

Interactive comment on “Holocene atmospheric iodine evolution over the North Atlantic” by Juan Pablo Corella et al.

Anonymous Referee #3

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In the paper “Holocene atmospheric iodine evolution over the North Atlantic” by J. P. Corella and co-authors a new record of iodine concentration in the ReCAP ice core (Greenland) is presented. The record covers the full Holocene and an interesting discussion about iodine variations in the atmosphere across different climatic periods is shown. In particular the authors elaborate here an easy conceptual model which seems to be very effective in explaining iodine variability during the three time periods here considered: Holocene Thermal Maximum, Neoglacial period and Great Acceleration. The paper shows a very good laboratory intercomparison exercise, showing a general agreement in the measured iodine concentrations. This work is generally well presented and well written and will surely be a milestone in the use of halogens measured in ice cores to achieve information about Sea Ice in the North Atlantic. My main

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concern about the paper is the lack of discussion about the possible post-depositional effects of iodine species in the snow. A detailed discussion about the preservation of iodine compounds in the snow and their stability in time due to a possible photochemical degradation should be added to the text. Before concluding that iodine is a real marker reflecting the past North Atlantic conditions, the authors should assess that the very low concentrations they are measuring are really telling us a story about past conditions, or, at least, that the record obtained is not significantly different from what was deposited in the Renland Ice Cap. Moreover, the meteorological conditions were surely very different in the different time periods here shown, and the authors should clarify why the variability of iodine is mainly linked to the primary productivity and not to changes in transport efficiency through time. Other papers by the authors reported an halogen enrichment factor by subtracting the sea salt contribution. I'm not sure about which is the best way to proceed, but I would like to know why in this paper this elaboration was not considered. I suspect that an elaboration of the iodine values with sodium can infer to iodine a variability which mainly comes from Na⁺ concentrations. Once this points will be addressed I would recommend the publication of this paper.

Minor issues.

Line 59: Change to "Ice core..." Line 109: explain what UPW means. Line 110: How were the d.I. calculated? Explain briefly how the two labs measured this figure of merit of the two methods. Line 116: Is there an age model for the RECAP ice core? Give more information on this point: how was the age scale built? Which absolute markers were used (i.e. volcanoes, tephra, etc)? Which is the uncertainty in the bottom part of the ice core? Line 137-138: units are not readable... Line 141: remove the comma between m-2 and yr-1. Line 198: Two references by Telesinski et al. were published in 2014, please call them a) and b) in order to make clear which reference you are referring to in the text. Line 198: is c. for ca.? Line 240: remove M.M. before Telesinski. Line 412: check the references: something wrong happened to the title of this paper. Figure 2 caption: use "concentration and flux" or "concentrations and fluxes", not a mix.

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Figure 2: during the HTM the authors' reconstruction seems to be very smoothed with respect to the marine records. Is it due to the poor resolution at this depth? Please discuss this point in the text. Figure 2 and text: is flux adding something new with respect to concentration? The two profiles are very similar and the authors should discuss this point in terms of wet/dry deposition mechanisms at the drilling site.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2019-71>, 2019.