Climate of the Past Discussions

Clim. Past Discuss., referee comment RC2
https://doi.org/10.5194/cp-2022-28-RC2, 2022

Comment on cp-2022-28

Anders Svensson (Referee)

Referee comment on "The ST22 chronology for the Skytrain Ice Rise ice core – Part 1: A stratigraphic chronology of the last 2000 years" by Helene M. Hoffmann et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2022-28-RC2, 2022

Review of ST22 2ka chronology

The manuscript presents a chronology for the last 2000 years of the Antarctic Ice Rise ST22 ice core – corresponding to the upper 184 m. The time scale is based on a well-known 1965 AD Tritium spike, on six volcanic match points of historical eruptions identified in other Antarctic ice cores, on a CH_4 concentration profile match to the WD ice core and on annual layer counting in high-resolution impurity profiles.

The manuscript is rather straight forward, it is well written, illustrated and documented, and furthermore the results are overall convincing. The identification of volcanic sulfate spikes based on sulfur isotope analysis is a smart way of distinguishing the spikes of volcanic origin from the high background signal. The Tritium marker is very convincing. The gas matching is of course a little sloppier, as it involves an (unknown) delta-gas correction that, however, is somewhat constrained by the volcanic matching. The annual layer counting looks very difficult as the annual signal is rather weak or even absent in some impurity parameters.

I have just some minor comments in the following.

Specific comments:

Were there no ECM measurements carried out on the core? If yes, does that record show evidence of seasonal variations or volcanic eruptions?

Why is the high-resolution dust record not applied for annual layer counting?

Why is the NH4 record not applied for layer counting?

Line 84 onwards, the DEP delay time estimation method: Is this approach assuming a constant melt rate over an entire CFA-day or is the recorded melting speed /encoder time somehow taken into account? Is it possible that the "xcorr" function could be dominated by a few dominant peaks in the profile and therefore be 'out-of-phase' for other sections of the profile? Were the position of core breaks at known depths in the core used to verify a correct melt-time - core-depth relationship? I guess contaminated sections (eg end pieces) were removed from the core before CFA analysis? If so, how was this taken into account for the comparison to the whole-core DEP profile? I am just a bit concerned with the 'drifting' seasonality of the conductivity signal and the complete lack of seasonality for the SO4 record in Figure 8. I understand the layer counting shifted with depth between the various records, but why do we not see similar seasonality drifts in Sodium and Calcium then?

Table 1: What does 'depth resolution' refer to here? Eg for Ca2+, does it mean 'we have one measurement point for each 1.4 cm depth interval', or does it mean 'we can resolve features (eg annual layers) of down to 1.4 cm thickness' ? Both values would be useful to have in the table for all of the measured parameters. I guess a simple power spectrum of a section of each record could provide a good estimate of the 'feature' or signal resolution of each of your records? I am thinking along the lines of Figure 7 in Bigler et al., ES&T, 2011.

Figure 6: Indeed, layer counting in those records does not look simple at all. I am (as usual) a bit concerned with the variation in layer thicknesses seen in the deeper section of the core: On the left hand side figure, the layer thicknesses are rather constant – maybe there is a factor of 3 between the thinnest and the thickest layer in the figure? On the right hand side, this variation of layer thicknesses appear to have increased? Now there is maybe a factor of 6-8 between the thinnest and the thickest layer? (I know there are more layers shown for the deeper section, but I do not think that can explain it and I think Figure 7 shows the same for longer periods). In other words, if you compress the left side figure corresponding to the layer thinning at the depth of the right side figure, the layer marks would form a more regular pattern for the youngest section. What it means is that the width of you layer thickness distribution is increasing with depth/age (this would be on a logarithmic scale since the mean layer thickness also changes) or that snow accumulation showed larger variability in the past than more recently. I do not think we have any arguments to say that the snow accumulation should have become more regular in recent times? I guess what I want to say is that I am worried that you have too many very thin and very thick layers in the deeper section of your record. You could test this postulate with a figure similar to Figure 7 in Andersen et al., QSR, 2006, where you compare the layer thickness distribution for the upper and the lower section of the core (if statistics allow).

Figure 7: It looks like you are able to identify annual layers that are down to a few cm in

thickness. Is that possible with the depth resolutions of the applied records (referring to Table 1)? A power spectrum analysis of your records may provide a lower limit for how thin layers you may be able to identify in each record. Again, it looks like the annual layer thickness vary with up to a factor of 6-8 in the deeper section of the core, whereas the variation in the upper section of the core seems to be smaller?

Figure 8: Why are the seasonality of the dust, NH4 and DEP profiles not included in this figure? Since conductivity was matched to the DEP profile the seasonality of those two records should be very comparable?

I made a simple depth-depth comparison of your volcano/CH4 match points to the WD and EDML ice cores in the attached figure. Based on that, my guess is that you have slightly overestimated the age of SP22 at 184 m depth (that I assumed to be 1950BP). Unless we cannot assume a linear depth-depth relationship among those cores… maybe because of enhanced thinning at the more shallow SP22 site? I guess it is fair to assume that the relative accumulation rates have not varied significantly over the last 2000 yr? I am not suggesting that you change the chronology of the deeper section. To be continued in Part two of the chronology.

References:

Andersen, K. K., et al. (2006), The Greenland Ice Core Chronology 2005, 15-42 ka. Part 1: constructing the time scale*Quaternary Science Reviews*, *25*(23-24), 3246-3257.

Bigler, M., A. Svensson, E. Kettner, P. Vallelonga, M. E. Nielsen, and J. P. Steffensen (2011), Optimization of High-Resolution Continuous Flow Analysis for Transient Climate Signals in Ice Cores, *Environmental Science and Technology*, *45*(10), 4483-4489, doi:10.1021/es200118j.

Please also note the supplement to this comment:
https://cp.copernicus.org/preprints/cp-2022-28/cp-2022-28-RC2-supplement.pdf