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Climate of the Past Discussions

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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We are grateful to the reviewer for his helpful comments which allowed to improve our manuscript. We hope that he will be happy with corresponding modifications.

General answer :

We have modified the title. It is now: Dating a tropical ice core by time frequency analysis of ion concentration depth profiles.

Some parts have been simplified: the simulation of an aliased signal is replaced by a

direct study on the data (see section 4.2.5: evidence of spectrum replication).

The question raised on aliasing led us to reconsider the presentation: instead of a discussion issued from a signal simulation, we evidenced the aliasing through a careful study of Shannon frequency of the true data set.

Concerning bias sources, we are aware that they have not been fully investigated. The main bias should result from data pre-processing and from the choice of filter parameters or/and window size. However we cannot currently estimate their impacts because of the lack of unambiguous reference horizons making it possible to definitely assess the method accuracy. The only ways we had for that to do was: 1/ to use the very few potential time markers available; 2/ to compare the core dating resulting from our study with another dating established with a completely independent method.

Concerning "the spectrograms are better than scalograms", we can answer in two ways: 1/ Firstly, we do not think that the spectrogram is, as a general rule, better than scalogram, but in our case, the spectrogram works better. 2/ We do not agree with the reviewer's remark "subjectively the scalogram looks much cleaner than the spectrogram in figure 6. Large patches of energy at low frequency and high frequency impulses are quite visible in the scalogram (Figure 5 of the revised version). The presence of such patches explains why in Figure 6 (new version), the direct estimate of the spatial frequency by the scalogram shows high frequency impulses and large low frequency steps. Finally, once the median filter has been applied, the plot of the spatial frequency from the spectrogram is smoother and more regular than from the scalogram.

Lastly, we would like to underline that the study presented here shows that time frequency applied to chemical signals for ice core dating is a promising tool. The method was used here in particularly difficult conditions. It is clear that the method needs to be improved. To do so, it could be particularly useful to apply it to very different kinds of archives.

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. We have replaced "fourier conjugate analysis" by "Fourier time-frequency analysis".

Title: It has been changed.

p3402 l29: Please rephrase "interesting step". We have changed.

p3403: "focuse" -> "focuses". We have changed.

p3404 l22: I would like an example reference for it being common/traditional. The full sentence has been removed in the text.

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. We have simplified the presentation of the method (see section 3.2)

p3406 "write" -> "written". We have changed.

p3409 line9: The impact of the spline interpolation in the frequency domain is nontrivial 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 don't think it has any impact on your results. At least you need to check and report how robust the results are to this choice. We have carried out several tests using different interpolation methods (spline, Hermite polynomial, linear...) and results were similar.

p3409: "pritty" -> "pretty". We have changed.

p3425: "mont" -> "mount" We have changed.

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. We have also tested a log transform instead of data thresholding, results were equivalent. We agree with the reviewer that the log transform does not require to set a threshold.

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. We do not agree with the fact that low frequencies cannot be a problem when using wavelet transforms . In Figure 6 (new version) it can be seen that the estimate of the spatial frequency by the scalogram is affected by low frequency spurious steps.

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

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. We thank the reviewer for this comment which allowed us to improve the estimate of the spatial frequency in the area of aliasing. To account for this observation, we were led to change several parts of our article. We have removed the simulation aliasing presented in the previous version.

We have introduced a new section: Section 4.2.5 “Evidence of spectrum replication” and a new figure (Figure 7). This allowed us to develop a desaliasing method using the exprimental Shannon frequency explained in section 4.6: “Correcting the aliasing: desaliasing”.

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

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

p3411 line26: Please give more details on how you replicate the original signal. This part has been removed.

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. The discussion of Shannon frequencies has been rewritten. In view of changes in the way the spectrum replication (see section 4.2.5, 4.2.6), is correted, the issue of upsampling resolution no longer arises

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. We do not agree with this comment. We believe that the layer thickness distribution is not involved in the occurrence of outliers in the spatial frequency estimate. These outliers result from the presence of scattered non seasonal pulses in the concentration signal.

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). 1 / spectrogram or scalogram: with regard to the treatment of our own data, we conclude that spectrogram gave better results than scalogram. We are not claiming that this conclusion holds in any case 2 / Choice of Morlet wavelets: we agree that further study would be useful. As discussed in the conclusion, our work is a first approach that demonstrates the feasibility of ice core dating based on Fourier time frequency analysis and may lead to further works. 3 / Spline upsampling: we are aware that spline resampling can introduce spurious pulses. To prevent this, we have thresholded the original data before applying splines. It would be, now, helpful to test other resampling method. 4 / When using wavelet transform to process our data, several artefacts appeared at low frequencies (see Figures 5 & 6 of the revised version).

section 4.2.4: Instead of high-pass filtering then you could simply restrict the search for the maximum to a certain frequency band. This idea is an interesting track for further studies, that we have not explored yet. Methods of image processing could also be applied (mathematical morphology ...).

