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Interactive comment on "Assessing the sensitivity of the hydroxyl radical to model biases in composition and temperature using a single-column photochemical model for Lauder, New Zealand" *by* L. López-Comí et al.

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We thank the reviewer for some useful and constructive comments. The reviewer's comments are repeated below in italics.

General Comments: This paper shows the influence of biases in modeled O_3*,* H_2O*, CO,* CH_4*, and temperature on modeled OH as investigated using a single-column model and observations over Lauder, New Zealand. Model fields of the parameters listed above are replaced with observations, and the photochemical single-column model is used to re-calculate OH and establish changes and sensitivities in OH relative to a reference run. Impacts of* O_3 *and temperature biases are further examined by separating kinetic and photolytic effects. Long-term OH trends and effects of clouds on OH are briefly examined.*

While this analysis is somewhat limited in scope and some aspects of the discussion are quite cursory, tropospheric OH is an important issue requiring varied and novel approaches to build on the community's understanding. With some revisions, this paper would contribute a useful method to help identify how model representation of OH can be improved and why model versus empirical estimates of the CH_4 *lifetime differ.*

We agree with the reviewer that novel approaches to understanding the diversity of OH in global models are needed; we think the approach presented here is such a novel approach. We also accept that the scope is limited, namely we only assessed OH at the Lauder observatory. Within the context of this paper this is not easily addressed. However, in a follow-up study, the approach could be rolled out to other locations albeit with caution due to issues with data availability, a necessary reliance on satellite remote sensing data, or the role of other pollutants not present above Lauder. This is dwelt on a little more in the final section of the text now. We hope the revised paper now addresses the reviewer's concern.

Major comments:

1. *Line 10: Please provide some quantification for these results. Particularly useful would be an indication of how much* H_2O *differed between the model and observations as well as a quantification of how OH changed in response. The same could be done for subsequent species.*

   A quantification of biases in the key forcings and also of the OH sensitivity have been included in the abstract.

2. *Line 47: You state two paragraphs above that "in-situ measurements of OH do not sufficiently constrain its global abundance." Here, you cite two Emmerson*

papers that do exactly that as justification for your SCM approach. I understand that constraining OH globally is not your aim, but the two statements still seem contradictory. It would be worthwhile to strengthen your justification for this analysis - what questions are you seeking to answer? What role can this approach play in constraining global OH, even if there are limitations?

The Emmerson et al. papers are about using a box model to understand measurements of OH and HO_2 taken in polluted environments. It would be incorrect to assert that these papers aim to quantify global OH. Our approach is unique in that we focus on the remote atmosphere and use long-term observations to constrain the model – other approaches such as Emmerson et al. have used campaign data. We have modified the text to this effect.

3. *Line 75: The number of species and reactions represented in the NIWA-UKCA chemical mechanism seems low, at least compared to explicit schemes like the MCM (easily into the hundreds of species and thousands of reactions). Might be worth noting why it's important to maintain consistency with the NIWA-UKCA model/why you wouldn't want a more detailed mechanism in your SCM, since "assessing fast photochemistry" is your goal.*

   The purpose of this work is to assess the contribution of NIWA-UKCA biases to OH. If we used a different chemical mechanism to the one used in the NIWA-UKCA model, we would be introducing more uncertainties in the analysis. Also this might make the analysis less relevant to chemistry-climate modelling in general. A sentence reporting on this has been included in the text. See paragraph 1 of section 2.1.

4. *Line 326: This section should be expanded. Even though the trends are not significant, they can still be quantified, and numbers here compared to values in the other studies you cite. Also, trends shown, for example, in Montzka et al., 2011 are derived from a globally, vertically integrated [OH] calculation, so*

separation into altitude bins, while useful, may not be the best comparison. I realize you don't seek to look at global [OH], but at least for this location, you could include a vertically integrated OH trend to compare to Montzka et al. In addition to quantifying the trend, you could also quantify the interannual variability.

We now state numerical values for the trends. We find no significant trend in the total column, which is in general agreement with a study on global OH by Montzka et al., however noting that we do not assess global OH. We now state the magnitude of interannual variability.

5. *Line 430: The reader is likely interested to hear your hypotheses on why NIWA-UKCA is too moist and O_3 is too high, even if further investigation is beyond the scope of this paper.*

   We appreciate that these are interesting though difficult questions. Difficulties with the hydrological cycle can be due to surface-atmosphere or cloud-atmosphere interactions at Lauder. The ozone biases could be partly caused by the water vapour biases. We have added two sentences to this effect.

