

This paper presents a new 120,000 year record of Br measured in the Renland ice core. It then interprets this in terms of sea ice extent. The analytical record is an interesting and potentially important one, and the paper should eventually become publishable in Climate of the Past. It is written in a clear style. However there are a number of issues that need to be addressed before it can be published. These fall principally into three areas, the meaning of Br_enr, dating of the ice, and the transformation the authors adopt.

1. The idea that some measure of Br enrichment may be associated with mainly first year sea ice (FYSI) is plausible and the authors have, in previous papers, made some kind of argument for it. However it is far from established and the paper is far too definite about that. It needs to spell out the caveats, and use the word “suggest”, “might” and “may” a lot more. I would also argue to add a question mark to the title.

The idea that Br_enr is a FYSI proxy relies on a number of assumptions.

- (a) The Br activation process leads to production of activated Br and to a depletion of Br in the salty surface on which it happens. As a result there can be both enhancements and depletions in what is deposited as snowfall, depending on the relative importance of depleted sea salt aerosol and gas-phase Br compounds that eventually get transported as (presumably) HBr. The implicit assumption used here is that the gas phase Br is transported to (in this case) Renland much more efficiently than the depleted sea salt aerosol. I agree with this, which is consistent with eg Simpson et al 2004. However the assumption needs to be stated, especially as this group has been inconsistent on that question, with Spolaor et al (2013), the first paper on this proxy, asserting exactly the opposite.
- (b) There is also an assumption that HBr is produced only by the photochemical Br activation and not by a reaction between salt and acid analogous to that described on page 3, line 10 for Br. Again, the weak Cl fractionation suggests this may be true, but it needs stating.
- (c) The paper assumes that Br activation takes place only on FYSI and not at all on MYSI. This is highly unlikely to be true, and the production ratio between these two forms of sea ice will be crucial to the interpretation. This is also the case for sea salt aerosol itself as shown in a modelling study by Rhodes et al (2018), where the use of sea salt as a sea ice proxy is crucially dependent on the extent to which FY and MYSI are involved in aerosol production. I don't expect a full modelling study here, but again it needs to be acknowledged that production on MYSI would affect the interpretation.
- (d) Finally the authors essentially assume that their untransformed proxy equates to FYSI, taking no account of transport distance. It's actually quite plausible that their proxy decreases in very cold periods not because there is less FYSI but because it's much further from Renland. This may still argue for a similar interpretation to the data, but is a more subtle mechanistic point that will eventually need to be tested in models.

I therefore ask the authors to state their assumptions and the caveats more fully, and I will suggest some places in the text where the interpretation should be more tentative.

2. This seems to be the first paper in which RECAP data have been presented covering the glacial, and is therefore the one in which the age model is effectively first presented. Dating Renland is far from simple, and it is completely unacceptable to rely on a reference to an in preparation paper (Simonsen) which the reader has no sight of. If this is the first paper

published then this one needs to demonstrate that the dating is plausible – in the appendix or SI I would expect at minimum an age-depth profile, a table of tie points, and an estimate of age uncertainty. Without that, the reader has no idea how robust the comparisons to the NGRIP isotope record are (ideally we would see the Renland water isotope record in this paper but I appreciate that must be the subject of another paper and that this paper is out of sequence). In the absence of a substantial dating appendix/SI, this paper should not be published until a full description of the dating has been published in another paper.

3. While it is a clever idea, I am not convinced by the value of the transformation. The paper claims that this “linearizes” the record, but how does it do that? The number that is achieved has not in any sense been shown to be linear with any climate variable, be it FYSI extent, total sea ice extent or anything else. What has been done is to give a spurious air of quantitativeness to a proxy that remains at present qualitative. In Fig 5 the panel where the data are colour coded red or blue is good, and I understand the temptation to try to turn it into a curve that can be compared with the IP25 record in Figure 6 but I feel it should be resisted. If the authors cannot resist producing something I suggest a different alternative:
(a) Have the y-axis go up to 7.4 (or whatever the change point is considered to be) and then decreases back to zero again, and plot the actual Br_enr value either above or below the line, maintaining its colour coding . (Mathematically this is achieved by making eq5 simply $Br_enr=-Br_enr$, plotting it with axis reversed and using the absolute value of Br_enr as the axis label). To indicate the uncertainty about the T threshold however, you should plot both the actual and the transformed Br_enr values for data within 2 sigma of the chosen temperature threshold so that it is obvious that there are two possible states at some depths. This procedure will produce a curve similar to the one in Figure 6 but without producing spurious new numbers.

More detailed comments

Page 1, abstract, needs to be more tentative: line 3 I suggest “and tentatively reconstruct”; line 6 “what we interpret as the transition from MYSO to FYSI started at 17.6 kyr”; line 8 “our proxy interpreted as FYSI reached its maximum”; line 10 “sea ice extent was probably greatest”.

Page 2, line 1 “reservoirs”

Page 3, equations 2-3 and surrounding text. I appreciate that correcting for crustal Na in Greenland is not straightforward, but you need to explain what your rationale is here better, and when you do, it doesn’t completely make sense. You are implicitly assuming that when the salt/acid reaction has created excess Cl, there is no terrestrial Na (eq 3); and that if there is a Cl/Na ratio less than 1.8 that is entirely because of terrestrial Na, and not at all because of the salt/acid reaction leading to removal of Cl from aerosol. I am not sure why either of those assumptions should be correct. Normally one would use a crustal element such as Al or perhaps Ca to put limits on the terrestrial correction. In the absence of that you need to revisit your method and at minimum state its limitations and suggest how much it may affect your results.

Page 3, line 15-20. Thank you for using the word “suggesting” here. However I am not sure what point you are making in line 19-20. Why would we expect Holocene-like ice extent at 120 kyr when we are already well into the glacial inception?

Page 3, line 26-28. The modelling study of Rhodes et al (Rhodes, R. H. et al., GRL, 45(11), 5572-5580, doi:10.1029/2018gl077403) doesn't really support your interpretation, as in present conditions, it says that both sites are overwhelmingly seeing OW conditions (Rhodes was looking at the influence of sea salt aerosol but I imagine modelling Br would produce a similar result). Comment? The difference between Holocene values for the two sites could also be partly related to the fact that the Br_enr measure is not really ideal because it does not measure the amount of Br reaching the site but rather the ratio of Br/sea salt aerosol. NGRIP of course receives much less sea salt than Renland so just a small amount of gas phase Br can induce a Br_enr >>1. This may not fully explain the difference between the sites but it is a factor. It's too late now because this measure is embedded in the literature, but the use of Br_excess ($[Br-Br_{seawater}]$ instead of $[Br/Br_{seawater}]$) would have avoided this problem.

Page 4, line 8 "a number of"

Page 6, last line "Our reconstruction suggests.."

Figure 2. I appreciate that Kindler only goes to 10 kyr but it is unhelpful that we don't see a climate record from 10-0 kyr. Can you infill with an ^{18}O record to at least indicate to the reader that Holocene temperatures remain warm.

Fig 3 caption, Fig 6 caption: Tzedakis et al is not an appropriate reference for orbital parameters. Please cite original papers by either Berger or Laskar.

Appendix B. I am wondering why you call this "sea salt aerosol source area" when what you are interested in is the source of the gas phase Br. The back trajectories are for air masses so certainly not specific to aerosol.

Fig B1 caption line 2 "constraint" (but can you explain what you mean by this constraint).

Fig B1 caption. Sorry for my ignorance but I never heard of a nabla before, can't you call it an inverted triangle?