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. 1 / We changed our presentation by emphasizing that these markers have been used for filter optimization and indicating that differences between the 2 eruption historical dates and corresponding dates estimated in this study provide an estimate of the overall accuracy of our dating method (see section 5.2.2, line 411-412). As stated above, we believe that the only objective validation is based on the detailed comparison with De Angelis et al. 2003 (see section 5.2.3). 2 / The comment concerning, the question “Then you can restrict the search of the maximum value to a narrow frequency band around that value” is addressed in the answer to preceding comment. 3/ “Consider a short window where there has been a change in accumulation and there are two different layer thicknesses”,this situation will be very difficult to treat. We have assumed that the annual accumulation did not present abrupt changes.

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. We thank the reviewer for this very important comment which led us to reconsider the treatment of concentrations profiles in the aliased part of the spectra. We have added Figure 7, that shows the “chemical Shannon frequency”, the “spline Shannon frequency” and the “one year frequency”. Several different areas may be distinguished in this figure. From the 0 depth to 35 m we(Part 1) the “effective” Shannon frequency is the chemical one and the spatial frequency coming from the

1 year period is not aliased. From 35 to 50 m we (Part 2) the “effective” Shannon frequency comes from the splines sampling. In this region the one year frequency remains not aliased. At 50 m we two changes are observed:The “chemical Shannon frequency” becomes again the effective “Shannon frequency” and the spatial frequency associated to the one year frequency begins to be aliased.

Below 50 m we(Part 3) the” effective Shannon frequency” deduced from the experimental sampling does not vary by much. Our hypothesis of a constant value for the chemical Shannon frequency was not so different from the real values. We have modified the treatment of the aliased part taking into account the real chemical Shannon frequency (see section 4.2.5, 4.2.6). Figures 8 and 9 show the new results. These modifications affect only the dating of the aliased part of the spectrum. So the optimization we made using the two historical markers located in the non-aliased part of the spectrum does not change. The comparison with previous year by year dating of de Angelis et al. (2003)), referring to non-aliased data was not modified.

page 3415 line10-13: The explanation using the phrase "symetrical representation with respect to the maximum" can be improved. This part has been removed, given the changes presented just before.

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. We have simplified.

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. We have assumed the existence of 1 y-1 periodicity. This assumption does not necessarily imply the appearance of a spectral trace in the spectrogram at 1Hz frequency. As shown in Figure 10, the 1 y-1 track is clearly visible on the spectrogram of the signal whose chronology was deduced by the spectrogram, while a similar track does not appear on the spectrogram of the signal whose chronology was deduced by the scalogram.

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 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. A careful examination of Figure 8 shows that the estimate of the spatial frequency in the vicinity of the surface is different in the spectrograme and in the scalogram. This explains why the peak in the scalogram is not at 1 y-1

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. We changed our presentation by emphasizing that these markers have been used for filter optimization and indicating that differences between the 2 eruption historical dates and corresponding dates estimated in this study provide an estimate of the overall accuracy of our dating method (see section 5.2.2, line 411-412). As stated above, we believe that the only objective validation is based on the detailed comparison with De Angelis et al. 2003 (see section 5.2.3).

figure 2: Not necessary. Please remove. We have removed.

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. We appreciate this reviewer comment, in line with of our feeling that this work provides investigation lines for further studies. Indeed, we indicate in the conclusion that we have not explored all the numerous “tracks” that could improve the dating precision. However we have taken into account the bias that can be introduced by the red noise by the optimization of the cut-of frequency of the high pass filtering of the data.

According to the reviewer’s suggestion we have replaced the high pass filter by a “whitening” of the data spectrum. However, we have seen that the whitening creates on the initial estimate of the one year spatial frequency trend a large number of discontinuities that perturb its estimation. We explain this by the fact that the spectrum whitening equalizes the one year trend, giving the same energy to outliers. This effect increases the relative influence of outliers.

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

section 6.1. "mout" -> "mount". We have changed.

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. It is not relevant to combine the spectrogram based on all ions because all these ions do not have the same geochemical behavior. Some of them may be influenced, for instance, by strong not seasonal events (dust storms, volcanic eruptions) and others to post deposition processes resulting in signal relocation or loss. This is the reason why we compared the independent chronologies provided by the different ions: such a comparison may provide an opportunity to better understand geochemical differences.

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. At the end of the conclusion, we propose an explanation for why the our dating approach was adapted to the type of data processed in this study . However and to go further, it would be necessary to apply the method (and, in parallel, to check data pre-processing and processing) to a large set of archive data.

p3426 line 1: You cannot start a paragraph like this. This sentence is incomplete and needs an "It is" or something. We have removed.

p3425 line8: Please highlight in abstract. This is done. We thank the reviewer for this suggestion.

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. It will be a new track of research. . .

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/9/C2026/2013/cpd-9-C2026-2013-supplement.pdf

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