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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Spatial and temporal variability of the hydroxyl radical: Understanding the role of large-scale climate features and their influence on OH through its dynamical and photochemical drivers" *by* Daniel C. Anderson et al.

Anonymous Referee #2

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Review of acp-2020-1192 by Anderson et al.

The manuscript acp-2020-1192 evaluates the drivers of interannual variability in hydroxyl radical (OH) throughout the troposphere. The authors primarily use model output from a 38-year chemical transport model to calculate OH variability and correlate this spatially and temporally with various climate modes, with specific focus on ENSO. They also compare results to other atmospheric chemistry models and where possible

satellite data to test the robustness of their conclusions. This is a detailed, careful, and well presented analysis that adds to our understanding of OH science and should be published in ACP once the comments below have been addressed.

GENERAL COMMENTS

Methane feedbacks: There are known feedbacks between ENSO, methane, and OH. However, according to Section 2.2 (lines 203-204), methane in the model is a fixed boundary condition, which I understand to mean that it wouldn't vary in response to ENSO or other conditions. Therefore some component of the ENSO-driven OH variability may be missing in the simulation (this may be true for other modes too, e.g. Australian monsoon). It would be worth a brief discussion somewhere of this and the implications for the conclusions.

Satellite comparisons (Section 3.2): As far as I can tell, the model comparisons to satellite data have NOT taken into account the retrieval process used in the satellite data (e.g., application of averaging kernels and a priori for CO / shape factors and scattering weights for NO). This introduces an element of bias into the comparisons – even if the model was 100% accurate, the values retrieved by the satellite would not match the model (because the satellite retrievals are not a perfect observing system). While this is an understandable choice for the scale of analysis in this paper, this needs to be clearly discussed. It also means that the comparisons should be considered more qualitative than quantitative (e.g. show of the quoted biases are probably not accurate), and some of the conclusions from this section should be stated less strongly.

SPECIFIC COMMENTS BY LINE NUMBER

79-91: The relationships between a variety of climate modes and CO anomalies quantified by Buchholz et al. (2018; https://doi.org/10.1029/2018JD028438) would be relevant to include in this discussion. Also, the last sentence notes observed relationships between NAO and CO long-range transport, but other work has shown CO long-range transport also changes with other climate modes (e.g. with ENSO, see Fisher et al., 2010; https://doi.org/10.5194/acp-10-977-2010).

143-162: I suggest including a table that lists all the modes and monsoons included in the analysis and a column that indicates their source (for the modes) or the fact that they are calculated (for the monsoons). Right now I think it's a bit hidden that the monsoons are model-dependent while the modes are not (and I wonder if this has any influence on the fact that you tend not to find agreement between the models in the monsoon analysis in Section 6).

203-204: More details of the methane concentration implementation would be useful here. What is the source for the methane values? At what latitudinal and time resolution are they input to the model? Are the specified concentrations applied throughout the troposphere, or only at the surface then allowed to advect freely?

229-231: What biomass burning emissions are used in the CCMI models, and are they consistent with the GEOSCCM biomass burning emissions?

325: I'm somewhat surprised to see the overestimates in the SH in JJA linked to biomass burning, as this is not peak burning season in the SH, and the spatial patterns within the SH continents don't look like the areas of primary dry season burning. Is there other evidence to support the conclusion that this is a biomass burning signal?

404: How are El Niño "events" defined? And what is the timescale used? I think in some places the terminology is "events" and in other places it is "years" – are there sub-annual events, or is each year classified in its entirety? This also needs addressing in the Figure 6 caption, which describes a difference between "El Niño events and neutral years" – if events are sub-annual, how does this work?

478: Does the Percent Variance in the table (and quoted throughout the text) refer to the spatial variance? It would be useful if this were clarified somewhere in the text, as it's a little confusing to see such large disconnect between the percent variance explained and the rˆ2.

513-514: "Analysis of vertical winds. . ." – it's not clear to me how Fig. S6 supports this conclusion (and I find it hard to tell what is going on in Fig. S6). To clarify would probably require more discussion – but as the paper is already very long with a huge number of figures between the main text and the supplement, I would suggest just cutting this one.

544: "the 2nd EOF for the PBL" – it would be useful to have an equivalent figure to Figure 7 included in the supplement to show the 2nd EOF for the PBL, since it is the most correlated EOF here.

