Replies to editor comments in italic

Dear Dr. Hoffmann and co-authors,
I found your answers to the referees' comments appropriate, and the revised manuscripts needs only few technical corrections listed below:

-At page 10, line 220 you mention a delta age of about 300 years nut then at page 15 in the figure 8 caption you used a value of 330 years and in the appendix, Table C1 caption you are using 300 years. Please, specify and adjust.

The 330 years were a mistake from an older version, all delta ages were changed to 300 years.

-Figure 8: you are not introducing satisfactorily this figure in the text. May you add a sentence, perhaps at the beginning of the section 3.3? Moreover, the figure 9 on the seasonality analysis could come before. Perhaps exchanging these two figure numbers?

We changed the section label of 3.2.1 to 3.3. We also changed the Figure positions of 8 and 9 and added a reference to the new Fig. 9 at the end of paragraph 3.2 and at the beginning of paragraph 3.4 (new) as suggested.

-Please, may you check that when you are using the age expressed as y BP you are introduced the fact that BP is 1950 CE? Perhaps you could add this info in the axis of figures 5 and 8.

We added the note 1950 CE in the axis labels of Fig. 5 and Fig. 8 and also added a short explanation at the beginning of paragraph 3.4 (new)

-In the conclusions, line 369 please add years after 1950 and before BP.

changed

Replies to reviewer 1 comments in italic

In this paper by Hoffmann and co-authors a chronology for the last two millennia is proposed for the Skytrain Ice Rise ice core. The chrononology was built via annual layers counting with the aid of selected accurate age markers to be used as tie points (volcanic eruptions, tritium peak and peculiar features in the methane profile).

Thank you for your review and your valuable input. As stated in the paper, we used the absolute age markers as main age indicators and only interpolated in between them via annual layer counting. We added a short paragraph to clarify this at the beginning of the section about layer identification L227-230 revised manuscript

Surprisingly, some of the major volcanic eruptions of the last millennium were not clearly recorded in the sulfate profile but the authors were able to spot these eruptions via S isotopic composition. While the analytical approach is adequate to retrieve several important information from the ice (including the used tie points), I'm not fully convinced of the reliability of the annual layer attribution. In particular, it looks like the counting of layers, as reported in figure 6, is not straightforward and clear-cut. I can see in this figure (especially for the deeper section) several grey bars which are not corresponding to minima or maxima in the chemical markers. I suppose that this is partially due to the thinning of ice layers at this depth and to the limited resolution of the analytical methods. In figure 6b, the sodium profile is certainly misleading, showing just a few clear peaks with respect to many annual layers counted in this section. It's clear that along this section just the calcium profile was used due to its high resolution, but, as an example I would have picked different peaks and, despite a similar pattern in the Ca^{2+} profile, the two sections 140.0 - 140.5 and 140.5 – 141.0 show 3 layers and only 1 layer respectively. About figure 6, I think that not all the layers are consistent since, sometimes the layer has been marked on a Ca^{2+} maximum (141.5 m) and other times on minima. I would recommend to the authors to better highlight the seasonal pattern in a new figure. For example they could remove those markers that are not useful to the counting purpose (i.e. I can't see any clear seasonal pattern in the MSA or SO_4^{2-}/Na^+ ratio of figure 6a).

We agree that the layer identification does not look straight forward and was indeed challenging. Regarding some obvious mismatches in the markers of the layers in Fig. 6, we found that an old, not finally depth corrected CFA dataset was displayed in the back. We updated and revised Fig. 6 respectively and also changed both shown depth intervals to 22-25 m and 137-140m, because these section proved to be much more representative for the overall variability of the layer thickness. We decided not to take out the MSA and SO_4^{2-}/Na^+ ratios in Fig. 6a to illustrate the difficulties in the layer identification process.

As concerning the CFA section, the reference used for the FIC analysis describes a method for the determination of cations but not for the anions. In particular, I think that it would be a precious information how MSA was measured using a FIC method. A short description with further details about this method should be added (also as supplementary information). How did the two FIC systems work? I guess they were operating alternately so that there is no complete overlap between the samples used for cations and anions determination, but this point should be clarified since the sentence in lines 72-73 is not easily comprehensible.

MSA has been analysed using the BAS FIC system before, we added a respective reference. We also extended the description and tried to better explain how the cation and anion system work together. The cation and the anion system sample parallel from the same depth of the core. Paragraph added L71-L76 revised manuscript

The authors measured nitrate by FIC but they did not use this marker to look for a seasonal pattern. At coastal sites, where the accumulation rate is relatively high, nitrate can be used for dating purposes. I would suggest to the authors to try this approach to corroborate the annual layers identification or, if they did it, to write that this marker was not useful in the construction of the chronology.

Yes, nitrate was measured, but also with the rather low-resolution FIC system and at very low intensities. We therefore decided that it had no advantage over the MSA and did not consider it for layer counting. A respective paragraph was added L246-L249 revised manuscript.

Minor comments:

Line 20 and 119: remove brackets for the reference (e.g. MacFarling Meure…)

Corrected

Line 138: I would prefer “introduction system”

Changed

Line 149: change “repeat” into “repeated”

Changed

Line 197: I would prefer “way” in the place of “fashion”, but I’m not a native English speaker thus, feel free to accept or refuse this change.

Not changed

Line 205: I would add “… (Table C1) and are shown by the dashed lines in Figure 5.”

Changed and added

Line 297: change to “(from -40 to -5 yBP)”

Changed

Fig. 5 and 7: add y or yr to the title of the x-axis

Adapted and changed

SO4 is not correct. The authors should replace it with SO_4^{2-} in the manuscript, captions, tables and axis titles.

