Review of Anderson et al., 2020

Major Comments:
The goal of this study is to determine the relationship between tropospheric OH and ENSO, Northern Hemispheric modes of variability, the Indian Ocean Dipole, and monsoons. The authors present an analysis of one main model (GEOSCCM) evaluated with aircraft and satellite observations, to make the case that their model can be used for this purpose, and show that their findings have some similarities with the results from four CCMI models. This work finds that multiple modes of climate variability, including ENSO, can explain OH variability over approximately 40 % of the globe. The authors also find that OH mainly changes in the boundary layer and upper troposphere, and the mid troposphere is less impacted by different climate modes. I have several key concerns. First, the analysis of the model with aircraft and satellite observations is lacking in clarity, and possible technical approach, depending on the answers to my specific comments below. As an example, given the difficulty the model has in representing ATom OH observations in outflow from South America and New Zealand, changes over these regions should be more carefully discussed in Section 5. The authors might also discuss why they do not use any aircraft observations over land. In addition, a key finding of this paper is the finding that lightning NO_x is not a main driver of the OH-ENSO relationship, in contrast to Turner et al., 2018. The authors need to improve their support of this argument. Finally, I think the authors are missing an opportunity to address the causes of the wide variety in OH and methane lifetime across models that they point out in their introduction. While the authors say that there is agreement across models in the importance of the driving factors of OH variability, for example only three models show that ENSO is important in DJF. This manuscript is appropriate for ACP and will represent a strong contribution to the field after the major revisions to the analysis described below.

Specific Comments:
Page 2, line 58 – It is incorrect to say that formaldehyde only comes from CH4 oxidation, please clarify what you mean here.

Page 3, line 135 – Could you clarify what you mean by detrend? Are you removing the seasonal cycle?

Page 3, line 143 – Could you describe whether the climate features you take from NOAA (MEI, DMI, the Northern Hemispheric modes) are well represented in the models so that you don't have to recalculate them from the model output?

Page 4, line 168 – Again please describe the method here a little better. What linear fit are you subtracting? What trend are you removing? Why do you have to divide by the standard deviation?

Page 5, line 202 – It might be clearer to say that "over the tropical Pacific area, …. emissions vary by up to 50 % over the time period studied."

Page 6, line 268 – Are you including the land crossings, or only using ocean data? The OH points that degrade the model/observation correlation seem like they must have some defining characteristic – if not over land, than maybe some other type of plume?

Page 6, line 275 – Per my comment above, would it not be more appropriate to average the ATom observations to the model output resolution, rather than interpolate to ATom? Otherwise, this is not an apples to apples comparison. There is no way the model will compare well to 5-min data when it is output at hourly resolution

Page 6, line 279 – I would prefer Fig S2 to be part of Figure 1. It seems arbitrary which is shown in the main text, and they are both important, particularly since Fig. S2 doesn't shown the same OH outliers in the SH as Figure 1. It would also be useful if they were on the same scale.

Page 7, line 285 – I see that the outliers are somewhat explained here as being driven by continental outflow from South America and New Zealand. Does this imply errors in model NO_x in these regions? Or missing model OH recycling from biogenic VOC oxidation? Can ATom shed some light on this? Just stating that the correlation improves when those points are removed does not improve confidence in the ability of the model to simulate OH.

Page 7, line 293 – To my knowledge, it has not been shown that fixing the CO bias improves model OH biases. It is true that the low CO bias in the NH is well known, and the model OH bias is well known, but not that the OH bias is due to low CO. Please clarify your point here.

Fig. 2 caption, please explain what you mean by "satellite lifetime?"

Page 7, line 324 – Again it would be easier to read this if Fig. S3 was part of Fig. 2. It seems important enough to the discussion to warrant being in the main text.

Page 7, line 324 – You say that "overestimates of column CO, averaging 45%, in the SH corresponding with areas of biomass burning." Is that really true in SH winter (Fig. S3)?

Page 8, line 341 – Are you re-calculating the OMI NO_2 columns using the model a priori to give you an apples to apples comparison, per Lamsal et al., 2014 (doi:10.5194/acp-14-11587-2014)?

Page 10, line 408 – You say, "The relative importance of the individual reactions is similar during neutral and La Niña years (not shown) and is in agreement with previous model studies (e.g. Spivakovsky et al., 2000)." I am confused since you show in Fig. 4 that ENSO is correlated with TCOH, mainly in the tropics. How can this be if the relative importance of R1-4 is the same during neutral and La Nina years?

Page 10, line 413 – You do not use the term 'UFT' in the supplement (Fig. S4). Please be consistent in your terminology.

Page 12, line 456 – Does this mean that the variability shown in Fig 6a is driven mainly by changes in DJF as shown in Fig. 7a? If so, should Fig. 6 be shown seasonally similar to Fig. 7?

