

# ***Interactive comment on “Assessing and improving cloud-height based parameterisations of global lightning flash rate, and their impact on lightning-produced NO<sub>x</sub> and tropospheric composition” by Ashok K. Luhar et al.***

## **Anonymous Referee #1**

Received and published: 6 October 2020

General Comments: Well written manuscript that is nearly ready for publication.

Scientific Comments:

Abstract reads well. My only quibble is that PR92's deficiencies over water have been well known for years. Perhaps you should go with ... via the Price and Rind (1992) (PR92) formulation, whose water parameterization is known to greatly underestimate flashes.

P5 L19: Be clear that the  $k$  referred to in line 19 is the same as the one referred to

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in equation (2). This could be done by removing the line 19 bullet and including this sentence in the preceding paragraph.

P6L23: What is the justification for parameterizing the thickness of the cold cloud region in terms of latitude when you could use temperature profiles from the driving model? What errors are induced by assuming a simple dependence on latitude for the cold-cloud region?

P12L25: How different would NO and NL be if you used the same value of  $k_1$  for land and ocean points, i.e., would the difference and values be comparable to that shown in Michalon.

P13 L5: When specifying flash rates, is continental-marine synonymous with land-ocean or is the convection in grid boxes within  $x$  km of the coast classified as continental in character? Or to put it another way: Should there be a transition zone over the ocean where flashes are still parameterized as if they are continental in character?

P16 L10-11 How sensitive is the conclusion (that Run 2 does the best) to the choice of meteorological model? Or to put it another way, how likely is it that the Michalon approach would do better if the cloud top heights were taken from a different model?

P18 L21: What do you mean by “significant spatial differences”? High-biases? Low-biases?

P23 L17: What is your rationale for distributing IC and CG emissions as you do? Is it motivated by the results of Pickering, Ott. Other? Wouldn't sub-grid scale mixing lead to some overlap in altitude between where NO from IC and CG flashes is deposited? How did you choose 500 hPa as the dividing line?

P28 L13: Several of the sites you selected are in regions where the impact of NO<sub>x</sub> from lightning is minimal. Please add a comparison or two to profiles from locations within the SHADOZ network that may be more affected by lightning-NO<sub>x</sub>. You could use boundary layer values from the profiles if you wish to retain your focus on the surface.

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P28L15: Is LNO<sub>x</sub> an important source of NO<sub>x</sub> at any of these sites? Are differences between the models larger in the winter or summer? I would assume the latter; however, it isn't clear at Ushuaia for e.g..

P33 L3: You should state that the LNO<sub>x</sub>-induced increase in OH due to the new scheme exacerbates the model high bias in OH burden. – but a softer adjective than “exacerbate” is fine.

P36 L6: Would it make sense to give the locations of all of the data sets here as opposed to referring back to the manuscript or the acknowledgements section.

Technical Details:

P2L11 NO and NO<sub>2</sub> have already been defined.

P5L18 mb — hPa

P6 L12: as a function of and H — as a function of H

P6 L14: (discusses later) — (discussed later)

P10 L10: when used in global models — when used over water in global models

P28 L8: global ozone — global annual mean ozone

P31 L1 The three “by”s are not necessary and can be removed.

P32 L8: At mid-troposphere — In the mid-troposphere

P34 L2 The NMSE and r values suggest a mixed result. — Be specific as to what you mean here.

P36 L1 “perhaps currently not well constrained” — poorly constrained

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