The Cryosphere Discussions

The Cryosphere Discuss., author comment AC2
https://doi.org/10.5194/tc-2022-139-AC2, 2022

Reply on RC2

François Burgay et al.

Author comment on "200-year ice core bromine reconstruction at Dome C (Antarctica): observational and modelling results" by François Burgay et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-139-AC2, 2022

Dear editor,

We thank the reviewers for their work and suggestions.

Below you will find the reviewers' concerns in bold and our answers as plain text.

Best regards

Andrea Spolaor and Francois Burgay on behalf of the co-authors

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REVIEWER #2

This manuscript concerns the investigation of a shallow firn core drilled in East Antarctica to address the behavior of Br at a low snow accumulation site, evaluating its resistance to post-depositional processes. The manuscript is well written and clear and provides important novelties in the field of halogen glaciochemistry and in the direction of the development of new proxies to reconstruct first year sea ice extension in Antarctica. I am not an expert in photochemistry so I can't really comment on the part dedicated to the different contributions of the sources for the hydroxyl radical in inner East Antarctica, but from what I see the authors made a nice job trying to take into consideration all the relevant aspects and hypotheses. My only real concern is about the importance given to the discussion that tries to link Br records and volcanic activities.

We are thankful to the reviewer for their feedback on our manuscript and on their comments/suggestions.

I think that the attempt to discuss your results in relation with volcanic eruptions is not well constrained. I suggest you to remove it or to drastically shorten it. As the authors say it is true that the ice core they investigated ideally contains the traces of a few known volcanic eruptions which had a global impact in the last 200 years, namely Pinatubo, Tambora, Krakatua and others. As the author do not find any anomaly in their Br and Brenr records in this time interval,

they argue that volcanic aerosol, in particular acidic species, do not affect the preservation of Br in snow at Dome C. This line of reasoning is definitely interesting but in my opinion the authors are going a little bit too far with speculation here. At first they did not measure any proxy for volcanic eruptions, secondarily if we consider past research on recent volcanic eruptions recorded at Dome C (see for example Castellano et al., 2005, JGR 110:D06114), we see that eruptions occurred in the last 200 years produced glaciochemical signals that are not among the most intense recorded at Dome C. Personally I found the paragraph dedicated to this discussion a little bit overstating. You could consider to drastically shorten this part. About the fact that in West Antarctica there are some evidence of volcanic-related interferences in Br preservation: this is probably related to the fact the event considered in the cited paper was a local Antarctic one, producing a local glaciochemical impact on snow properties that was much stronger that the one produced by eruptions that despite being of global relevance had only limited impact on remote areas such as Dome C. This is definitely an interesting topic to investigate but I feel that in its current state this is not the right study for this. What you can tell here is that from your evidence you can say that in the considered record volcanic eruptions are not impacting Br-related records, but being the considered time interval short and the site notably peculiar, further studies are needed to better investigate this.

This is the only relevant issue that I have noted in this manuscript, once the authors have adjusted this, I will recommend its publication.

We thank the reviewer for this punctual comment and we agree on their suggestion. We edited the text accordingly to highlight that our findings need to be corroborated by other studies. We edited the 3.3 section accordingly (L382-392):

Nevertheless, we highlight that, due to low snow accumulation and strong wind erosion, not all the volcanic eruptions listed above might be present in our record. Indeed, a previous investigation that compared the sulphate signal from five ice cores drilled 1 meter apart from each other at Dome C showed a bulk probability of 30% of missing volcanic events when a single core is used as the site reference (Gautier et al., 2016). Among the volcanic events embraced by our record, only Krakatua, Cosiguina and UE 1809 were observed in all the previously mentioned five replicate cores, giving us confidence that for these eruptions the volcanic fingerprint is present also in our record. However, we acknowledge that a proxy-based volcanic reconstruction is missing for our core and, considering the strong spatial variability observed at Dome C, further and more specific studies are needed to investigate the impact of large inter-hemispherical volcanic eruptions on the preservation of bromine in the snowpack.

