Replies to reviewer 2 comments in calibri italic

Thank you for your review and your valuable input. We agree that there were some technical inaccuracies and the signal interpretation including especially the CFA – LA-ICP-MS comparison section needed some clarification.

General comments

Paper by H.Hoffmann et al. presents the laser Ablation - ICP-MS measurements for high resolution chemical ice core analyses with a first application to an ice core from Skytrain Ice Rise (Antarctica). The new Cambridge LA-ICP-MS ice measurement system was applied to selection 130 samples over 6.7 m of the Skytrain Ice Rise ice core. Authors claim that that periodic concentration changes on the millimetre scale can be identified in ice from 26 ka BP.

LA-ICP-MS is a relatively new, promissing and challenging technique that offers ultra-high resolution data. Clear method descriptions and in-depth discussions are essential for such studies, as some challenges, such as elevated concentrations at crystal boundaries and contamination, can only be addressed experimentally.

The section on ablation settings and resolution needs further clarification.

We changed the section and removed a few small errors.

It appears from the text that the settings were optimized in each measurement session, but it's not clear how many measurement sessions there were. It seems from Table 4 that this was based on depth/age-dependent resolution requirements, but the text states that it depends also on ice sample surface conditions.

This statement was misleading. The surface conditions of the ice were the same for alle samples, but the other parameters (expected layer thickness impurities etc.) were not. We corrected the first paragraph of section 2.3.3 (L162 ff revised manuscript) accordingly.

The adjustments made for each sample and why they were made is not clearly outlined.

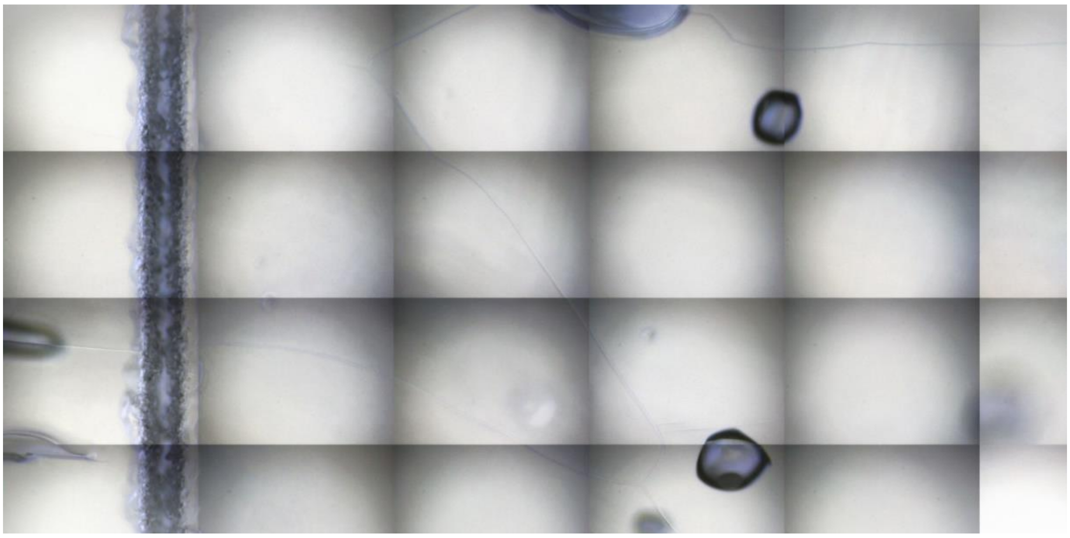
The settings for the glacier ice sample measurements were optimised at the beginning of every measurement session, which usually lasted 3-4 days with several weeks to months in between. There were 10 sessions in total. During this time, the laser ablation system was not only used for ice but for geological samples and was subject to the normal processes of usage and wear. This led to the need of readjustment of the major settings (laser power, repetition rate) for each measurement session. Within the session, the settings were kept constant, to ensure that each continuous set of 8 or 16 consecutive ice core samples were analysed with the same parameters. We amended section 2.3.3 accordingly (L 165 ff revised manuscript).

Regarding the low-resolution value mentioned in the text (182 µm) versus Table 4 (185 µm), there seems to be a discrepancy.

