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Interactive Comment

Interactive comment on "Seasonal variability and long-term evolution of tropospheric composition in the tropics and Southern Hemisphere" *by* K. M. Wai and S. Wu

Anonymous Referee #2

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The authors have examined how current and future anthropogenic and biomass burning emissions influence tropospheric constituents, such as CO, O3, and OH in the tropics. Using the GEOS-Chem model, driven by meteorological fields from the GISS GCM, they have quantified how changes in emissions associated with the A1B emission scenario might impact tropospheric composition by 2050. There is a clear need for studies of this nature. However, I cannot recommend the manuscript for publication in this present form. As it is currently written, it is difficult to tell what experiments were conducted and, as a result, it is difficult to assess the interpretation of the results presented in the manuscript. There are also several places where statements are made

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without evidence to support the claims. For example, at the top of page 20025, they state that "the contribution of fossil fuel emissions is $\sim$10% of total O3 to the east of the continent (< 60E) over the Indian Ocean in the future," but it is not clear how they obtained this estimate. Similarly, in the penultimate sentence of the manuscript they state that the reduction in OH in the boundary layer "is due to the lack of OH sources to offset OH loss from increased of assumed CH4 and calculated CO in future." I am suspicious that CH4 and CO could be the cause of the decreased OH and no evidence was given to support the claim. The manuscript also needs significant editing to improve the grammar. I editing some of it, but stopped after page 20017. I think this is an interesting study that would be of interest to the community, but the manuscript needs to be better written to properly describe the work that was done. I encourage the authors to consider revising the manuscript to address my comments below.

General Comments

1. Page 20015, line 6: How are the MOPITT data used in the analysis? Care should be taken when using MOPITT version 4 (V4) for trend analyses. As noted in Worden et al. (Atmos. Chem. Phys., 13, 837–850, 2013), MOPITT V4 data have a positive bias as a result of the assumption in the retrieval algorithm that the instrument characteristics were constant. I would encourage the authors to use V5 MOPITT data instead.

2. Page 20021, line 12: How do you reconcile the positive trend in CO estimated at Ascension with the negative trend reported by Worden et al. (2013) at all latitudes in the northern and southern hemispheres? They found a weaker trend in the southern hemisphere than in the north, but it was negative. A concern with using the results of Fortems-Cheiney et al. (2011) to support the positive trend estimated at Ascension is that they used the biased V4 MOPITT data in their analysis.

3. Page 20024, lines 1-6. I don't understand this first sentence. It suggests that the reduction in biomass burning will be significantly offset by an increase in fossil fuel emissions, but according to Table 1 biomass burning will decrease by 8.3 Tg CO (38%)

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in Southern Africa, whereas the fossil fuel source will increase by 1.31 Tg CO. How did the authors conclude that the net reduction in CO emissions will be only 15%?

4. Page 20026, line 9: How did the authors estimate that "> 55%" of the ozone at each model level in both the present and future time slices is not affected by emissions from Southern Africa and Latin America? Did they run the model separately without biomass burning and lightning NOx emission to isolate their impact on ozone abundances? Based on the discussion at the end of page 20025 I believe that was done, but it is not clear. The authors need to better explain what sensitivity analyses were conducted.

5. In Figure 9 the ozone profile shows a sharp decrease across the tropopause, which is not physical. I assume this is due to the use of Synoz for stratospheric ozone? What is the impact of this on their analysis of the ozone budget? Because of the upward vertical motion across the tropical tropopause, such a strong decrease in ozone could make vertical transport across the tropopause a significant sink of upper tropospheric ozone, which would make it difficult to meaningfully interpret the results of the study. The authors should try to quantify this sink.

6. Page 20027, lines 11-16: I don't understand how increases in background CH4 and CO could produce the large localized changes in OH in the boundary layer as shown in Figure 10. Is this due to changes in biogenic emissions of shorter-lived gases, such as isoprene, as a result of changes in surface temperatures? I would like to see a more detailed analysis that better explains the OH response shown here.

Technical Comments

1. Page 20013, line 2: "total CO sources" should be "total CO source".

2. Page 20013, line 7: "regions than the northern" should be "regions than in the northern"

3. Page 20013, line 9: Change "more biomass burnings" to "more biomass burning".

4. Page 20013, line 14: Remove "the" before "tropospheric composition".

5. Page 20013, line 15: Add “the” before “ozone anomaly”.

6. Page 20014, line 1: Please restructure the sentence. Maybe along the lines of “global increase in emissions of ozone precursors, including emissions from biomass burning.”

7. Page 20014, line 3: Remove “alone” after climate change.

8. Page 20014, line 6: Remove “the” before “tropical composition”.

9. Page 20014, line 7: Change “on the impact” to “of the impacts”.

10. Page 20017, lines 13-14: Please rewrite the sentence starting with “It is evident by. . .” as “This is evident by the numerous fire events present in the MODIS fire map and by the elevated CO observed by MOPITT.”

11. Page 20017, line 20: Change “transports” to “transport”.

12. Page 20019, lines 25-26: It is not obvious to me from the information presented that there is more frequent deep convection in September than in August. Why not show the map for August as well?

13. Page 20020, line 15: Replace “circumscribed” with “circumnavigated” or “circled”.

14. Figures 6, 7, and 8 are too small. Why not plot the whole tropical region rather than just selected regions? It would be helpful for the reviewer to see the modeled response across the whole tropics and subtropics. Maybe show -180W to 180W and 50S to 30N.

15. Figure 9 is too small. Please make it larger so that the reader can actually see what is plotted.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 20011, 2013.