

Interactive comment on “The Berkeley High Resolution Tropospheric NO₂ Product” by Joshua L. Laughner et al.

Anonymous Referee #2

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Improvement to the tropospheric NO₂ retrieval is important especially for the widely used instruments like OMI. The authors present in detail the improved BEHR version 3.0 algorithm, concentrating on the most important parameters in NO₂ retrieval, e.g., surface albedo, surface pressure, and a priori NO₂ profile shape, which is potentially beneficial for studies focusing on high resolution application. The dataset is accessible to the public and is consistent with the description in this manuscript. However, I have got the feeling, at least from the abstract, that the manuscript is more suitable to journals like AMT or ACP, since it is mainly talking about algorithm instead of the dataset itself. Therefore, I would suggest the authors to update the abstract and maybe also the conclusion with more descriptions about the dataset and revise the manuscript addressing the following comments:

general: Despite the good written language, the organization sometimes makes it difficult for me to identify which improvement is for v3.0A which is for v3.0B. For instance, in the “Methods” section, some methods are introduced for v3.0A and some are for v3.0B. Also, the methods of older version are sometimes introduced in “Methods” section (e.g. surface pressure) and sometimes in each subsection (e.g. visible-only AMF calculation). Therefore, this methods section is not fully referable when reading the following sections. In addition, illustrations like “figure 1 shows...” and “table 1 shows...” are missing.

specific:

page2line4 GOME2 (GOME2A in 2006 and GOME2B in 2012) is newer than OMI.

page2line24 I believe the difficulty of NO observation is not only because of the absorption in UV. Even it is noisier than VIS, it still works for gases, e.g. O3 and even possible for NO2. Please specify this sentence.

page2line25 “inferences about total NOx are made from NO2 measurements” maybe also because of the quick conversion of NO to NO2?

page3line4 I suggest including the TM5 also as examples, since it is largely used currently, e.g. for OMI and TROPOMI retrieval.

page3line3-11 I recommend combining these two paragraphs together, since they both talk about how to calculate AMFs, and the “input data” in line 12 talks mainly about the input data (i.e. profiles) in the 1st paragraph.

page5line8 The definition of RRA it not that special, can be removed.

page6line10 What does “BEHR uses the file dated for the day being retrieved for the BRF coefficients.” mean?

page8line7 Why does the BEHR include these different cloud products? For instance, is there a specific reason to include MODIS cloud fraction? Please also add more

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information about the OMI-derived quantities. Do you retrieve the cloud fractions, or do you use OMCLDO2 or OMCLDRR? Also, please update the expression “radiance cloud fraction” to cloud radiance fraction here and through the manuscript.

page8line28 What output is used here?

page9line7 When would this extrapolation happen? If it is because of the different surface pressure from scattering weight and profiles, then it might be even better to shift the profile but not extrapolate.

page9line14 Please specify “when possible”.

page11table2 What is ocean LUT here and through the manuscript? Do you mean ocean reflectance LUT?

page11figure2 This figure is comparing with BEHR v3.0B but it is described in the “Changes in BEHR v3.0A” text. Additionally, the interpretation after figure is talking about the changes in surface reflectance over land. Please add more analysis about changes over water, since the difference is quite significant.

page13figure3 The figures are not clear and the conclusions are not convincing to me. Why is there a straight line (a deep blue line with no percent change in NO₂ near cloud pressure 700 hPa in (a) and a distinct line near the black dashed line in (b))? What is the definition of surface NO₂ concentration and why does it matter here? Since in Eq. 2, it is only the profile shape (relative vertical distribution) matters in the AMF calculation but not the absolute concentration. Also, I am not sure with “Greater percent difference with greater surface NO₂ concentration.”, because most of the largest differences in the figure are found for low surface NO₂ concentration (blue to deep blue in (a)). Similarly, greater percent differences are also found for small difference between the cloud fraction and cloud radiance fraction, since quite a lot of yellow dots are close to the black dashed line in (b).

page14line21 What is UT?

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page15figure4 the (a) and (b) panels are not described anywhere.

page15line3 I do not understand this hypothesis, since the profile shape itself depends not on the cloud information. The selection criteria of cloud fraction 0.2 only has impact on the NO₂ column calculation, and it has no impact on the profile shape.

page17line5 Is there probably a name or reference to this temperature profile?

page18line6-13 There are introductions of the previous method and why PSM method cannot be used here. It might be better to add a small introduction of the new CVM which is actually used in this study.

page19line7 The ocean reflectance is calculated without MODIS data, therefore I do not understand the goal of this change to 460 nm. Even the impact is small, the reflectance at 430 nm shall be used because of the reason exactly described in the text.

technical:

page7line2 change “within IN” to THE

page8line20 add “that year’S total”

page14line15 add “meteorology ARE also”

page15line6 remove “it IS seems”

page15line8 change “INFLUENCED by lighting”

page18line11 change “algorithm RESULTS”

page18line15 remove (2)

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