at the effect of diurnal changes in the vertical NO2 distribution on the variation of AMFs over the day?

Figure 1: I find it odd to mix volume mixing ratios and mass concentrations in one figure.

Figure 2: While the median profiles are nice, it would be good to add a figure also indicating the variability of these profiles

Figures 2 – 5: There are some strange steps in these curves – what is the vertical resolution / sampling?