

“AstroComb(v.1.0): Non-linear, Multi-channel, Probabilistic Cyclostratigraphic Analysis”

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Manuscript by Iris Fernandes, Klaus Mosegaard, Aske L. Sørensen,

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Review by Stephen R. Meyers

Having served as a referee for the prior version of this manuscript, I appreciate the authors’ revisions, which have improved the presentation. However, there remain several critical aspects that need to be addressed for completeness and clarity, some of which were also raised in my prior review. I hope that the authors find the suggestions in this review helpful in improving the presentation of the method and making it accessible to the community.

With best wishes,

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OVERARCHING COMMENTS:

Published journal articles on statistical methods serve as the official peer-reviewed record, and thus the proposed algorithm should be documented sufficiently that, in principle, it can be reproduced from the published manuscript. This is not possible to do from the present manuscript. In other words, a reader should not be required to consult the code for an accurate description of the methodology that was applied. In addition, the fields of astrochronology and cyclostratigraphy are highly interdisciplinary, so it is important to provide a manuscript that adequately introduces the method to the broad range of scientists that will potentially apply the approach and/or interpret the results. With these two aspects in mind, I offer the following recommendations.

SPECTRAL BACKGROUND: A major problem at present is that there is no indication of the spectral background estimation approach that is used in AstroComb. Appropriate determination of the spectral background is critical to the estimation of the probabilities/likelihoods, and this has been a topic of considerable debate in the cyclostratigraphic community (see reviews in Vaughan et al., 2011; Meyers, 2012; Vaughan et al., 2014, Meyers, 2019; Weedon, 2022). This is not trivial and has been a major issue in cyclostratigraphy. The fit of the stochastic model to the data spectrum background (“spectral continuum”) must be validated for the probabilities to be reliable (e.g., see Malinverno and Meyers, 2024). The authors need to discuss the background fitting approach that is implemented, and ideally, show at least one example spectrum with the background fit for each analysis, which will also provide guidance for the community. Finally, in the response to my last review, the authors state, “We believe, however, that classical misfit approaches are much more sensitive to the background trend removal than the spectral peak detection method used in our

likelihood calculation." This is an assertion that must be tested and demonstrated explicitly, rather than assumed based on a belief.

SMOOTHING AND LAYER-TO-LAYER DEPENDANCY: In the abstract (lines 13-14) it is stated that "Our framework does not smooth data or impose layer-to-layer dependency". This is not an accurate statement, as the method is a moving window approach, in which data is shared between adjacent windows; shared data necessarily results in some dependency. The resolved sedimentation rates are analogous to an average for the window (here, a 3.3-meter window was used). This is often referred to as data smoothing, for example as in "moving average smoothing". That doesn't prohibit the method from resolving rapid shifts in the 3.3 m window average sedimentation, but this is not unique to *AstroComb* and is a shared property of other time-frequency Fourier methods such as the spectrogram. For example, rapid shifts in sedimentation can be observed in the model analysis presented in Figure 5 of Meyers et al. (2001, Journal of Sedimentary Research). It is important to be clear about these aspects, for the sake of accuracy and to better communicate the approach to the broader community. Pertaining to these issues, guidance on how the optimal window size is selected would be very helpful for the community.

Related to the above comment, on line 22 of the abstract it is noted, "This approach advances astrochronology by combining cycle detection with formal stratigraphic modelling, while preserving fine-scale depositional variability". This may lead to confusion by practitioners, so it is important to be clear that "fine-scale depositional variability" refers to shifts in the 3.3 meter average sedimentation rate, not fine-scale sedimentation rate changes at the scale of the astronomical cycles. A better way to state this is that the method can resolve secular shifts in sedimentation rate, defined by the scale of the moving window. These can be abrupt or gradual (see Figure 5 of Meyers et al., 2001). There are other methods designed to truly resolve "fine-scale depositional variability" at the bedding scale, such as that of Schiffelbein and Dorman (1986), the Gamma method (Kominz and Bond, 1990), TimeOptTemplate (Meyers, 2019) and the Alpha method of Ma et al. (2020), but this is not achieved by *AstroComb*.

