In this paper by N. Maffezzoli and coauthors a new record of Bromine concentration measured along the Renland ice core is presented. As already observed by the other reviewers, the authors consider Br enr as a proxy of FYSI, but this is still to be proved. With respect to the original version of the paper, the revised one clarify this point and the “certain assertions” have been changed and transformed in “probable conclusions”. I appreciate this change all along the manuscript but, since Bromine is thought to be a marker just of the FYSI, I would suggest to change the title in “120,000 year record of First-Year Sea Ice in the North Atlantic ?”.

The paper is well presented and written and the discussed dataset seems to be robust in terms of accuracy and precision, given the good inter-laboratory exercise here described.

I have two major concerns about the use of Br enr as a proxy to reconstruct sea ice (or FYSI):

1) Bromine in the snow and ice has been deposited through dry and (mainly, in this case) wet depositions. How stable is this element once is deposited in the snow? Assuming that bromine concentration is really dependent by FYSI only, is the recorded climatic signal well preserved in time? This point is just briefly discussed in terms of “possible photolytic reemission of bromine”, but this phenomenon is strongly dependent on the accumulation rate and I expect a high year to year accumulation variability in the RECAP core, due to its proximity to the ocean. Furthermore, this problem can be amplified when looking at larger time scales (glacial/interglacial) as done in this paper. I think that this point should be described and discussed in more details.

2) The calculation of Br enr could represent a caveat of the method. The “correction” of the Br concentration using Na+ concentration could produce an artefact in the Br enr profile: the higher the Na+, the lower the Br enr. Being Br part of SSA, we can observe also in this paper an agreement (not always good) between ssNa and Br profiles. To better clarify this point: in the last years ssNa+ has been used as a potential proxy of sea-ice extent (Iizuka et al., 2008, JGR, Severi et al., 2017, Chemosphere) and its use to calculate an enrichment factor for Bromine, could transfer the information hidden in the Na+ profile to the Br enr one. The proof of this is clearly visible comparing the ssNa profile with the Br enr one (they are clearly anti-correlated, see figure below). I think that both markers are reflecting changes in the sea ice conditions and a correlation between the two would help understanding this point. If the correlation between the two is good enough I think that Br concentration (or flux) itself would be a reliable marker without any correction or enrichment factor calculation. For example, in the paper by the same authors’ team using Iodine

(Corella et al., 2019), they did not make any correction and used directly the I concentration. Also this aspect should be discussed along the main text and a correlation plot between ssNa and Br should be added (in the SI would be fine as well).

Minor changes:
Page 8 line 7: the values 0.36 and 0.54 are not so close as the authors say (it's 50% higher!). Thus, change this sentence.

Simonsen et al, Nat.Comm is now published and should be added to the references list.