

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.

Anonymous Referee #2

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Manuscript Number: acp-2016-448 Title: Assessing the sensitivity of the hydroxyl radical to model biases in composition and temperature using a single-column photochemical model for Lauder, New Zealand Authors: L. López-Comí et al.

General Comments: This paper shows the influence of biases in modeled O₃, H₂O, CO, CH₄, 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 rel-

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ative to a reference run. Impacts of O₃ 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 CH₄ lifetime differ.

Specific Comments:

Line 10: Please provide some quantification for these results. Particularly useful would be an indication of how much H₂O 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.

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?

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.

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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.

Line 430: The reader is likely interested to hear your hypotheses on why NIWA-UKCA is too moist and O₃ is too high, even if further investigation is beyond the scope of this paper.

Minor Comments:

Line 11: Reference to O₃'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

Line 12: Sentence about OH being inversely related to CO and CH₄ is unnecessary for an audience familiar with OH.

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.

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

Line 101: use of word "produced" is unclear

Line 115: You make the case for not trusting radiosonde H₂O data above 8 km, but how about the NIWA-UKCA output? Does modeled H₂O agree well with FPH? A figure addressing this point might be suited for supplemental material.

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Line 131: It would be helpful to address some anomalous behavior in the H₂O 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?

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

Line 174: Make clear that you're discussing local O₃, or the "kinetics" effect

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?

Line 188: I'm not sure what you mean by the statement that kinetics and photolysis effects of the O₃ 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.

Line 241: The statistic that sensitivity of OH to CH₄ changes peaks at ~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.

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

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temp changes, and your result suggests the latter? Or something to that effect.

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

Line 366: what do you mean by “slow chemistry”? My best guess is something like oxidation of CH₄ (long-lived), yet that is considered here, so I’m not sure your intended meaning.

Line 382: Again, would like to see quantification here; how much is the H₂O overestimated?

Line 387: Please include some references, particularly when citing “accepted literature estimates”

Line 400: “...small reduction . . . due to the strong dependence of OH+CH₄ on temperature.” This does not logically follow; you’d think, with a strong dependence, that you should see a large reduction. Please clarify.

Line 406: Thank you for quantifying the H₂O bias! I think this statistic would be better suited to earlier paragraph on H₂O, plus repeat in Section 3 and in abstract.

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).

Table 1: Is the O₃ photolysis effect analysis done in an altitude-dependent manner? I.e., is a new $j(\text{O1D})$ value calculated at each vertical point based on an overhead O₃ 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.

Fig. 5: Use of $d(\ln(\text{OH}))/d(\ln(\text{H}_2\text{O}))$ 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).

Fig. 9: y-axis label is misleading since, based on the caption, this shows OH anomaly

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lies. 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?

Technical Corrections:

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

Line 29: “plays a important” should be “plays an important”

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.

Line 53: I think “Section 1” should be “Section 2”?

Line 250: “altitide” should be “altitude”

Line 256: spelling of the word “assess” is incorrect

Line 275: use of “explicitly” does not add meaning to this sentence but makes it read awkwardly; I suggest removing

Line 279: “nearly completely linearly” should be “nearly linearly”

Line 324: instead of “altitude bands”, I more often see the phrase “altitude bins”

Line 378: use of word “effect” doesn’t seem quite right; it’s the bias you’re correcting

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-448, 2016.

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