DESCRIPTION OF THE ALGORITHM: To make the method description more broadly accessible to the cyclostratigraphy and astrochronology communities, and to allow the complete method to be reproduced from the published article, section 2.5 ("The *AstroComb* Alogrithm") should include the following information:

In practice, the AstroComb algorithm is implemented as follows in this study (considering only one variable/channel for this description):

1. *Apply a moving window of specified size (e.g., 3.3 meters) to the data series, incrementing across depth (e.g., every 0.2 mm), and for each data window, calculate an amplitude spectrum with background removed, as follows:*

- 2.1 *Detrend data in window (quadratic function)*

- 2.2 *Taper data (Tukey taper)*

- 2.3 Pad the tapered data on both sides of the data series (with zeros)
- 2.4 FFT the tapered and padded data, then return the amplitude spectrum
- 2.5 Estimate and remove the background spectrum using fast L1 fit of sum of 3 decaying exponentials (or if another approach is used, specify)
2. Compute sedimentation rate posteriors. For each data window:
 - 3.1 Calculate the equivalent frequency (cycles/m) for each theoretical astronomical line (cycles/kyr) using a grid of potential sedimentation rates.
 - 3.2 Loop over all sedimentation rates and astronomical target frequencies; use a smooth, monotonic sigmoid function to estimate probability of amplitude peaks at target frequencies in the data spectrum, given specified sedimentation rate (following background removal in step 2.5)
 - 3.3 Calculate product for posterior pdf of sedimentation rate
3. Using above information, compute the time-depth posterior

COVARIANCE OF ELEMENTAL DATA: I was pleased to see that the authors now discuss the issue of covariance of elemental data, and the need to demonstrate that the different channels are independent. To better communicate this to the broader community, it would be useful to show crossplots for the selected elements (S, Al, Mn), and to report their Pearson correlation coefficients.

ADDITIONAL COMMENTS:

Line 18: change "radiometric" to "radioisotopic"

Line 38: remove "where"

Lines 53-56: "jointly optimizes sedimentation rates and astronomical tuning". Note that sedimentation rates are derived from astronomical tuning, so as this is presently stated it may appear redundant. The AstroGeoFit method, as well as the related Bayesian regression-based methods of Meyers and Malinverno (2018) and Malinverno and Meyers (2024), jointly evaluate uncertainty in the astronomical tuning frequencies (using priors for k and the fundamental frequencies) and the geological timescale (sedimentation rates).

Line 80: change "correct" to "predicted"

Line 83: To better illustrate the potential misuse of the algorithm, please include an example analyzing a pure noise data series, as in Trayler et al (2024; see their Figure 5). Below, I will refer to this as the "Noise test".

Lines 146-147: In discussing a key property of their approach, the authors state, "but AstroComb is instead aimed at simulating the data uncertainty evaluation carried out by practitioners analyzing observational data." It's not clear to me what this statement really means. A key distinction is that the regression fit method that underlies TimeOpt,

TimeOptMCMC, TimeOptBMCMC and AstroGeoFit evaluates the fit (and uncertainty) in the time domain, while AstroComb evaluates fit in the frequency domain (as does the approach of Malinverno et al., 2010). Related to this, on lines 151-153 the authors note, "However, in our approach we are automatically taking all data uncertainty sources into account, including the practitioner's confidence in identifying spectral peaks." The authors argue for an ad hoc approach, based on a sigmoid function for estimation of likelihood. I cannot identify any theoretical reason why this approach would automatically take all data uncertainty sources into account, more so than other methods such as TimeOptMCMC. These are not justifiable arguments, so I recommend the authors remove them, and simply describe what they are doing.

