Atmospheric Chemistry and Physics Discussions

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Insufficient credit to other efforts

Anonymous Referee #1

Referee comment on "Technical note: Constraining the hydroxyl (OH) radical in the tropics with satellite observations of its drivers – first steps toward assessing the feasibility of a global observation strategy" by Daniel C. Anderson et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-763-RC1, 2023

With the advance of satellite instruments and machine learning techniques, atmospheric chemistry research is developing fast. This paper does an attempt to build a machine learning method for total (tropospheric) column OH, based on satellite observations. To this end, they train a GBRT model on results of a chemistry transport model and apply this model to satellite observations from mainly MOPITT, OMI, AIRS, but also location (i.e. solar intensity). The basic idea is that OH in the remote atmosphere in the tropics is driven mainly by the abundance of O_3, H_2O, NO_x, CO, and hydrocarbons. In that respect, this provides an original contribution and shows some promises for the future.

The main problem I have with the paper is that is insufficiently credits and discusses other developments in the field. Reading the paper, I was wondering if the authors are aware of these developments at all? In a fast advancing field, reading, referencing, and discussing the work of others is of utmost importance. And the paper fails to do this. References to own work dominate. Below I outline how the paper should improve to become acceptable for publication.

- General methodology

In studying OH in the remote atmosphere, we have to rely on knowledge on atmospheric

chemistry. In this paper, the authors use results of atmospheric chemistry simulations to train a machine learning algorithm. No criticism here. In the discussion, however, they "come up" with the idea to reduce the uncertainties between the 3D model results and satellite observations (without any references). Long-standing efforts have been made to "merge" satellite information and models in a process called data assimilation. First, there is the idea of chemical data assimilation, performed in e.g. the EU Copernicus services (e.g. https://atmosphere.copernicus.eu/sites/default/files/custom-uploads/3rd-joint-training/ACT2021_AInness.pdf)

Second, some authors worked their whole life on the subject of OH, satellites, and models, and do not receive even a citation in the manuscript (e.g. https://acp.copernicus.org/articles/20/931/2020/). I am not claiming that the work in this paper is useless. What I am saying is that the added value could be much more when proper credit and discussion is dedicated to related studies.

In the revised manuscript, the authors should catch up with existing work and should discuss that in the introduction and discussion. This should replace the current self-centered manuscript with restricted references to work of other groups.

- NO2 satellite data

More or less along the same lines. NO_2 abundance in the remote tropics appears to be very important in determining TCOH. In section 5, the authors attempt to use alternative satellite products. In their evaluation of the results, they systematically refer to differences with their product as 'biases'. Although they evaluated to some extend their TCOH product against Atom data, with relative OK result, this does not imply that their product is OK in May 2018 (the analyzed month) and that all the other results are biased. On top of that, they fail to refer to an extensive (EU-funded) program QA4eCV in which the NO_2 products (e.g. of OMI) have been evaluated (e.g. https://amt.copernicus.org/articles/11/6651/2018/). This effort is so central to the discussion, that it really shameful that relevant literature is not cited.

I found the paper a pleasant read, presenting an interesting view for future exploration. In that respect, publication is possible, but the paper should discuss and give credit to internationally well-established efforts, which would require a major overhaul of the introduction and discussion.

Some other comments on the paper are in the annotated pdf file.

Please also note the supplement to this comment:
https://acp.copernicus.org/preprints/acp-2022-763/acp-2022-763-RC1-supplement.pdf