

## ***Interactive comment on “An improved air mass factor calculation for NO<sub>2</sub> measurements from GOME-2” by Song Liu et al.***

**Anonymous Referee #2**

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The manuscript “An improved air-mass factor calculation for NO<sub>2</sub> measurements from GOME-2” by S. Liu et al. describes three improvements to the AMF calculation for GOME-2 NO<sub>2</sub> retrievals:

1. Accounting for directional dependence of the surface reflectance
2. Improving the resolution of the NO<sub>2</sub> a priori profiles
3. Accounting for clouds in the radiative transfer as layered objects, rather than a single reflective surface.

The work done to implement these improvements is solid. Of the three, the change to how clouds are represented is the most innovative, and while other works have

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implemented directionally dependent surface albedo, the authors’ approach is distinct from previous approaches and is a clever way of using the GOME-2 instrument directly. These improvements should make the GOME-2 retrieval better, and therefore should be published in AMT after addressing the issues below.

My only major concern with this paper is that the new retrieval is only compared to a single MAX-DOAS site. Given the wealth of data available for NO<sub>2</sub> validation (Pandora spectrometers, aircraft data from HIPPO/ATom/DISCOVER-AQ and other campaigns), and that recent validation efforts have seen variation in performance even within one region (e.g. Laughner et al. 2019), a global retrieval really needs to be evaluated in multiple locations around the world. Therefore I strongly recommend that the authors expand the validation before final publication in AMT.

My other general question is relates to the authors’ other paper on the GOME-2 retrieval earlier this year (Liu et al. 2019). The authors state that that retrieval is used as the reference retrieval for the one presented in this paper. Why are these two upgrades being published separately? Will either be available publicly?

### **Specific comments**

- When discussing the NO<sub>2</sub> prior profiles, I recommend avoiding characterizing them as “high-resolution”, given that ~ 80 km is still relatively coarse compared to the horizontal resolution used in regional retrievals (e.g. Russell et al. 2011, McLinden et al. 2014, Goldberg et al. 2017, Laughner et al. 2018).
- Pg. 6, l. 15: Over land, SZA and RAA will be relatively constant at a given latitude over a certain time period, but can vary quite a bit over the course of a year. Have you tested how the DLER varies if you fit the constants in Eq. (4) for different latitude bands and time periods (e.g. 1–3 months)?

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- Pg 9: could you be more specific about what characteristics of the DLER are in good agreement with previous work?
- Pg. 15, end of first paragraph: the authors note that the differences in a priori profiles may be due to, among other factors, difference in chemical mechanism? But Table 2 shows no difference in chemical mechanism between the old and new model.
- Related to the previous point: some plots or a discussion of how the emissions differ in the two models would be welcome (perhaps in a supplement).
- Regarding the a priori profiles, how are they matched up to GOME-2 pixels? Interpolation? Area-weighted averaging? Simple averaging? Nearest?
- Pg. 16, l. 8: the authors cite agreement with one study (Kuhlmann et al. 2015) that used much finer horizontal resolution NO<sub>2</sub> profiles (3×3 km<sup>2</sup>) than used in this study. Given that other products using NO<sub>2</sub> profiles at comparable resolution to the Kuhlmann et al. retrieval see larger changes to the NO<sub>2</sub> columns (e.g. Russell et al. 2011, McLinden et al. 2014, Goldberg et al. 2017), this comparison needs to be careful to avoid implying that 0.7° resolution for the a priori profiles performs as well as 3 km resolution.
- Pg. 18, end of second paragraph: my understanding is that using the optical centroid as the “cloud top height” was a compromise between simplicity and accounting for the fact that there is penetration of light into the cloud, that the actual top of the cloud is not a hard surface. This line could be written more carefully to acknowledge that.
- Cloud discussion in general: would be helped by making explicitly clear whether the VCDs being considered are total VCDs including the below cloud ghost column via the AMF correction (just to avoid potential confusion).

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- Cloud discussion: it might be interesting to discuss how this would affect results from cloud-slicing approaches. Since this shows that the sensitivity to below cloud NO<sub>2</sub> is not 0, how should the theoretical framework for cloud slicing be modified?
- Fig. 18 & 19: I don't understand what is meant by “no” aerosol correction. Does this mean that clear-sky box-AMFs were used? If so please state that explicitly, if not, please clarify.
- Table 4: which cloud/aerosol correction is used?
- Tables 4 & 5, Figs. 20 & 21, validation section in general: it would be nice to actually see a comparison of both the operational product and the reference product against the MAX-DOAS data as well as the new product so that we can actually see how the new product is an improvement over those products.

### Technical comments

- Pg. 3 l. 16: “high horizontal [resolution]” - in the context of NO<sub>2</sub> chemistry, 80 km is not really high resolution.
- Table 1: please add to the caption that the different in prior profile models is detailed in table 2.
- Fig. 1c & d: having 0 be at different colors in the two plots is potentially confusing. Recommend standardizing and using the same colormap as in Fig. 3.
- Pg. 10 l. 19: define “case 1 waters”
- Fig 6: does “CIFS” in the legend = IFS(CBA) in the caption, and if so, why are these not the same text?

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- Pg. 15: “These values are of the same order of magnitude as the model resolutions of TM5-MP and other chemistry transport models currently employed in the satellite retrieval of NO<sub>2</sub>” – this is the typical resolution for *global* retrievals, regional retrievals often use 5-80x finer resolution.
- Pg. 15-16: “Consequently, the AMF is underestimated for unpolluted areas and overestimated for polluted areas” - this has been well known for many years (e.g. Heckel et al. 2011, Russell et al. 2011, Valin et al. 2011), please acknowledge previous work.
- Pg. 18, last paragraph: perhaps the discussion of aerosol effects should be moved to the aerosol section? It might make it easier for the reader to follow if this discussion of the shortcomings of the CRB method vis-a-vis aerosols is integrated with the treatment of aerosols in the CAL approach.

## References

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McLinden et al. Improved satellite retrievals of NO<sub>2</sub> and SO<sub>2</sub> over the Canadian oil sands and comparisons with surface measurements. *Atmos. Chem. Phys.*, **14**, 3637–3656, 2014.

Russell et al. A high spatial resolution retrieval of NO<sub>2</sub> column densities from OMI: method and evaluation. *Atmos. Chem. Phys.*, **11**, 8543–8554, 2011.

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