   Minor comments:

6. *Line 11: Reference to O_3's kinetics and photolysis effects is unclear until defined in the body of the paper; please rephrase for abstract. Assertion that both are of similar magnitude does not seem well-supported, as pointed out above.*

   We have rephrased this in the abstract and now give numbers (in response to your earlier comment). These numbers do support the assertion that "both are of similar magnitude".

7. *Line 12: Sentence about OH being inversely related to CO and CH_4 is unnecessary for an audience familiar with OH.*

   We have removed this sentence.

8. *Line 19: Use of "less-than-additive" is vague, especially for an abstract. Instead of focusing on how the LWC and IC effects combine, it would probably be more informative to note the quantitative results of the combined LIC simulation, if that is the more realistic one. This would likely be of greater interest to the reader.*

   We have removed this sentence and now quantify the impact of clouds.

9. *Line 20: Please quantify trends as well, even if they are insignificant.*

   Done

10. *Line 101: use of word "produced" is unclear.*

    The phrase has been replaced by "total column ozone calculated by integrating the observed O_3 profiles".

11. *Line 115: You make the case for not trusting radiosonde H_2O data above 8 km, but how about the NIWA-UKCA output? Does modeled H_2O agree well with FPH? A figure addressing this point might be suited for supplemental material.*

    NIWA-UKCA H_2O is subject to biases also above 8 km. We simply do not have any observational data above this level that is of high enough quality and frequency and covers the study period. We therefore do not discuss impacts on OH above 8 km here. This is now made explicit in the text.

12. *Line 131: It would be helpful to address some anomalous behaviour in the H_2O profiles shown in Fig. 1: in the winter (presumably) of 1996, and to a lesser extent in other years, there are sudden high temperatures around 40-60 km – what's the cause of this? Is there evidence of this truly happening in the atmosphere or is it a result of interpolation, instrument artifact, etc?*

    These warming events may be the result of planetary wave breaking in the upper stratosphere. These stratospheric temperatures are from the NCEP/NCAR Reanalyses, and are based primarily on satellite measurements from TOVS (TIROS

Operational Vertical Sounder). We have no reason to question these measurements, but we have not studied these events as they are outside the scope of this paper. We have added a sentence to the text to this effect.

13. *Line 137: What are the native temporal and spatial resolutions of this simulation? Do you also interpolate spatially?*

    We use 10-daily instantaneous output fields by NIWA-UKCA to construct the interpolates. The native resolution of NIWA-UKCA is $3.75° \times 2.5°$. We do not interpolate spatially. We consider the closest grid point to Lauder position. We have modified the text (section 2.1) to this effect.

14. *Line 174: Make clear that you're discussing local O_3, or the "kinetics" effect.*

    We have modified the text to this effect.

15. *Line 177: The sentence "The largest impact is in the free troposphere where these differences vary with altitude." is a bit vague. Please be specific; what are the differences you're referring to, and how do they vary?*

    We have removed this sentence and added more detail to this discussion.

16. *Line 188: I'm not sure what you mean by the statement that kinetics and photolysis effects of the O_3 bias are comparable. Based on my interpretation of the contours in Fig. 2, the response of OH to kinetic effects is both positive and negative, depending on the month and ranges from -12 to $+4\%$; the response to photolysis effects is only positive, about $4-16\%$. The two effects somewhat cancel around Feb-June - is this what you're referring to? Please clarify.*

    We have qualified this statement. The magnitudes of the two effects are similar but seasonalities and height dependencies differ.

17. *Line 241: The statistic that sensitivity of OH to CH_4 changes peaks at $\sim 40\%$ can be easily misinterpreted as the OH response; it may be helpful to highlight both*

the max OH response as well as the OH sensitivity to avoid confusion.

We now consistently express relative changes of OH, O_3 etc. as percentages, and sensitivities such as this as fractions. We have added in the text that the maximum OH difference due to correcting CH_4 is 0.6%.

18. *Line 295: You stated above that the O'Connor et al. result may be due to cancellation of positive and negative temperature biases, but you show that temperatures at Lauder are cold-biased, so saying that your result of small impact of temp on OH corroborates that of O'Connor et al. seems like an apples-to-oranges comparison. I'd suggest reframing the discussion of O'Connor et al. – the small impact on OH in O'Connor et al. could have been due either to cancellation of temp biases or to low sensitivity of OH to temp changes, and your result suggests the latter? Or something to that effect.*

    We have followed the reviewer's suggestion.

