

Response to Reviewer 3 (Stephen R. Meyers)

Comment

OVERARCHING COMMENTS

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. ...a reader should not be required to consult the code for an accurate description of the methodology that was applied.

Response

The reviewer is right: For some reason, we left out the (important) description of the background spectrum estimation. This has now been added to section 2.3 (in the description of the likelihood function). Note that this is one implementation of the background spectrum fitting. Others are possible, and we have used some of these possibilities after this implementation (AstroComb 1.0) was made.

Furthermore, to clarify our methodology, section 2.3 has now been simplified and improved, avoiding introduction of unnecessary concepts.

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

Response

It is true that determination of the spectral background is critical to the estimation of the probabilities/likelihoods when traditional misfit methods are used, because "residual" spectral background (not properly fitted) from the entire spectrum contributes significantly to the misfit. In our approach, however, the spectral background only contributes locally around the spectral lines, so - in our experience - any reasonable fit to the spectral background will work.

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.

Response

The assertion is mathematically very simple: In classical misfit approaches, you would calculate the misfit between the observed spectrum, where the background is removed, and a synthetic spectrum consisting of isolated Milankovic peaks. If the background is not removed properly, the misfit (integral of sum-of-squared differences) will be significantly increased by the background residual between the peaks. In our approach, however, the spectral background only contributes close to the peaks, and the values away from the peaks are not entering into the likelihood calculation. Hence the sensitivity to the background trend removal is much smaller.

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".

Response

The statement that

"... shared data necessarily results in some dependency. The resolved sedimentation rates are analogous to an average for the window This is often referred to as data smoothing, for example as in "moving average smoothing"

is not accurate. In moving average smoothing, the average is a continuous function of the location of the window center. In contrast, a maximum a posteriori (MAP) solution to the spectral fitting problem is a *NOT* a continuous function of the location of the window center. This means that the best estimate of the sedimentation rate can jump significantly from one layer to the other, and hence, if you plot a best-fitting Milankovic variation to your records in the time domain, it will not be continuous. This phenomenon can be defeated by smoothing in the time domain, but we have not done so. Furthermore, we do not claim that the lack of smoothing in our approach is not shared by other methods.

Our statement in the abstract that our method is "...preserving fine-scale depositional variability..." simply refers to the above-mentioned fact that the best estimate of the sedimentation rate may change significantly between neighboring layers.

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: ...

Response

We believe that the steps of the algorithm are sufficiently described in the current manuscript.

COVARIANCE OF ELEMENTAL DATA: ... 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 ...

Response

We have not included cross-plots, in order to limit the number of figures in the paper.

ADDITIONAL COMMENTS:

Line 18: change “radiometric” to “radioisotopic”

Response: Done.

Line 38: remove “where”

Response: Done.

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).

Response: Corrected. We thank the reviewer for noting this ambiguity. To avoid potential confusion, we have revised the sentence to read: “More recently, AstroGeoFit has introduced a unified, uncertainty-aware framework that jointly optimizes sedimentation rates and enables rigorous error estimation in age–depth models (Hoang et al., 2025)”.

Line 80: change “correct” to “predicted”

Response: We have changed “correct” to “given”, since the astronomical periods are input (assumed known from other studies).

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”.

Response: We have now added a test with pure noise data (figure 3). For comparison, figure 2 with the synthetic example with 200% noise was re-run and re-plotted.

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.

Response:

In Bayesian data analysis, prior information is provided by the analyst, usually as probability density functions whose form and parameters are provided, either from empirical information (“histograms”), or from domain expert knowledge, based on education and experience (often called “subjective” information). Prior information can be defined for model parameters, as well as for data (Tarantola, 2005). Domain expert knowledge is widely used and accepted. Even when measuring the length of, say, a table with a scale, the data uncertainty is evaluated from “subjective” experience with reading the scale and evaluating the uncertainty. In this simple example, the reading includes all sources of uncertainty: the health of the eyes of the analyst, his/her experience, lighting conditions, shaking of the table, etc.. Some of these uncertainties are related to the analyst. Ultimately, the total uncertainty is usually described by a simple function - for instance a Gaussian with a given variance - and used in subsequent calculations.

