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Interactive comment on "Quantification of the effect of modeled lightning NO_2 on UV-visible air mass factors" *by* Joshua L. Laughner and Ronald C. Cohen

Anonymous Referee #2

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The authors present a sensitivity study examining the impact of lightning NOx in the BEHR product. As the authors discuss, this is likely not a major concern for operational NO2 products as most operational algorithms rely on a priori profiles taken from global models, which generally include various emission sources including lightning NOx. Simulations from regional air quality models may not include certain emission sources such as lightning NOx as they have little impact on surface level concentrations. Lately, there have been some efforts to use high-resolution a priori information in satellite (e.g., OMI) NO2 retrievals, recognizing profile sensitivity of retrievals and use of course resolution model profiles by operational algorithms. This paper is providing an example that using high-resolution a priori profiles does not necessarily improve

NO2 retrievals, but rather it could add considerable errors in the product. I would be surprised if this is any new piece of information for those involved in operational algorithm development.

This reviewer is aware of the BEHR product and has used it for own research, partly due to the team's assertion that the BEHR NO2 product is highly precise and accurate. In their first paper (Russell et al., 2011), the claim was that the impact of lightning NOx is minimal and could be neglected. In fact, I just discovered that one of the reviewers of the Russell et al., (2011) paper expressed a major concern about the neglect of lightning NOx in their simulation which apparently was dismissed by suggesting that the effect is negligible. As a user of the product, this paper from the same group is very frustrating. If this is still the case in their product, it should be discussed in this paper so that the data users are aware of the error/deficiency in the BEHR product.

I feel that this is a lengthy paper based on very limited research. Large part of the discussion on scattering weights may be a self-educating piece for the authors. The discussion section (Section 4) is either inconclusive or conclusions drawn based on limited research. Due to several major concerns I do not think this paper, in the current form, would add any significant insights to the AMT readership.

Major concerns:

1) Lightning NOx emissions vary strongly both spatially and seasonally. Therefore, retrievals are affected differently for various seasons. Conclusions drawn from ∼1 month in late-spring/early-summer will most likely be incomplete and misleading. I strongly recommend expanding this analysis over the continental US (analyzing urban vs. rural, east vs. west, north vs. south, etc.) for all months. It would be more instructive if this could be discussed in global context.

2) Model vs measurements discrepancies for the upper tropospheric NO2 are bundled solely to lightning NOx emissions and therefore are attempted to address by using a fixed NO mol/flash. There are wide varieties of estimates in the literature, going as low

as a factor of 5 lower than the estimate used in this paper. I think, the authors should perform similar analysis with additional simulations for confidence in the presented results.

3) I am also concerned by the incomprehensive nature of this analysis. The BEHR algorithm is based on several inputs from the operational OMNO2 products, for instance the use of stratospheric NO2 estimates from OMNO2 for calculating tropospheric slant column amount. Modulation of upper tropospheric NO2 should lead to different estimates for stratospheric NO2. Could you do your own stratosphere-troposphere separation and quantify the effect on both stratospheric and tropospheric NO2 estimates?

Minor comments:

4) Page 2, lines 27-33: Wouldn't the lifetime of NOx vary with altitude? Suggesting the upper tropospheric NOx lifetime < 4 days might be misleading.

5) Page 4, line 17, Section 2.3: What is the altitude range of DC3 measurements? Discuss how you treat simulated profile beyond the range of DC3 measurements.

6) Page 5, line 19: How was the estimate of ghost column made? Does this mean, you add a-priori-derived NO2 columns below the cloud to the retrieved tropospheric columns? How does this approach compare with the operational (DOMINO, OMNO2) procedures?

7) Page 5, line 24: What is the logic behind using the fixed tropopause pressure at 200 hPa? Does that mean the BEHR NO2 columns represent columns below 200 hPa? How do you deal with possible errors from using OMNO2-based stratospheric NO2 columns that is likely based on (variable) meteorology-based tropopause pressures?

8) Page 6, Eqn 4: Here and everywhere else in the text. This should be tropospheric AMF, not total AMF. Correct or clarify this.

9) Page 6, line 16: Use of black-sky albedo instead of Lambert-Equivalent Reflectivity (LER) should be a large source of errors. Please comment on this based on some

recent publications (e.g. Lin et al., 2015; Vasilkov et al., 2017).

10) Page 6, line 18-20: Will this approach capture the seasonal variation of surface pressure? How big is its effect on AMF?

11) Page 6, line 22: "OMI cloud fraction...". Is this "effective" or "radiative" cloud fraction?

12) Page 6-7, Section 2.5.2: Related to "Surface pressure (cloudy)". I cannot understand the logic of having different surface pressures for clear and cloudy pixels? Should not this be cloud pressure instead?

13) Page 8, Figure 1: Please, also include profile shapes which might be more relevant for AMF.

14) Page 9, lines: 1:3: This discussion is confusing. Should not the effect be based on the altitude of lightning generated NOx?

15) Page 10, line 2: In "obscure the surface NO2...", don't you mean "below cloud"?

16) Page 16, Section 4.1: What is the message of this section? Why mention nudging at all in the paper if understanding of the 50% decrease in the flash rates by activating FDDA nudging is beyond the scope of the paper?

17) Page 16, line 22: In "less UT NO2 that" => "less UT NO2 than"

18) Page 16, Section 4.2: To estimate uncertainty in cloud slicing, Choi et al. 2014 might have conducted more comprehensive analysis, considering errors in cloud and other parameters.

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