

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2021-381

Anonymous Referee #2

Referee comment on "Direct estimates of biomass burning NO_x emissions and lifetimes using daily observations from TROPOMI" by Xiaomeng Jin et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-381-RC2>, 2021

This paper estimates biomass burning NO_x emissions and lifetime using daily observations from TROPOMI. The topic has a broad interest, and the investigation is solid. I particularly appreciate the validation using a plume model. The work suggests decreasing NO_x lifetime with fire intensity due to the increase in both NO_x abundance and hydroxyl radical production. I would recommend minor revision before publication.

General comments:

- section 3.1 profile correction using GEOS-CF: How sensitivity the derived lifetime and emissions to this correction?
- section 3.2. It is not very clear to me how ALH is related to the EMG approach. I suppose the authors indicate a consistent wind layer height and injection height. If so, I would suggest making this clearer in the text. Are the derived results from the EMG approach very sensitive to the choice of wind layer heights? Additional sensitivity analysis would be beneficial to the study.
- section 3.4. "pixels are grouped to separate plumes based on their connections with surrounding pixels" I recommend a diagram or plot here to illustrate the grouping algorithm. It is not easy for me to get it from the text here.
- section 4.5. I'm surprised to see the results for the wind speed less than 2 m/s. As far as my understanding, the EMG function is not suitable for the cases of calm winds. Is there any special reason for applying EMG for calm-wind conditions?

Specific comments:

- Page 2, line 49. Please add reference for “the improved signal-to-noise ratio”. I would suggest more details about the improved signal-to-noise ratio to justify the usage of daily observation. For example, how does the ratio improve from OMI to TROPOMI? How does one TROPOMI observation compare to several OMI observations? It is not necessary to discuss this in the abstract, but somewhere in the main text would be appreciated.
- Page 6, line 180. Please add reference for PECANS. Additionally, please clarify the reasons for the settings in the model, such as the diffusion coefficients and O₃ concentrations.
- line 227. Please clarify the details of the 50 initial conditions.
- line 305. Does Mebust and Cohen (2014) adopt a similar method as this study? If not, I would suggest rephrasing this part by mentioning the results using standard TROPOMI products firstly and then comparing with that of Mebust and Cohen (2014). Otherwise, the readers may get confused here.
- section 5.3. please clarify the calculation of chemical lifetime.