Changed throughout the manuscript

Replies to reviewer 2 comments in italic

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 CH4 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.

Thank you for your positive and constructive feedback. We agree that the layer identification process was challenging and would like to emphasize that the counting was used as an interpolation in between the absolute age markers and does not stand alone as an age scale. We added a respective paragraph to clarify this in L227-L230 revised manuscript

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?

No, unfortunately no ECM measurements were carried out on the Skytrain ice core.

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

The dust record proved to be very noisy and did not show a distinctive seasonality like for example in some Greenland ice cores. We therefore did not consider it for annual layer counting and chose the calcium instead.

Why is the NH4 record not applied for layer counting?

In this CFA campaign, the NH_4 was only measured using the FIC system not the higher resolution fluorescence detection system. We therefore did not see an advantage of the NH_4 signal over the MSA signal, especially because the seasonality of the MSA was expected to be much more distinct in this ice core.

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?

For this time delay calculation, only the differences between two depth scales (DEP, solid core and liquid conductivity) were compared. First, the depth scale of the liquid conductivity was determined including the varying melt speed from the decoder for each core piece (in between start and end and breaks). Unexpected spikes in the melt rate were removed using interpolation. xcorr was then used to compare the DEP vs. liquid conductivity data on a depth scale for each day. The depth lag found using xcorr between the two datasets was converted into CFA time delays by dividing the daily depth delay by median daily melt rate. We tried to keep the melt rate consistent and at the same rate for all of the top 184 m (except for the firn). This CFA time delay was then used to compare to the other instruments as explained in the manuscript. We extend the description of this correction approach in the revised manuscript L95-L97

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?

In principle such an influence of a few dominant peaks might be possible, but we checked this by using a linear regression between the DEP and shifted conductivity data for each day. The *breaks in the full core were logged in the field before DEP measurements, however they do not necessarily correspond on a millimeter scale to the ones in the CFA pieces (tilted breaks, additional breaks during processing etc.). The melt time / speed was logged by the encoder in between the breaks, which were logged in the same software, when they touched the melt head. The depth scale is never allowed to drift beyond the next break. If that was the case, an evenly-spaced depth scale was produced. Then the daily delay is applied to each core section.*

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?

Yes, the edges of the ice around the breaks were straightened and smoothed before measurement. The breaks are treated as data gaps in the initial assignment of the depth scale, so the overall comparison between the DEP depth and the conductivity depth will not be affected by the core break positions.

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?

The lack of seasonality in the sulfate record can be explained by the low depth resolution of the measurement on one hand and on the other by the large and noisy background of the marine influences of this chemical species. This was also one of the main problems why we had to apply sulfur isotope analyses to identify volcanic eruptions. The visible drift in seasonality for the conductivity signal is most obvious between the top 50 m and the other depth intervals. We attribute this to the change in layer counting strategy around 60-70m depth rather an influence of the time lag assignment.

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.

Depth resolution in Table 1 refers to the minimal thickness of features that are unambiguously identified by the respective method. It does not mean how many measurement points were made in the respective depth interval. We tried to clarify the caption of Table 1 regarding the definition of "depth resolution" and also in the main text, L81-L84 revised manuscript. We also added a column specifying the measurement density (points per mm) of each instrument for an average melt rate over the last 2000 years.

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).

We agree that layer identification in the presented records was challenging. First we would like to note that some of the visible mismatches of the layer assignment were caused by the use of an outdated CFA dataset in the back of Figure 6. We updated the dataset and revised the figure. However, we agree, that the two examples we chose in the first version of the manuscript were possibly misleading. They suggested very equally spaced annual layers in the shallow part (Fig. 6a) and highly variable layer thicknesses in the deeper parts (Fig. 6b). We revised Fig. 6 and slightly adjusted the shown depth intervals (13-15m to 19-22m and 140-143m to 137-140 m) to give a more representative example of the overall spacing of layers.

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?

Yes, given the depth resolution of the fluorescence calcium method, it should theoretically be possible to resolve layers down to a thickness of about 2cm. The ICP-MS layer thickness resolution should be in the order of 3-4cm. We can not give a comprehensive explanation for the apparent higher variability of layer thickness in the deeper part of the core. However, we observe enhanced thinning in the top 90m (especially between ~ 30-50m) of the core, which we attribute to the comparably large influences of the Raymond arch deeper down in the glacier body. We can not rule out that this enhanced thinning together with anisotropic features of the ice can lead to a higher variability in layer thickness with increasing depth. We did some simple power spectra calculations for the Na, Ca, MSA and conductivity records and added a respective figure (Fig. 7 revised manuscript). We also added a respective discussion paragraph, L265-L277 revised manuscript

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?

We included the seasonality plots for DEP, dust and NO_3, but not for NH_4, because that was only measured via low resolution FIC (see comment above). The nitrate shows a very weak seasonal trend in the top part, similar to the MSA as expected. The DEP signal does not show a distinctive seasonal trend and looks rather noisy, which is probably due to the lower depth resolution compared to the conductivity signal. The dust signal shows a very weak seasonality in the top part, but is overall very noisy. We added a respective description in the revised manuscript L323-L331

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.

Thank you for your enthusiasm and for making that comparison. We think that the WD-Skytrain depth-depth comparison might not reflect the differences in the glaciological settings of both sites. 184m depth at Skytrain equals about one third of the total ice sheet thickness, whereas at WD the same depth equals only about 15% of the total depth. We therefore attribute the small deviations from the linear relationship to the differences in glaciological settings of both sites and the enhanced thinning in the top part of Skytrain ice core.

References:

Andersen, K. K., et al. (2006), The Greenland Ice Core Chronology 2005, 15-42 ka. Part 1: constructing the time scaleQuaternary 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.