583-584: This sentence needs a reference

654-656: I am confused and think more discussion is warranted here. The statement is that the difference in the role of lightning NO emissions could be due to "differences in the handling of biomass burning emissions in the two models", but only the MERRA2 GMI biomass burning emissions are explained. How were the biomass burning emissions handled in the Turner et al. study? Second, why would the lightning NO emissions change in response to the biomass burning emissions? Or is the argument that the Turner et al. study was missing variability in biomass burning and so incorrectly attributed the variability to lightning NO? Please explain.

715: In Fig 18 and other similar figures, I'm not sure of the validity of including the MERRA2 GMI model that is the focus of the paper in the model count. What would be more useful would be to see how many of the independent CCMI models reproduce the relationship seen in MERRA2 GMI. I think this could be done by changing these figures to have only the 4 models in the color scale, but then using e.g. hatching to overlay where MERRA2 GMI had a significant correlation. This would be a clearer comparison of the robustness of the MERRA2 GMI relationships already identified.

799-800: "TCOH is anticorrelated with the DMI" – any ideas why?

TECHNICAL COMMENTS BY LINE NUMBER

29: “Reductions in OH due to ENSO” – please change ENSO to El Niño as ENSO would imply OH is reduced during both phases.

195: Please specify the version of MEGAN used.

263: I would suggest turning this into a 4-panel figure that combines the existing Figure 1 with the existing Figure S2 – the ATom 1 comparison is also useful to see without having to flip to the supplement.

286: “When data from these regions are omitted” – it would be useful to identify the “omitted” data points in the plots shown in Fig. 1 (e.g. with a different shape, filled circle, etc.).

292-293: It would be worth referencing the more recent work of Travis et al. (2020; https://doi.org/10.5194/acp-20-7753-2020) here.

319: “boreal winter” – specify months in parentheses.

376: “In JJA, the climate modes” – please clarify what this refers to... Is it just the NH modes? All climate modes (NHmodes + ENSO + IOD)? Or all drivers (NHmodes + ENSO + IOD + monsoons)?

407-408: “We focus our analysis on DJF....” – the text would be clearer if this sentence came earlier, before the current sentence starting on line 405 “Figure 5 shows...”

425: Duplicated 5.1.1, this should just be 5.1

483-484: “with greater than” – please change to “with NO2 greater than”

518-519: “The dominant OH sink...” – this sentence would fit better with the earlier discussion of Figure 5 and the dominant OH source pathways.

527: “Same as Figure 6” –> “Same as Figure 6ab”

536: “smaller than in the UFT (Fig. 10)” – it is odd to mention the UFT in this context before it has been discussed. I suggest removing this part and just keeping the

comparison to TCOH here.

557: “Figure 11 shows...” – the wording is unclear both here and in the caption. I think in both cases, the correlation is with the MEI, but in one case it is MEI vs. OH production from H2O+O1D and in the other it is MEI vs. total OH production rate. If this is correct, I would suggest clarifying the sentence to start “Figure 11 shows the correlation of MEI against both OH production from this reaction and the total OH production rate for the PBL.”

577: Here and elsewhere, JO1D → J(O1D)

638: “Fig. S7” – should be “Fig S6.” (although I have suggested earlier that this figure be cut)

643-644: “lightning emissions ... does not” → “lightning emissions do not”

644-651: There should be a reference to Fig. 16 somewhere in the text of this section.

708-709: “the CCMI models” – it would be useful to remind the reader here how many CCMI models are being included (4 I think?); e.g. “the 4 CCMI models...”

765-767: I find this long sentence difficult to interpret. I would suggest splitting into two sentences, one about the NAO and one about the PNA.

774: “vary markedly among the different monsoons (Fig. 4)” – I find this very hard to see from Fig. 4. I would suggest a separate figure in the supplement that shows just the monsoon correlations to aid this discussion. Same goes for the reference to Fig. 4b&d in line 786.

811: Suggest swapping the order of panels b and c to reflect the order in which they are discussed in the text.

Throughout: If I understand correctly, the main model is sometimes referred to as GEOSCCM and sometimes referred to as MERRA2 GMI - please make the name of the model consistent throughout.

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