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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Technical note: Reanalysis of Aura MLS Chemical Observations" *by* Quentin Errera et al.

Anonymous Referee #3

Received and published: 23 August 2019

This paper evaluates BRAM2 against both assimilated and independent observations for multiple species. Overall, I find the paper to be an impressive piece of work that is well written and well organized, and think the BRAM2 output will be an important dataset for analysis by researchers. I only have two minor issues that I bring up below, followed by a few technical points.

Main issues:

The paper is quite long, and the descriptions of comparisons where the authors break down each species is tedious because the authors essentially list a bunch of information that is already in the figures (e.g., satellite X agrees with BRAM2 within y%, at z hPa. . .). There is still useful descriptions in these sections of the overall comparison, but I would suggest minimizing the listing of mean differences/standard deviations

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where the information is already in the figures

Why are comparisons done to the forecast and not the analysis? I think of the analysis step as the “best estimate” of the atmospheric state (and the main point of reanalyses in general), not the forecast. Could the authors at least explain this decision to do FmO a little better?

Technical points:

Page 7, lines 3-4: Can some reference be given for the v2.1 quality flags algorithm?

Page 7, lines 18-20: Rather than just saying the O3/WV products were validated, could the authors also state some high level findings from these studies (e.g., whether or not MIPAS had biases).

Page 8, line 14: Why not average the ozonesonde data to a vertical resolution more closely corresponding to the model? Later, on page 22 line 19 you mention that this difference in vertical resolution might cause larger standard deviations. I don’t see the advantage of using ozonesonde data on such a high vertical resolution.

Page 9, lines 17-18: Can the authors state the approximate vertical resolution of the model? This is important so readers can understand how it compares to the assimilated observations.

Page 16, line 13: ACFTS -> ACE-FTS

Page 25, line 3: I’m not sure this result suggests BRAM2 could be used as a transfer function, although I don’t dispute that BRAM2 could be useful as a transfer function. The N2O drift seems more like a “garbage in-garbage out” scenario where if you assimilate an obs that has an unphysical drift in it (i.e., MLS N2O) and thus all of the other measurements show an apparent drift relative to BRAM2.

Page 26, line 13-14: same comment as above

Conclusion section: BRAM2 just assimilates MLS, and the authors have done an ad-

mirable job in comparing to independent data. I this section they talk about potential improvements, but one improvement they don't discuss is if and how the assimilation would be improved by assimilating new sources of data (e.g., in the regions where MLS data are unreliable). Some discussion of this would be interesting if the authors are willing.

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