Page 14, line 514 – I don't understand the plot of satellite NO_2 against the MEI for Fig. 8. You say that "OMI data are insufficient," but show a plot anyway. If you aren't going to talk about it, maybe don't bother showing it. If you are filtering out noise from the NO_2 retrieval, and this leaves you only with NO_2 over land and outflow regions, why do you show ocean values for NO_2 in Fig. 2?

Figure S6 – Should the caption say, "model" NO_2 to differentiate from satellite?

Page 14, line 535 – Is there a plot somewhere for the sentence "Changes in the PBL during La Nina…?"

Pate 14, line 547 – You might want to consider the mass-weighted concentration of OH more consistently throughout the paper, and possibly the methane lifetime as well. This might provide an additional perspective on how different climate indices change the atmospheric oxidation capacity and address the variability in methane lifetime across models that you describe in the introduction.

Page 15, line 545 – According to Table 1, the r^2 with ENSO in JJA is less than 0.25, this should be pointed out.

Page 15, line 551 – Europe looks like it has decreases not increases, but it is hard to tell with the way the plot is centered. Please double check.

Page 17, line 558 – Do you mean "These increases in **j**O1D", not "These increases in O1D?" Also, does the model show that decreased stratospheric ozone is the driver behind the increase in photolysis?

Page 17, line 621 – It is surprising to me that if stratospheric ozone impacted jO1D in the PBL, it wouldn't also be important in the UFT. Please clarify.

Page 18, line 638 – I don't think the reference to Fig. S7 is correct, did you mean to refer to a different figure?

Page 18, line 645 – H2O(v) is Fig. 8a, not Fig. 8c.

Page 18, line 646 – It seems that there is a figure, Fig. 16, that you should refer to here in your discussion of lightning.

Page 19, line 652 – I don't understand your discussion of Turner et al. (2018). If I understand Turner et al. Fig. 4, correctly, there is more lightning during La Nina, but less during El Nino and thus La Nina is positively correlated with increases in OH. If I understand your Fig. 15 correctly, if NO goes down as MEI goes up, then NO goes up as the MEI goes down, meaning more lightning during a La Nina. This actually agrees with the findings of Turner et al. Please clarify if I am missing something here. If I am wrong, you will definitely need to better clarify your statement about biomass burning and be more specific about how interannual variability in emissions could have changed your results vs. Turner et al., 2018. For example, maybe you need a correlation plot of biomass burning emissions with MEI.

→ Page 19, line 667 – It seems that part of the argument is given here. Again, I would suggest that a biomass burning correlation plot or something similar would strengthen your argument. It does seem unlikely though that biomass burning would have such a large impact on the UFT, this definitely needs more discussion.

Page 21, line 740 – What is going on such that EMAC has no correlation in the column, but a very strong correlation in the UFT?

Figure 18 – While you focus on the agreement in your discussion of this figure, the discrepancies are actually quite large considering that all 5 models do not agree on the relationship of ENSO to TCOH in most cases. While you say it is beyond the scope of the paper on Page 22, line 748, given your analysis on the drivers of the ENSO to TCOH relationship from MERRA2 GMI, can you suggest areas of focus that might help us understand the huge model to model variability on OH and methane lifetime that you discuss in the introduction (e.g., Voulgarakis et al., 2013;Nicely et al.,;Zhao et al., 2019)?

Page 22, line 767 – It seems like Figure S13 is more appropriate for the main text.

Page 22, line 768 – Here you do talk about mass-weighted OH. I will just reiterate that it could be extremely useful to calculate changes to mass-weighted OH, or even better, changes to the methane lifetime in El Nino vs. La Nina.

Page 22, line 789 – Can you check the model output during JJA to see whether there is really no interannual variability? It does seem more likely that the model is not accurately capturing the chemical variability within the monsoon anticyclone.

Page 23, line 819 – Could this not also be that most models fail to capture the mechanism behind the monsoon impact on OH? If it is due to convectively lofting pollution above the monsoon clouds, then it seems very likely that all models have difficulty with this. Could you look to see whether your model has expected elevated levels of pollution during the monsoon in the UFT that might drive increased OH as described by Lelieveld et al., 2018?

Page 24, line 832 – Could you say something as well about the average decrease in the methane lifetime (using the methane concentrations from the model)?

Page 24, line 844 – This statement about lightning emissions is very important, and definitely needs more support in the prior text. I don't think that "increased convective lofting of low NO air from near the surface and advection of air with lower reactive nitrogen than during neutral years" has been well supported.

Technical Corrections

Page 7, line 288 – "a NMB", not "an NMB"

Page 15, line 546 – I think it should be "ENSO-related".