

Interactive comment on “Representativeness errors in comparing chemistry transport and chemistry climate models with satellite UV/Vis tropospheric column retrievals” by K. F. Boersma et al.

Anonymous Referee #1

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This manuscript presents a methodology for properly comparing models for atmospheric composition with Level-2 retrievals from satellites in the UV/VIS. It does so by identifying three potential issues, testing the potential introduced errors for these issues and recommend procedures to avoid the errors. The manuscript is generally well written (apart from Section 2; see detailed comments below) and the scientific methods are sound. However, the presented concepts and methods are not really new and the manuscript is in that sense a bit disappointing. Sampling issues and the use of averaging kernels have been discussed in the literature for some time now. Never-

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theless, I support the publishing of this manuscript as a model assessment methods paper, subject to minor revision, because it provides a good summary of the issues and guidelines, which will hopefully encourage scientists in the field to make better use of these concepts.

Detailed comments:

Section 1: I am missing some important earlier work, such as the studies by Rodgers and Connor, JGR, 2003 and Migliorini et al., Monthly Weather Review, 2008. These could maybe be added in lines 16/17 of page 7827?

P7827, line 11: The authors go a bit fast here. While the statement is correct, I had to read it several times to understand the logic. I would advise to add a sentence that explains that the contribution of the prior profile to the final solution increases with decreasing sensitivity of the measurement.

P7827, line 22-25: Assuming $xa=0$ makes the problem non-Gaussian, because xa cannot be negative. How does this assumption affect the discussion?

P7828, line 23: ‘whereas satellite measurements provide “snapshots” at a particular local time’: this is not a description of spatial error but of temporal error. Please remove.

Section 2.3 is a slightly odd section. In section 1 the authors say that section 2 will introduce the issues, while section 2.3 is actually used to describe the proposed solution. There is therefore overlap with text later in the manuscript, which does not help the reader. I suggest to either remove section 2.3 or make it part of text later in the document (e.g., Section 3.2). Also, the end of section 2.2 describes conclusions, which should not be part of this introduction.

P7830, line 3: Should you not do the opposite in this case, i.e., average over all the grid boxes that fall within the satellite foot print?

Section 2.4 is slightly strange as well. Of course, one has to be aware of clouds and their impact on the retrieval. But wasn’t the whole point of recommending the use of

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averaging kernels exactly to deal with these kind of issues. Maybe, the authors could mention the impact of clouds in a shortened paragraph earlier in the manuscript and then present the use of averaging kernels later in the manuscript both in the context of vertical sensitivity in general and in the context of clouds.

P7832, last paragraph: Treating systematic errors as random is a strange assumption considering the fact that the authors acknowledge later on that the systematic model errors can be as large as 50 %. This needs some further clarification.

P7833, lines 10 – 12: I am not sure if I understand this argument. Averaging will remove the random component from the error budget, so systematic errors will actually become more dominant. Or do I miss something?

Section 3.2: Maybe the text of Section 2.3 could be used here.

Section 4.1: It is unclear what the relationship is between the model profile used for the AMF and the a priori profile (set to zero under the weak absorbing assumption) used in the retrieval. Both prior assumptions play a role in the definition of the end product, but it is not clear how this is accounted for in the averaging kernel.

Page 7837, eq. 8: This was already described in Section 2.3

Page 7838, lines 2 – 4: Does this mean that the DOAS error estimates are not correct? If they would be, they should be taken into account. I am not convinced by the argument that the method drives the statistical interpretation of the results.

Page 7841, line 13: Are the TM5 cloud fractions indeed simulated by the model or do they come from ERA-Interim. If the former is the case, it would be worth mentioning how these cloud fractions are simulated. If it is the latter, this should be mentioned as well.

Section 5.3: Although the presented comparisons do indicate a better agreement between model and observations, it is not rock-solid proof. There remains the possibility that the use of averaging kernels is masking/compensating other errors in the model.

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This is probably worth mentioning for completeness.

Appendix D is not referenced in the text. However, it contains interesting content that could maybe be used in Section 4.1? See also my earlier comment about Section 4.1.

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