Line 190: Change the following,

"That is, we can assume channels are independent of each other."

to,

"In order for this approach to be valid, the channels must be independent of each other."

Line 197: at the end of this sentence, add "(a uniform distribution)".

Section 2.6: Please be clear about the sources of the eccentricity, obliquity and precession periods used here (with references). The precession and obliquity periods cited here do not match the values from 497 Ma in Waltham (2015; <https://davidwaltham.com/wp-content/uploads/2014/01/Milankovitch.html>) or Farhat et al. (2022). They do appear to match the Waltham (2015) periods for 515 Ma. The authors should justify why they chose the 19.5 ky precession period among the four that are predicted (20.5, 19.5, 16.9, 16.8 ky), and why they only chose one of the short eccentricity terms (e.g., see Meyers, 2015, Table 1). Note that the periods on line 258 also disagree with the periods shown in Section 2.6 of the manuscript (405, 95, 124, 41, 25.7, 112, and 100 ky). There are some typographical errors here to fix.

Line 246-247: "We do not define their expected spectral power ratios based on orbital modulation transfer, since the Earth's climate and environmental response to the Milankovitch forcing modifies these ratios in an unknown way". To be clear, this should be restated as, "We do not define the amplitudes of the astronomical cycles, as these are unknown *a priori*, due to potential non-linear transfer functions associated with the recording of the cycles in the stratigraphic record (Meyers, 2019)." Discussing "ratios" may be confused with the "frequency ratio approach", which is what AstroComb is essentially building upon. "Orbital modulation" also may be confused with the inherent amplitude and frequency modulation of astronomical cycles.

Lines 272, 391: Note the comment about smoothing discussed above.

Figures 3, 5, 6: Indicate the units for sedimentation rate on the probability density plots.

Figure 7: Indicate the reconstructed sedimentation rate next to each spectrum. Indicate if amplitude or power is plotted.

Figure 3: Please discuss why the Kullback-Leibler information measure varies through the interval; for example, why is it consistently reduced below 77 m depth. Are the oscillations in this measure above 77 m meaningful; what is causing them?

Figure 5 and Lines 313-316: Again, please discuss why the Kullback-Leibler information measure is consistently reduced below 76 m depth. What is it about the data and depositional system that results in a poorer fit? Why does this lower interval with reduced values show a better match between the AstroComb result and the previously published result of Sørensen et al. (2020) (see Figure 8, further discussion below)?

Line 316-318: "Overall, figure 5 suggests that Milankovitch cyclicity is present throughout the core, as indicated by the non-zero information content". How do these values compare to those from the analysis of a purely random record ("Noise test")? This is important to consider, if the claim is that non-zero information content supports the presence of Milankovitch cyclicity.

Figure 8 and 9: There are large discrepancies between the previously published astrochronology (Sørensen et al., 2020) and those reconstructed from the AstroComb method. In Figure 9, at 70 meters depth, the two timescales disagree by about 1.5 million years – this does not give one much confidence in astrochronology! There needs to be a discussion of why these differ so much in the upper half of the core. What are the sources of bias in the original tuning of Sørensen et al. (2020), and what are the potential sources of bias in the AstroComb tuning? Why should we consider one more accurate? Please provide guidance to the community about this issue.

Figure 9 caption and title: Note that "Relative Geologic Time Scale" does not apply once the record is anchored, as is done here.

Line 367: Change "is therefore expected to" to "therefore could"

Line 370-373: Change "An additional problem with the non-linearity of the sedimentary system is that, even if we know the amplitudes of the input Milankovitch signal, the output amplitudes observed in boreholes are entirely unknown." to "An additional problem with the non-linearity of the sedimentary system is that, even if we know the THEORETICAL amplitudes of the input Milankovitch signal, the EXPECTED amplitudes observed in boreholes are POORLY CONSTRAINED."

Lines 405-409. Please indicate that the code is written in Matlab.

Lines 511-513. Correct the authorship on this publication (I am not an author on this study).

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