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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.

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Review of M.Gay et al.: Exploitation of chemical profiles by conjugate variable analysis: application to the dating of a tropical ice core (Nevado Illimani, Bolivia)

I believe this manuscript convincingly illustrates how frequency space methods can be used for ice core dating.

There are however, several things which I think can be improved in the manuscript.

1) Tightening up the presentation. I would suggest fewer figures, and in my opinion there are some sections that have excessive detail. 2) Discuss every potential source

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of bias. 3) Some methodological issues.

The authors argue that spectrograms are better than scalograms. I am not convinced that this is not just in this particular implementation. Subjetively the scalogram looks much cleaner than the spectrogram in figure 6.

Detailed comments:

I have never met the phrase "fourier conjugate analysis" before, and indeed a google search for that phrase turns up this CPD paper as the only hit. Please rephrase - I propose you try to avoid the word "conjugate". Something with "time-frequency" would be better.

Title: The title is not clear, and rather long. I am convinced it can be made more descriptive and shorter at the same time. I would start the title with "Dating ... ". I do not like the "conjugate variable analysis" (only 56 hits in google so this is also a rather exotic phrasing).

p3402 l29: Please rephrase "interesting step".

p3403: "focuse" -> "focuses"

p3404 l22: I would like an example reference for it being common/traditional.

Eqn2-6: This level of detail is excessive in my opinion. It is sufficient to say that t(d) can be obtained by integrating fs of the annual cycle.

p3406 "write" -> "written"

p3409 line9: The impact of the spline interpolation in the frequency domain is non-trivial and I strongly recommend you use a simpler upsampling method, unless you can present a strong argument for why you choose this upsampling method. I dont think it has any impact on your results. Atleast you need to check and report how robust the results are to this choice.

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p3409: "pritty" -> "pretty".

p3425: "mont" -> "mount"

p3409: You aim to create an automated method. So it would be a great benefit if you could remove the need for choosing a threshold. I see several easy approaches. First and simplest is using the observation that most chemical profiles are log-normally distributed. So taking the logarithm of the raw data would make the distribution closer to normal and remove outliers. Another approach would be to simply use the empirical CDF as a transformation curve. I.e. replacing each value by the percentile it corresponds to.

p3409 line20: It is correct that fourier transforms are strongly affected by any low frequency trend within the short windows. However, the continuous wavelet transform does not suffer the same problem. So there is little benefit in the high-pass filtering for the wavelet method. The main problem with the high-pass filter is that you have to choose a cut-off. And that choice is hard to make unless you know the layer thickness in advance.

p3410 line10: "there are many DFR". This is an odd sentence. Has dfr has been defined.

p3410 eqn8: You have the original coarsely sampled signal. This has a nyquist/shannon frequency associated with the original sampling resolution. It is impossible to resolve higher frequencies by upsampling, and the fshannon associated with Ps is therefore not interesting. I would much prefer to have the fshannon(raw_data) calculated as a function of depth and plotted on the spectrograms. This would probably also be very useful in explaining the aliasing effect.

p3411 line15: It is not a layer thickness, but a window size.

p3411 line 26: It is not a "replication effect". It is an "aliasing effect" which you explain through a replication with simulated data.

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p3411 line26: Please give more details on how you replicate the original signal.

p3412 line20-22: It is clear that the limiting factor is the original sampling and not the upsampling resolution. This goes without saying. So in my opinion the entire shannon frequency discussion could be distilled into a much more compact section.

p3413 section 4.2.3: The layer thickness distribution cannot be normally distributed (you cannot have a negative layer thickness). It is a skewed distribution (probably much closer to log-normal). This means that the median not the same as the mean value but smaller. Thus taking the median will introduce a bias which can introduce a drift . I do not think it is a big problem because you do not look at layer thicknesses of individual layers but average layer thicknesses in short windows. This will reduce the variability considerably and the resulting distribution will be less skewed. However, it is a concern. Please discuss.

p3414 sect 4.2.2: You argue that the spectrogram is better than the scaleogram. I am not convinced. You make several subjective choices. E.g. a morlet is a cosine convoluted with a gaussian, and the width of the gaussian can be adjusted with a parameter. You do not give details on what you have chosen, but typical morlet choices results in only a few oscillations before the wavelet dies out. In your application you want the frequency estimate to be as accurate as possible. That means you should choose a wide version of the morlet. In case of the spectrogram you have a window width of 4-5m. I am convinced that the wavelet approach will perform much better if you use a comparatively wide morlet.

