

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2022-139

Anonymous Referee #2

Referee 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-RC2>, 2022

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.

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 than 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.

Below a few specific comments

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

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

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

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

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

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

Line367: please see my comment about this part above

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

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."