Below a few specific comments

Below we provide specific responses to each of the reviewer individual comments

Line60:maybe better "inversely" than "linearly"?

Done

Line103: why snow accumulation of the 2006-2013 interval is missing the standard deviation?

We re-checked the data and we changed some of the snow accumulation values with more updated ones (we added a new reference).

Frezzotti M, Scarchilli C, Becagli S, Proposito M, Urbini S. A synthesis of the Antarctic

surface mass balance during the last 800 yr. The Cryosphere. 2013 Feb 20;7(1):303-19.

Unfortunately, the standard deviation is not provided in neither of the cited manuscript. However, the evolution of snow accumulation (in cm) is provided in Genthon et al., 2016. The conversion in kg m^{-2} yr^{-1} is done using an average surface snow density of 320 kg m^{-3}.

Genthon, C., Six, D., Scarchilli, C., Ciardini, V., and Frezzotti, M.: Meteorological and snow accumulation gradients across Dome C, East Antarctic plateau, International Journal of Climatology, 36, 455-466, 2016.

Line116: it is not common to perform ICP-MS analyses on samples that were not acidified, as this is uncommon, could you spend some words to better explain and justify this choice? I am also asking if the same treatment was followed for standards

Usually the acidification of molten ice/snow samples is performed when we want to dissolve particle-bound elements, such as iron (e.g. Burgay et al., 2021). In the case of this study, we did not acidify the samples because we were only interested in the water-soluble Br and Na fractions. Consistently, also the standards were not acidified and the overall procedure (cleaning of vials, etc…) did not use any acid. Lastly, the acidification of the samples might led to analyte loss when halogens are targeted (Flores et al., 2022). We add additional text at L114-115 and L125-126 for clarification.

Burgay, F., Spolaor, A., Gabrieli, J., Cozzi, G., Turetta, C., Vallelonga, P., & Barbante, C. (2021). Atmospheric iron supply and marine productivity in the glacial North Pacific Ocean. *Climate of the Past*, *17*(1), 491-505.

Flores, E. M., Mello, P. A., Krzyzaniak, S. R., Cauduro, V. H., & Picoloto, R. S. (2020). Challenges and trends for halogen determination by inductively coupled plasma mass spectrometry: a review. *Rapid Communications in Mass Spectrometry*, *34*, e8727.

Line225-228: as this is not something coming from this manuscript, I would expect to find some references here

The statements presented from L225 to L228 refers to Vallelonga et al., 2021, which is now explicitly cited in the text.

Line263-165: could you provide a rough estimate of this increase? Just to better understanding the intensity of the change

The UV-forcing roughly increase 10 times (see Figure 1 in Spolaor et al., 2021). We add this value (and associated reference) at L265.

Spolaor, A., Burgay, F., Fernandez, R. P., Turetta, C., Cuevas, C. A., Kim, K., ... & Saiz-Lopez, A. (2021). Antarctic ozone hole modifies iodine geochemistry on the Antarctic Plateau. *Nature communications*, *12*(1), 1-9.

Line365: what about coastal sites where snow acidity is also enhanced by biogenic marine emissions?

This is a great point. We decided not to discuss in details because it is beyond the scope of the manuscript (i.e. Dome C is not a coastal site), but we added this suggestion at L361-362.

Line367: please see my comment about this part above

This section has been implemented accordingly with the precious suggestions given by the reviewer. See our previous answer for more details.

Line 442-443: maybe better adding a reference for this here

Done at L429-430. In general, the sea-ice extent data were also retrieved from the NASA Earth Science portal (https://earth.gsfc.nasa.gov/) and at the National Snow and Ice Data Center portal (http://nsidc.org) as stated at L160-162. To better highlight how sea-ice has changed over the investigated time period, we added a new figure (Figure S3) in the supplementary material.

Line 469: I would reformulate in a more conservative way, something like: "Future investigations at Dome C need to focus on glacial/interglacial transitions to assess whether Brenr at Dome C is somehow related to large scale variations of sea-ice extensions."

Thanks for the suggestion. We changed accordingly