The authors have addressed some of my concerns, but I still have significant remaining concerns and some additional issues.

1) Lack of consistent notation for what is calculated, inferred, and observed in the peroxides. The authors go back and forth between various notations in the text and figures. PSS-H2O2, H2O2 PSS, $[H2O2]_{PSS}$. Similarly UHP is referred to as UHP or PSS-UHP, and MHP is referred to as MHP or PSS-MHP. Observed H2O2 is referred to as H2O2 HYPHOP or $[H2O2]_{HYPHOP}$, or in situ H2O2 or H2O2. EMAP H2O2 is referred to as H2O2 EMAC or modelled H2O2 (Figure 3). This is highly confusing! Please pick one notation and be consistent throughout the manuscript, carefully going over the figure legends and captions. For example, on Figure 4, H2O2 should be labeled as H2O2 HYPHOP to be consistent with Figure 3. Also, MHP should be MHP PSS (or $[MHP]_{PSS}$). I suggest that throughout the text UHP be labeled as $[UHP]_{PSS}$ as it isn't measured. For the figures to be consistent with the text, H2O2 PSS should be labeled $[H2O2]_{PSS}$.

2) While the authors have changed the manuscript in several areas to include a more quantitative comparison between PSS, EMAC and observations, there are still several areas that remain vague and qualitative, and could thus be improved (see in list of minor comments below).

3) In response to my concern about the authors' discussion of the deviation between EMAC and observed H2O2 and ROOH in terms of scavenging efficiencies, the authors added information in their reply that is not included in the revised manuscript. For example, the authors state that "CO in the upper troposphere is lower in EMAC due to weaker convective transport (possibly caused by the low resolution of the global model) as described in Tost et al. (2016) and Tomsche et al. (2019)." This is relevant to their study but not included in the manuscript, where the authors attribute the difference between observed and EMAC H2O2 solely to excessive scavenging. Also, when looking at Tomsche et al. (2019) (their Table 1), it seems that EMAC is overestimating CO in AMA but underestimating CH4, so I am confused. Is this an issue with emissions of CO and CH4? In their response, the authors also say that scavenging of all soluble species is turned off in their sensitivity study and that this is done globally. This is not clear from the text, which remained unchanged as "To investigate this assumption we performed a sensitivity study with EMAC excluding scavenging." The authors should update the text to be provide more detail to the reader. Finally the authors do not address whether the EMAC very large underestimate in ROOH can be explained by excessive wet removal during transport. It seems that their sensitivity study should be able to answer this question in a straightforward way.

Various other comments:

- Abstract: The deviation from steady-state is mentioned twice in the abstract but not quantified "In general, the observed concentrations are higher than steady-state calculations and EMAC simulations. " "…explaining strong deviations to steady-state calculations which only account for local photochemistry. " The authors should clearly state the magnitude of the deviation from PSS for H2O2.

- Abstract line 29: “strong deviations FROM steady-state calculations “

- Abstract line 30: The expression “Deviations to EMAC simulations” does not mean anything.

- Abstract line 30-33: “Deviations to EMAC simulations are most likely due to uncertainties in the scavenging efficiencies for individual hydroperoxides in deep convective transport to the upper troposphere, corroborated by a sensitivity study.” And text lines 410-411 “Differences between observation and EMAC simulation could potentially arise due to uncertainties in the scavenging efficiency for H2O2, as the chemistry does not seem to be a dominant cause of uncertainty.“ From the response to the reviewers it seems that another explanation is the too weak convective transport in the model due to its relatively coarse resolution. The authors quoted an underestimate in CO. This should be noted in the abstract and text.

- Line 78-80. The authors should state what is actually measured in this paragraph. They only mention that the measurement of ROOH is unspecific without mentioning what is actually measured…

- Line 95. “The hydroperoxide data…” is very vague. It would be helpful to the reader if the authors would actually state what species are measured. Please clarify.

- Lines 132-140. Given that the ROOH measurement is non-specific, I find it confusing that the LOD and uncertainties are given for MHP. Shouldn’t they be given for ROOH, which is actually what is measured? Please clarify the text.

