Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC1
https://doi.org/10.5194/acp-2022-79-RC1, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on acp-2022-79

Anonymous Referee #1

Referee comment on "Investigating the global OH radical distribution using steady-state approximations and satellite data" by Matilda A. Pimlott et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-79-RC1, 2022

Pimlott et al use a steady state approximation for OH concentrations to develop a global OH product from satellite data. They use observations of CO, CH4, O3, and water vapor from the IASI satellite to calculate OH concentrations globally for the 600 – 700 hPa layer. To test the validity of their steady state approximation, they use comparisons from the TOMCAT model as well as observations from the four ATom campaign deployments, finding that the steady state approximation generally captures the ATom OH within observational uncertainty. They also compare OH calculated from satellite data to these ATom observations, and look at the drivers of trends in OH over the IASI time period. While this is an interesting idea, the authors do not spend enough time discussing the limitations of their approach, namely the effects of omitting key species from their approximation, most notably NO. In order to be suitable for publication, the authors need to address the comments outlined below.

General Comments:

While the authors do spend an appreciable amount of time evaluating the steady state approximation, I'm still left wondering how useful this is in regions that have appreciable OH production from NO. Buried in the supplement is a figure showing that in boreal winter, 2/3 or greater of OH production is from the NO + HO2 term for most zonal bands in the northern hemisphere. Omitting this from a steady state approximation would undoubtedly lead to incorrect OH values, or at best, correct OH values but for the wrong reason. In comparisons between ATom observations and both the ATom steady state OH and satellite steady state OH, there are multiple points where the steady state approximation dramatically underestimates the observations by a factor of 3 or greater. The reasons for these differences are not clearly articulated but are likely due to the omission of production terms. There is still value in this approach, however, if the authors more clearly show where secondary production from NO is important in the main text of the paper, and highlight regions where the approximation is likely not to hold.

Specific Comments:

Paragraph starting on Line 166: Is the simulation discussed in Monks et al (2017) the same as that discussed here? If not, are the two simulations close enough to have similar O3 and CO fields? Similarly, when you say "TOMCAT has a slightly higher global mean tropospheric OH…" are you talking about this simulation explicitly or the one discussed in Monks et al?

Line 207: Please indicate the sign of the bias (ie. The satellite is high by up to 20%). The current wording is ambiguous.

Figure S5: It would be helpful to have a legend on the figure itself indicating what color corresponds to which observation, instead of just having this information in the figure caption.

Line 221: Is this just the global average? It looks like there could be significant variation in this value, but it is hard to judge that from comparing Figures 2 and S6. A map showing the percent uncertainty, instead of the absolute value, might be more useful.

Section 2.3.3: What's the time resolution of the ATom observations?

Line 248: I think you need a more thorough comparison between the TOMCAT OH and the SS approximation. Just looking at zonal means is likely obscuring regional effects, particularly because NO contributions to OH production are likely to be more important over land than over the remote ocean, even at 600 – 700 hPa. Some of this can be discerned from Figure 3, but there should be more discussion about the regional differences in agreement. Figures showing the absolute or relative difference between TOMCAT and the SS approximation would be appropriate as would a regression.

Table 1 and throughout the text: Are you using mass-weighted OH when you're making your global comparisons? If not, you should be, otherwise you're likely giving too much importance to regions that don't particularly matter.

Line 287: How accurate is your model JO1D? CAM-Chem, for example, has a notable low bias in JO1D in the altitude range you're examining (Nicely, et al, 2016). In your uncertainty analysis, you assume there is no error in JO1D, but that is highly unlikely. If you don't know how accurate the modeled JO1D is, you should add a sentence or too at least noting that this is a potential source of error.

Line 305: What's the r^2 value for a regression of the satellite and model OH?

Line 309: You're missing a period after "18%".

Line 311: Is this missing peak in North America likely due to the omission of a NOx term in your SS approximation? I think either here or elsewhere, there needs to be a more explicit discussion of how omitting NO and VOC sinks likely limits the accuracy of your satellite SS product in certain regions. Maps showing the relative importance of NO to OH production and the other VOCs to OH loss could help illustrate where this product will likely have more limitations, or bringing a subset of the panels from Figures S9 and 10 to a main figure could be helpful.

Line 325: More discussion of why agreement is significantly degraded for ATom 2 as compared to the other campaigns is needed. Also, for each of the campaigns there is almost a second trend line, where observed OH ranges up to 10 x 10e6 molecules/cm3 but the SS approximation doesn't exceed 1. What is driving the poor agreement for these points? Does agreement improve if you include an NO term in the SS approximation?

Line 359: What is the horizontal extent of the OH observations and how does this compare to the satellite product resolution? Is the horizontally homogeneity of OH enough to allow for a comparison to a satellite product at 3 degree resolution?

Figure 7: This figure highlights the poor performance in the 30 – 90 N range. Figure S9 likely suggests part of the reason, since, according to TOMCAT, greater than 2/3 of the OH production is from the NO + HO2 reaction. Again, more discussion is needed as to how this limits the applicability of your product to regions with appreciable secondary OH production from NO.

Line 391: How much does the stratospheric O3 column in your model vary between 2008 and 2017? How would any trends or internal variability affect your JO1D and consequently your OH calculation?

References:

Nicely, J. M., et al. (2016). "An observationally constrained evaluation of the oxidative capacity in the tropical western Pacific troposphere." Journal of Geophysical Research: Atmospheres **121**: 7461-7488.