We corrected the numbers.

Were any images taken during ablation? This could help understand the source of noise and peaks detected in MQ ice.

Yes there were images taken before and after measurement of each sample. We can provide an example picture from the on board camera in the appendix (see below, 2 x 3.8 mm, ablation path on the left), but we cannot see an advantage of this kind of picture compared to the high resolution photographs (2 x 5 cm example picture below). The image of the MQ sample did not reveal any features, which could explain the peaks in the signal



Mosaic on-board camera picture (2x3.8 mm)s from the Cambridge laser system on top. Picture of a 2 x 5 cm sample from ~ 100 ka BP from Skytrain ice core on bottom, grain boundaries and bubbles clearly visible

My major concern is the annual dating section and comparison to CFA. It's indeed an important point. It would be better to see the initial raw data and then the results of different smoothing techniques. Some statements in this section are not fully supported by the data/figures.

We revised Fig. 6 and 7 and added the raw data in the background. We also revised the discussion to be more precise about the detection of features.

L.215-217 should be included in the figure caption.

Done.

The statement at L.220-221 about the higher frequency fluctuations detected by LA-ICP-MS compared to CFA is somewhat subjective and depends on the criteria used for annual counting. It's not clear how annual layers were identified before at this depth using CFA. Could you plot annual boundaries?

We changed Fig. 6 accordingly, added the annual layer positions and extended the discussion of the layer detection.

L.222 Regarding the statement about the correlation being less obvious but still visible, it might require further explanations or additional smoothing. At some places, correlation seems to be negative. The dip in concentration around 83.7 m is not well seen in the same place in LA-ICP-MS data. Further smoothing of the LA-ICP-MS data could make it visually comparable however, the raw data should be provided as well.

We reformulated the paragraph and weakened the statement about the magnesium correlation.

From Section 3.2, I'm not convinced that the conclusion holds true for all the elements mentioned at L.248. L.260, why compare calcium to sodium and not to 43Ca measured with LA-ICP-MS? The "striking alignment" mentioned might be overstated.

We changed section 3.2 and Fig. 8 (see below). We now only compare the Na LA-ICP-MS signal to the respective CFA signal. The laser Ca background was generally too high and therefore the signal mainly below detection limit

The section on spectral analysis is not very convincing since although overall periodicity for annual layers may be expected, we are looking at 1 m of the core and annual layers of approximately 10 cm. It's just ten years, and in this case, you can simply plot suggested boundaries and discuss discrepancies in CFA vs. LA-ICP-MS and differences for specific measured species.

This is what we did in section 3.2. We agree that the spectral analysis would not have been necessary in this shallow section. This depth interval with well known annual features serves as a test case for the spectral analysis method to be applied in deeper sections later.

Section 3.3.2 Needs clarification and possibly extension.

“Based on the conclusion here, which of these possible sources is the most dominant cannot be determined.” I would have expect similar experiment for other sections of the core with different grain size and different annual layer thickness in order to assess. The sources and level of contamination should be explained in this regard too.

We agree that this statement was not precise and misleading. We reformulated the paragraph.

At L.291, why was this section of the core chosen for the experiment? Did you perform line staking for other sections? Please comment and explain in the text.

This section was chosen, because the expected layer thickness is below the resolution of the CFA but still large enough to be not significantly affected by grain boundary effects. We changed the beginning of section 3.3.2 accordingly (L305 ff). Yes, we also performed parallel line measurements for other depth intervals. One deep example is discussed in depth in section 3.4- The ice section discussed in 3.3.2 was chosen because the expected annual layer thickness is below the resolution of the CFA, but still large enough to probably not be affected by the grain boundary effects too much.

Section 3.4 is rather speculative at the moment, which is also stated by the authors. I would recommend shortening it and reporting the main findings, with most of the Wavelet spectra and Fourier spectra put into supplementary materials since they show similar features.

We changed 3.4 entirely. We removed the results of the wavelet analysis, which might have been confusing and reduced and clarified the paragraph with respect to the discussion of the section from 26 kaBP only. The section is now focused on the investigation of signal preservation together with the influences of the grain boundaries.