

## ***Interactive comment on “Exploitation of chemical profiles by conjugate variable analysis: application to the dating of a tropical ice core (Nevado Illimani, Bolivia)” by M. Gay et al.***

### **Anonymous Referee #2**

Received and published: 14 February 2014

The review is based on the new version of the paper (“Dating a tropical ice core by time frequency analysis of ion concentration depth profiles”), which was provided as supplement to the response to reviewer #1.

The manuscript presents a method for establishing ice core chronologies based on spectral analysis of ice core chemistry records. I agree that spectral analysis can provide information on the suitability of various proxies for annual layer dating, and that it can provide an estimate of the average annual layer thickness at a given depth. However, I do have some reservations regarding the robustness of the method as a tool for establishing accurate year-to-year ice core chronologies. McGwire et al (2011) in “Identifying annual peaks in dielectric profiles with a selection curve” also used FFT

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within a sliding window to provide annual layer thickness estimates, but to improve performance, the results from this analysis were not used directly, but only used as input to a more sophisticated layer counting routine. I struggle to see why the method should be better applicable for the Nevado Illimani core, where the annual layering according to the authors is very difficult to distinguish, and thus annual dating (using any method) should be difficult. What is the authors' comment on this?

The constructed timescale shows good agreement with the multi-parameter layer counts in the upper part of the ice core (above the aliasing depth). However, the comparison to multi-parameter layer counts is the only proper evaluation of the resulting timescale. The timescale is tuned to match the two volcanic marker horizons, and hence these cannot be used for validation. The method requires a non-trivial treatment of the data series before spectral analysis, and subsequently median filtering of the high-peak location in the spectrum in order to provide a reasonable evolution of the main frequencies. To become convinced of the robustness of the method, I would like to see some sensitivity studies of the various filtering and tuning parameters on which the method is based. I also strongly recommend the authors to apply the same method to another ice core record for which it is easier to evaluate the resulting timescale. The author remarks that for the Andean cores “dating tools remain very limited and their accuracy is hard to check”. If so, this is in my opinion a bad choice for trying out a new dating tool.

Further, and as also requested by reviewer #1, I would like to see a discussion of potential biases of the method. This should preferably also allow an estimation of uncertainty on the resulting timescale.

What makes manual layer counting superior to most automatic approaches is the ability to use several data sets in parallel, and base the chronology on the annual signal in all of these. Does this method allow for using multiple proxies simultaneously?

What are the advantages of this method compared to other automated annual layer

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dating approaches, which have recently been developed?

Specific comments:

Title: The new title is much better.

p.1, l. 5: “notably improves ice core chronology in comparison with other methods as manual counting”. I see no foundation for this statement, since the accuracy of the fourier chronology, according to the authors, cannot be assessed below the aliasing depth.

Line 20: remove “visual stratigraphy“. Visual stratigraphy is a data set showing annual layering, not an approach. Perhaps rephrase sentence as: “. . . approaches like identifying annual snow layers by seasonal features in high-resolution records“

Line 22: ice flow or inverse modeling -> ice flow modelling. (The ice flow modelling can then be used in an inverse approach)

Line 25: distorted -> distorted

Line 29: remove “for time periods longer than one or two centuries“. Synchronization of ice cores by e.g. volcanic marker horizons is valuable to check chronologies at all times.

Line 34: “marine sediment ice cores”?

Line 41: remove visual stratigraphy from list, since it is covered in the subsequent sentence

Line 53: rephrase as “. . . required to understand the processes. . .”

Line 68: Irreversible species?

Line 69: along -> for

Line 70: overlapped -> masked

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Line 73: Add here that manual layer counting has been performed on such ice core records, and some information on what the layer counting has been based on, what the uncertainties were etc.

Line 75-79: To focus this paragraph on automated method used for producing timescales, I suggest removing the sentences “Preto (2004) . . . studies”, and change the first sentence accordingly.

Line 90-93: Some of the methods mentioned in the paragraph are used on data sets, which do not have a very nice or clear annual layering (e.g. the visual stratigraphy data in Winstrup et al, 2012). Please change this paragraph to reflect this.

Line 96: checked along -> applied to

Line 96: The method is only applied to a depth of 70 m, not 100 m.

Line 101: 70m?

Line 104-108: Visual stratigraphy was not used in the de Angelis (2003) multi-parameter dating

Line 115: Remove “in a first step” since the two methods are not dependent on each other.

Line 126: marker suitability -> data suitability?

Line 129: why not use d for depth?

Line 135-138: the equation providing the conversion from firn depth to m.w.e. is excessive and can be removed.

