

# ***Interactive comment on “Quantification of the effect of modeled lightning NO<sub>2</sub> on UV-visible air mass factors” by Joshua L. Laughner and Ronald C. Cohen***

## **Anonymous Referee #1**

Received and published: 13 September 2017

In this manuscript, Laughner and Cohen quantify and discuss the impact of lightning NO<sub>2</sub> on a priori profiles used in satellite retrievals of tropospheric NO<sub>2</sub> columns. The authors note that high resolution air quality models such as CMAQ and WRF-Chem may be preferred in some retrieval algorithms due to their spatial resolution, but these models may not represent lightning sources accurately (if at all). They find that ignoring lightning NO<sub>2</sub> can result in substantial underestimates in the calculated air mass factors, mostly in cases with large viewing geometries (including those that will be encountered in forthcoming geostationary observations) due to the relatively longer optical path through the upper troposphere. The effect of accurately including lightning NO<sub>2</sub> is most important under clear conditions given the strong function of scattering

weights with altitude, while the error becomes smaller for cloudy pixels due to fairly uniform above-cloud scattering weights. The biases that result from ignoring lightning NO<sub>2</sub> are regionally dependent, cannot be reduced by averaging, and are sufficiently heterogeneous in space that even a coarse ad-hoc correction to the AMF cannot account for the issue.

Overall, this manuscript is well-written and presents a cogent argument. It is extremely detailed but exceedingly clear, and should be of interest to the satellite remote sensing community. In my opinion, publication in AMT is certainly warranted, and I have only very minor suggestions.

1) I think adding a reference to Travis et al., ACP, 2016 (<https://www.atmos-chem-phys.net/16/13561/2016/>) somewhere in the introduction is important. That paper also called specific attention to the importance of upper troposphere NO<sub>2</sub> from lightning, noting that, if ignored, it can lead to underestimates of the air mass factor particularly over the Southeast US. This seems relevant to the manuscript here, and the present authors are listed as co-authors.

2) In Section 2.2, could the authors include a statement about the vertical resolution with which WRF was run?

3) In Section 2.5.1, the authors note they have used slant column densities from the NASA Standard Product version 2 (SP v2). Krotkov et al. (2017) recently published documentation for the latest NASA release (SP v3), which includes a new spectral fitting algorithm, and higher resolution profiles. Of course, I don't anticipate the major conclusions of this paper to change. But could the authors include a short statement/argument to reassure the audience this is expected to be the case?

4) Also in Section 2.5.1, the authors note the use of TOMRAD look-up tables for the pressure-dependent scattering weights. Can anyone access this look-up table data, or is this provided by request from Bucsela et al. specifically for the BEHR retrieval? If the former, could the authors include the location of where this open data could be

[Printer-friendly version](#)[Discussion paper](#)

accessed by others for their own work?

5) I am curious whether the default BEHR NO<sub>2</sub> retrieval, which uses WRF-Chem model profiles, currently accounts for lightning. I understand this is not directly relevant to the present study (since new a-priori profiles are tested in this case), but it might be useful to point out to the readers, given the authors acknowledge that WRF-Chem is often run by default without lightning NO<sub>2</sub>.

---

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-263, 2017.

Printer-friendly version

Discussion paper