- Section 3.3 For clarity [MHP] in all the equations should be replaced with $[MHP]_{pss}$. This would make things consistent with section 3.4, where $[H2O2]_{pss}$ is used. Also, it is unclear if [H2O2] in equation (13) is the observed ($[H2O2]_{obs}$) or PSS value ($[H2O2]_{pss}$). Please clarify the text.

- Lines 254-255. This statement is misleading as it suggests that the EMAC simulations were done only on the OMO flight tracks. I assume that the authors mean to say that the EMAC results were extracted along the OMO flight tracks. Also, what is the vertical resolution of the simulation?

- Line 262. The meaning of “which found on cluster analysis” is unclear. Please reword.

- Figure 6. The regression coefficients listed in the figure legend (0.99, 0.98, 0.99) are not consistent with what is shown in the Figure and what is discussed in the text (line 313-314) stating that the correlation for H2O2 and ROOH are not as strong. Also please specify in the figure legend whether the values are r or r^2. Same comment for Figure 10, where the quoted correlations (0.96, 0.97, 0.96) seem too high. Also for Figure 10 is the black line for the entire dataset of only AMA? This should be specified in the figure caption.

- Lines 358-360. I already pointed out in my first review that the statement "The deviations from unity in the slope are within the combined uncertainties of measured and steady-state estimations of H2O2 (51%)" is quite wrong. The authors concurred by saying in their reply that 82% of the points are OUTSIDE the range of uncertainties, but haven't updated the text to correct their misleading statement.

- Section 4.3.2, lines 368-370. I agree with the second reviewer in that comparing the ranges of observed and modeled values is not informative. I suggest that the authors remove the discussion of ranges here as they have added the discussion of medians, which is a more relevant metric.

- Section 4.3.2. The authors go back and forth between referring to AMA and monsoon in the text, Table 2 and Figure 14. Please use one consistent notation.

- Lines 372-374. "For the SH the model simulated H2O2 mixing ratios is four times higher than in the NH background (272 pptv), while the observations only show a median increase by 47 pptv to 211 pptv (Table 2)." This seems incorrect as the observed increase is 111 pptv. It would actually be more relevant to compare the relative increase in both (factor of 2 increase in observed H2O2 between NH and SH background.

- Table 2 and Section 4.3.2. For the discussion of the comparisons of MHP and UHP, it would be more relevant to compare observed ROOH and EMAC MHP+EHP+PAA as the comparison between EMAC and PSS MHP is the comparison between two models, and the comparison for UHP is highly indirect. I suggest that the authors add a column for ROOH in Table 2 and discuss it in the text. The comparison between observed and EMAC ROOH is shown in Figure 17 and shows a very large underestimate of EMAC (factor of 5-10?). A similar suggesting was made by the other reviewer and not addressed by the authors.

- Section 4.3.3 Longitudinal gradient. The EMAC model is a factor of 5-10 lower than observed ROOH, but for H2O2 is it only a factor of ~2 lower compared to observations as Figures 16 and 17 show. The authors do not discuss potential reasons for the more extreme degree of disagreement in ROOH, which I suggest that they add to the text.

- Lines 455-457 and Figure 19. It would be useful to include a panel comparison modeled and observed ROOH in Figure 19. It seems that removing scavenging in EMAC results in between agreement for H2O2, this begs the question as to whether it also leads to improved agreement with ROOH. Please address this.

- Lines 470-475 "Steady-state calculations for H2O2 and MHP based on observed precursors yield much lower values, in particular in the AMA" Lower values relative to what? Also, this is misleading as MHP is not measured while only H2O2 is measured. Finally, the authors are again very qualitative, please give a quantitative comparison between observed and PSS H2O2.

- Lines 477-480 “Convective injection of H_2O_2 (and MHP) into the upper troposphere over India most likely forms a pool of hydroperoxides in the upper troposphere that subsequently influences the western AMA, giving rise to a significant longitudinal gradient of H_2O_2 and MHP mixing ratios, with increasing values towards the center of the AMA. It is likely that at least a large part of UHP is due to additional MHP from an up-wind source.” I suggest replacing MHP with ROOH as ROOH is the only quantity that is measured. Also, the conclusions should reflect the fact that EMAC significantly underestimates ROOH, but that the underestimate in H_2O_2 is not quite as large. The conclusion lacks any mention of the sensitivity study results.