I am also concerned that the spline upsampling injects artificial high frequency variability that act as transients.

Further there are disadvantages of the spectrogram. One of which is the need for highpass filtering. Secondly that not all frequencies are affected equally by the finite window length, which in turn could lead to a potential bias (although it seems not to be an issue).

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section 4.2.4: Instead of high-pass filtering then you could simply restrict the search for the maximum to a certain frequency band.

section 4.2.4: It is a disadvantage that you have to have a rough estimate of the time scale before you can use this method. Using the volcanic markers in the optimization means you cannot also use them for validation. An alternative would be to plot the spectrogram/scalogram, and then subjectively argue that the clearly visible ridge is the annual cycle. Then you can restrict the search of the maximum value to a narrow frequency band around that value.

A potential issue with frequency based methods that need to be discussed somewhere. Consider a short window where there has been a change in accumulation and there are two different layer thicknesses. If this happens close to fshannon then the thin layers will have smaller amplitude than the thick layers. Thus you will tend to get a spectral maximum at the thick layer thickness. I.e. a potential bias towards too thick layers.

section 4.2.5: I like this de-aliasing. However it is not being "mirrored" in the max freq just before aliasing. It is being mirrored in the depth dependent shannon/nyquist frequency from the original sampling. Thus it should be the same for the scalogram and the spectrogram.

page 3415 line10-13: The explanation using the phrase "symetrical representation with respect to the maximum" can be improved.

section 5.1: Again I think this is excessive detail. It is clear that you can get the time scale from the frequencies/layer thicknesses by integration.

section 5.2.1: I do not think this can be considered a validation. It is clear that the peak has to be at Freq=1.

section 5.2.1: Figure 12 shows that your implementation of the scalogram method does not work. However, I am not convinced that this is a problem with wavelet methods in

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general. Can you explain how the strong peak at t=0 is not centered at F=1. I cannot understand how that is even possible considering your methods.

section 5.2.2: You cannot use the same volcanic markers for validation if they have also been used as input to the method. This is somewhat acknowledged in the text, but could be emphasized more strongly.

figure 2: Not necessary. Please remove.

The general red noise nature of the background signals means that when the annual cycle is weak then the method will have a greater chance of falsely picking a lowfreq peak than a high freq peak. This is again a potential bias, and i believe this is what is happening for the scalogram estimates in fig8. I think this could be easily fixed/improved. E.g. by normalizing the scalogram/spectrograms by the background red noise spectrium from and Ar1 fit prior to picking the maximum. Alternatively it could be helped by restricting the search for the maximum to a narrow band around what is obviously the annual peak.

figure 8&9 could be combined by plotting fig9 on top of fig8 in a different color.

section 6.1. "mout" -> "mount"

Section 6: Could it be possible to combine the spectrogram based on all ions, and select the peak in that. Or perhaps calculate figure 8 from all species and then calculate the median value for all depths.

conclusion: It is important to note that as you integrate the frequencies then you will also get an error that accumulates (this is no different from counting methods). However, this means you have to be very careful with everything that might introduce even a slight bias in the estimated frequency. This is why i think it is essential that you discuss every source of bias you can think of. Some might be trivially small in your case, but if other people apply the methods to other ice cores then they need to be aware of it.

p3426 line 1: You cannot start a paragraph like this. This sentence is incomplete and

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needs an "It is" or something.

p3425 line8: Please highlight in abstract.

Would the method work even better if you do it iteratively? I.e. you first make a preliminary timescale using your method. Then correct the preliminary timescale by using the peaks in figure 12.

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