19. *Line 320: Care to hypothesize about what might be causing these non-linearities?*

    Quite likely there are some feedbacks between correcting the water vapour and ozone biases which we now allude to in the text.

20. *Line 366: what do you mean by "slow chemistry"? My best guess is something like oxidation of CH_4 (long-lived), yet that is considered here, so I'm not sure about your intended meaning.*

    We supply independent initial states at every iteration of the SCM, meaning that species with a local chemical lifetime longer than the timestep of 1 hour can be considered prescribed for this purpose. Hence the model is only really suitable for fast radical chemistry. We have modified the text to this effect.

21. *Line 382: Again, would like to see quantification here; how much is the H_2O overestimated?*

    We now give this information in the text.

22. *Line 387: Please include some references, particularly when citing "accepted literature estimates".*

    Done

23. *Line 400: "...small reduction...due to the strong dependence of OH + CH_4 on temperature." This does not logically follow; you'd think, with a strong dependence, that you should see a large reduction. Please clarify.*

    It's been rephrased in paragraph 6 of the conclusions.

24. *Line 406: Thank you for quantifying the H_2O bias! I think this statistic would be better suited to earlier paragraph on H_2O, plus repeat in Section 3 and in abstract.*

    We have now added this information in the abstract and in section 3.

25. *Fig. 2: The use of both blue and red for strictly positive values is slightly confounding at first glance (panels (c)-(f)); if possible, would help to keep the white contour at value 0 (applies to various upcoming figures as well).*

    We have changed all such plots displaying the OH sensitivity to changes in the forcings, i.e. keeping the white contour at value 0, and blue and red colour levels for negative and positive values respectively.

26. *Table 1: Is the O_3 photolysis effect analysis done in an altitude-dependent manner? I.e., is a new j_{O1D} value calculated at each vertical point based on an overhead O_3 column that's adjusted to account for the strat column plus the partial tropospheric column overhead? I did not see any details regarding this in the text.*

    The j_{O1D} values are calculated at each altitude level, so it is taking into account the stratospheric contribution and the corresponding partial tropospheric column for each level. A sentence stating this has been added in the text. See section 2.1.

27. *Fig. 5: Use of* $\mathrm{d}\ln(OH)/\mathrm{d}\ln(H_2O)$ *is not mentioned in text, is not consistent with "*A_i*" terms in Fig. 4; please either justify switching metrics or maintain consistency (same with Fig. 6).*

    We have added eq. 3, which basically states an alternative, equivalent way of formulating A_i.

    Also we now consistently use percentages to indicate model biases (except for temperature) and fractions to indicate model sensitivities A_i.

28. *Fig. 9: y-axis label is misleading since, based on the caption, this shows OH anomalies. Perhaps include word "Anomaly" or a "delta" sign. Also, the values chosen for the y-axis tick marks are not easy to work with; it would be nice if they were adjusted to lie along round calculation-friendly values (e.g. increments of 0.5 instead of 0.417 in panel c). Also, how are these anomalies calculated, relative to what?*

    We have relabelled plot 9c. These are absolute differences in units of 10^5 molecules/cm^3.

    Technical corrections:

29. *Line 23: Use of word atmospher-e/-ic 3x*

    We have rephrased the sentence.

30. *Line 29: "plays a important" should be "plays an important"*

    Done

31. *Line 50: "long time series" wording seems off; perhaps "long record of observation" instead. I'm also curious at this point, how long is long? Perhaps give an earliest year of observation.*

    This has been changed to "long records of measurements are available since 1986". We use data since 1994 in this work as MOPI1 measurements started in 1994.

32. *Line 53: I think "Section 1" should be "Section 2"?*

    Corrected.

33. *Line 250: "altitide" should be "altitude"*

    Done

34. *Line 256: spelling of the word "assess" is incorrect*

    Done

35. *Line 275: use of "explicitly" does not add meaning to this sentence but makes it read awkwardly; I suggest removing.*

    Done

36. *Line 279: "nearly completely linearly" should be "nearly linearly"*

    Done

37. *Line 324: instead of "altitude bands", I more often see the phrase "altitude bins"*

    Done

38. *Line 378: use of word "effect" doesn't seem quite right; it's the bias you're correcting.*

    Done

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