The situation is similar in our algorithm. The geologists, who have looked for Milankovic periods in geological records, have built an experience with this type of data. Their confidence in identifying valid Milankovic peaks in spectra of geological records is our source of information about data the uncertainties, and the way we quantify the confidence (in this case through a sigmoid function) is one possible choice. The sigmoid

function can be replaced with a function that is better adapted to the experience of a new user, or a community of users.

To emphasize this, we have now in the revised manuscript (line 182-184) stating that: "The probability function $p(x)$ is chosen from user experience, and in our implementation, it was chosen as a smooth, monotonically increasing sigmoid function...".

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."

Response

We have changed the sentence to "... , assuming that noise on different channels are independent of each other." The reason is that channels can be dependent in two ways: (1) If the noise-free parts of the channels are considered random, they can be (statistically) dependent, and (2) if they are noisy, their noises can be statistically dependent. We assume that the channels consist of a deterministic part (the Milankovic signal, for which we have a mathematical model), and a random part, for which we only have a statistical model. It is the latter that needs to be independent amongst the channels, for our method to work.

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

Response: Done.

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.

Response: Indeed. The values were taken from Waltham et al. 2015 for age = 515 Ma. This is not too different from 497 Ma. However, we have now changed the chosen periods to 497Ma, rerun the results, and referred to the papers where the periods appear:

	In manuscript	Waltham 497 Ma
Long eccentricity:	405 ky	405 ky
Short eccentricity:		98.9 123.9 ky (Waltham 2015 for today, or as observed in Sørensen et al. 2020)
Obliquity:	32.4 ky	32.6 ky
Precession:	19.5 ky	20.6, 19.6, 16.9, 17.0

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.

Response: We have now changed the sentence to "We do not define the amplitudes of the astronomical cycles, as these are unknown *a priori*. The Earth's climate and environmental response to the Milankovitch forcing modifies these amplitudes in an unknown way." We do not mention non-linearity, because even in an unknown, perfectly linear system the (relative) amplitudes of the output Milankovic periods will, in general, be unknown.

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

Response: See our comments about smoothing/discontinuities above.

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

Response: Done.

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

Response: We have indicated that it is amplitude spectra. However, a spectrum does not give are specific constructed sedimentation rate. The method is probabilistic, and gives distributions of sedimentation rates.

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?

Response: We added to the updated figure 2 caption. “Lower information content below 79 m depth, albeit with local peaks near 82.3 m and 86.1 m, reflect fewer and/or less pronounced peaks within the sliding window.”

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)?

Response: We added to the Figure 5 caption: “Below 76 m, the information content is smaller where spectral peaks are few and/or less pronounced.” After having rerun the inversion with 8 Milankovic cycles, there is an overall better match with the previously published result of Sørensen et al. (2020).

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.

Response: See our new figures 2 and 3.

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.

Response: We have now rerun the algorithm on the Fågeltofta-2 data with the 8 Milankovic cycles given by Waltham (497 Ma). This changes the MAP results of AstroComb so there is now better consistency with previously published astrochronology (Sørensen et al., 2020).

However, as now carefully explained in section 4.1, apparent discrepancies between different analyses can occur, even when those analyses are fully correct. The point is that the solution to the problem is non-unique: As seen in figures 5 and 6, there are multiple

solutions to the sedimentation rates, all having a high posterior probability. The AstroComb output represents the highest-probability (MAP) values, but other studies may use different (also quite high-probability) sedimentation rates, resulting in deviations from the AstroComb timescale.

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

Response: Done.

Line 367: Change “is therefore expected to” to “therefore could”

Response: Done.

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.”

Response: There were some slight inaccuracies in our original formulation. It is now modified to: "For linear, as well as nonlinear sedimentary systems, even if we know the amplitudes of the input Milankovic signal, the output amplitudes observed in boreholes are entirely unknown."

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

Response: Done.

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

Response: Done.

References

Tarantola, A. (2005). Inverse Problem Theory. SIAM.