Figure 1: It would be nice to have larger figures with zoom-in on specific data sections, and have the annual layers as found by manual multi-parameter counting marked onto these. Currently it is difficult for a reader to judge how well the annual layers are preserved in the data at various depths. Axes labels are missing.

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Line 140: left -> right

Line 143: This is an unnecessarily difficult way of saying that the annual layers are becoming thinner with depth. Please rephrase.

Line 151: to clarify it to the reader please change: “and that the period is known” -> with a period of 1 year.

Line 189: remove “and its mean is different from zero”. This is obvious since we are dealing with concentration profiles, which can never have negative value.

Line 190: I do not understand how the data is thresholded. Please explain.

Line 193: As also mentioned by reviewer #1, spline interpolation is a rather complex way of interpolation, which can easily introduce spurious peaks. It would be good to show some sensitivity studies on the effect on the resulting timescale when using various interpolation schemes, including more simple ones.

Line 194: remove “as shown” to make this a proper sentence

Line 195: pritty -> pretty (or “quite”)

Line 196: this is the second thresholding? Or is it the same as was referred to in line 189-190? If so, remove the mention of it in line 189-190.

Line 199: remove or replace the reference to Wheatley, 2012

Line 208: It would be nice for the reader to see the details of these adjusted data, e.g. for the same intervals as in figure 1, to judge how the processing has changed the annual layer signal in the data.

Line 213: This “Shannon frequency” is most commonly referred to as the Nyquist frequency. Please include a short paragraph of what it is, what it tells us, and why it is important.

Foot note p. 7: Please expand.

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Line 226: thickness of the selected sliding window -> length of the selected sliding window

Figure 4: After the data preprocessing, concentrations are no longer in any specific unit. All prior treatment of the data should be mentioned in the figure text (mean removal etc.).

Line 257-258: Add a brief description of what spectrum aliasing is, and why it is a problem.

Line 259-260: “these discontinuities come from transients”, please explain what is meant by the word transients. Another reason for why the “streak” is discontinuous may be due to the layering not occurring as a regular “beat”: i.e. if one layer is longer than average, this does not mean the next will be shorter than average. In spectral analysis, this is an inherent assumption, and if violated a spectral analysis will have difficulties catching the 1-year frequency signal. Please discuss.

Figure 5+6: Add to description the values used for cut-off and median filter length.

Line 305-306: The following sentence is difficult to read: “the thickness . . . depth”, please rephrase along the following lines “the size of each sample,  $d_{\text{slice}}$ , is changing with depth.”

Line 321: in relation with ice flowing -> due to ice flow

Line 332-336: What is the crossing depth used for? It does not appear in any equation.

Figure 8 could be added to the lower panels in figure 6.

Line 354: the divergence of the two chronologies starts around 40m, which is significantly before the aliasing depth around 50m.

Line 369-371/Figure 10: Please indicate on figures the area affected by aliasing (the boundary of which depends on the frequency band).

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Line 379: The “increasing trend” in the scalogram looks rather random to me.

Table 1: For comparison, please add the ages obtained from multi-parameter counting

Line 403-404: As the authors also mention in line 403, the reference horizons have been used for tuning the model, hence they do not provide a “valuable estimate of the overall precision“ for this dating method. Please rephrase/remove.

Line 407: What is the depth of the Tambora layer, how far below the aliasing depth is it located?

Line 418: Visual stratigraphy is a data set, not a layer counting approach (see e.g. Svensson et al 2005).

Line 421: According to line 105, multi-parameter counting was performed down to 49.6m. Why is only the section down to 35m shown in figure 11?

Line 448-453: “This timescale is also more reliable than the year-by-year counting performed on the under-sampled part of the core as proposed by Ramirez et al”. What is this statement based on? How does their uncertainty compare with the uncertainty of the timescale provided here?

Tabel 2 and figure 12: Please correct the chemical names.

Line 480-485: Do 1-yr peaks in the spectra of the various chemical species appear if using the manual multiparameter timescale? The strength of this peak would be another (easier) way to estimate their value for timescale development.

Line 487-488: This is an odd sentence.

Figure 13: It is very hard to distinguish between the different timescales on this plot. The figure can be removed since the necessary information is better shown in figure 14.

Figure 14: Again, why not show the results down to 49.6 m?

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Line 506: overlap -> mask

Line 535: Since biases are not discussed, and uncertainties cannot be assessed, I don't see any foundation for the statement "that notably improves the ice core chronology..."

Line 546-551: Why should this approach be particularly suitable for ice cores where we have no evidence for what to compare the resulting timescale with? Shouldn't the approach also be applicable to other cores where the layering is better constrained?

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Interactive comment on Clim. Past Discuss., 9, 3399, 2013.

**CPD**

9, C3448–C3455, 2014

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