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Atmospheric Chemistry and Physics Discussions

ACPD

Interactive comment

Interactive comment on "Constraints and biases in a tropospheric two-box model of OH" *by* Stijn Naus et al.

Anonymous Referee #1

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Naus et al. presented a modeling analysis to quantify the uncertainty in the derived estimates of global OH and CH4 emissions due to the difficulty in an accurate representation of the real atmospheric 3-Dimensional transport and chemical processes in a simplified two-box model framework. A comprehensive set of experiments were conducted to investigate the impact of inter-hemispheric transport, representative of surface observational network, inter-hemispheric OH ratio, the differences in the sensitivity of various chemical compounds to the spatial distribution of emissions and OH, etc. The analysis presented in this work is a further step of what have been discussed in the recent literature on global OH abundance and CH4 emission estimates, e.g. Rigby et al. (2017), Turner et al. (2017), Liang et al. (2017). Results from this work are a nice addition to these previous published papers and should be published after the

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following comments, mostly minor, are addressed.

Major comments:

1. Use of tense in "Abstract" and "Summaries and Conclusions" are not consistent. I have noticed that the authors swap back and forth between present tense and past tense in these two sections. The common practice would be use consistently one tense through abstract and conclusions sections. For example, we did . . .; we found . . ., we investigated . . .

2. Personally, I found the current version of Abstract not fully capturing the essence of the findings from this work. While the results presented in the main body of the text are important additions of the existing literature, the abstract only includes rather general and vague discussions and no clear identifications of what are the crucial parameters that needed to be considered if one is to adopt the two-box model approach, etc. A reorganization of the abstract, with clear emphasis on what are the key findings of this, in the context of previous literature, would be helpful to readers.

3. P12, last paragraph and Figure 2. The positive trends in the IH exchange rate for CH4 and SF6 are very puzzling. Based on the results presented, it is not convincing to arrive at the conclusion that these trends are due to acceleration of IH transport of air mass or a shift in the pattern of IH transport. Are you sure this isn't due to a model spin-up related issue? If it is indeed the change in the IH transport rate, there must be a way to quantify this using the proper diagnostics from the TM5 model or set up sensitivity runs to tackle the problem. At this point, it sounds very hand-waving to arrive at the conclusion that it is due to changes in IH transport with no real analysis backing up the conclusion.

4. P15, 1st paragraph. As discussed in section 3.3.1 last paragraph in Liang et al. (2017), if one were to use the surface measurements to perform two-box model calculation, the NH surface to SH surface IH transport time is needed in the gradient-to-emissions calculation to be consistent. On the hand, if the entire tropospheric air mass

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is considered, the IH transport timescale is significantly reduced (as demonstrated in this paper) due to the nature of cross-hemispheric transport in the troposphere. More details can be found in Liang et al, section 3.3.1. The authors seem to have the two methods (or concepts) mixed when discussing this. It would be good to add a discussion on these two different conceptual models if the authors wish to compare the results presented in this work with those discussed in Liang et al. to avoid confusion.

5. P22, L26-27. "In the end, conclusions from our study and those drawn by Rigby et al. (2017) and Turner et al. (2017) remain qualitatively similar". Isn't this a much more important conclusion than the way it is presented here in this paper? Despite all the other details discussed, e.g. box-model simplifications, bias corrections, etc., this paper confirms the findings from Rigby et al. (2017) and Turner et al. (2017). In addition, I found Sections 4 and 5 somewhat wordy. While lots of details are discussed, it is hard to drawn the main conclusions, e.g. what are the important details that one needs to consider when conducting box-model-based calculations. Some reorganization of the discussion and conclusions and emphasis on the key factors/uncertainties/parametrizations can be helpful.

Minor comments:

1. P1, L3: OH is already defined in the previous line.

2. P2, L 4: What do you mean by "involving"? It might be better to use "complex" or "state-of-the art"

3. P2, L30: has -> had

4. P3, L18: Add "Observation-inferred" before "emissions"

5. P3, L23-34. "these two processes have a very different effect on the IH gradient of MCF". It is much more helpful to stated directly what are the different effects of these two processes, than leave it to the readers to wonder about it.

6. In some places, inter-hemispheric is used while in others, interhemispheric (intra-

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hemispheric) is used. Please be consistent.

7. P8, L11 and L13: subscript 4 in CH4
8. P9, L2: delete have after we
9. P9, L5: the 3D and the two-box models. (plural for model)
10. P9, L10: Add Global Monitoring Division (GMD) after NOAA.
11. P9, L30: “we identified three parameters . . .”. Please state which three.
12. P15, L24. Add “it” before “is”.
13. P22, L18-19. This sentence is awkward. Need rephrase.
14. P22, L19. Change to “uncertainties we found”
15. P23, L20. Change to “They found”
16. P22, L22. Add “,” after “likely solution”

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