

Geosci. Model Dev. Discuss., referee comment RC1
<https://doi.org/10.5194/gmd-2021-107-RC1>, 2021
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Comment on gmd-2021-107

Anonymous Referee #1

Referee comment on "Iodine chemistry in the chemistry–climate model SOCOL-AERv2-I" by Arseniy Karagodin-Doyennel et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-107-RC1>, 2021

This manuscript presents the implementation of iodine chemistry into the chemistry–climate model SOCOL-AERv2. The paper shows the importance of iodine chemistry in atmospheric and climate models. I would recommend publication after addressing the comments below.

- I would suggest the authors provide more information for their simulation results in Section 3. I think at least a map for I_y or IO should be provided in a similar way as Fig.4. Based on that, a detail explanation of how model mechanisms result in the simulated distribution could be given. Such information can largely help the analysis in later sections.
- Section 2.2, figures for emission fluxes could be helpful, maybe can be shown in supplements.
- I think the introduction is way too long. As a manuscript for model development, it spends nearly two pages to introduce atmospheric iodine. I would suggest shorten the introduction and refer to the references for the details.
- Line 170, is the 50% underestimation similar everywhere? Will applying the scale factor make the distribution of the emissions different from other models?
- Line 241, any reference or potential evidence for this assumption?
- Will the sea salt alkalinity affect the uptake of iodine species?

- Section 2.2, it seems that the model doesn't include reactive uptake of iodine in liquid clouds. Is this because there is not enough information available for the model parameterization? Maybe make it clearer in the text.

- Figure 1, maybe make the line thicker and the shadings lighter, or use the logscale. It is very hard to see the values in the lower levels.

- Line 355-362, this part needs to be explained clearer. The authors referenced a few evidences to show the contribution of cross cycles, but didn't point out whether those evidences were proved in their own model simulation. Maybe provide some numbers to describe the contributions of sole iodine cycles and cross cycles on ozone loss would help.

- Line 372, similar as above, the authors referenced Daniel et al. (1999) for the explanation, but did the model simulation in SOCOL show the same?