

To the Solid Earth Editor of Geoscientific Model Development, Dr Thomas Poulet.

Response to the review of our manuscript 'egusphere-2025-6145', entitled "AstroComb (v.1.0): Non-linear, Multi-channel, Probabilistic Cyclostratigraphic Analysis".

Thank you for the interesting and constructive reviews of our manuscript. In our revision of the paper, we have addressed the issues raised by the reviewers and yourself. Review comments in blue fonts and our responses are using black fonts with *new text in italics*. Below you will find our general comments and our response to each of the points raised by the reviewers. Part of the text below is now included in the manuscript.

Response to the Editor

1.

Revisions are needed both the text and some figures. The structure, framing, and level of analysis need to be strengthened to meet GMD standards.

Yes, the manuscript has now been revised to accommodate these points.

2.

All references cited must be accessible public sources, in line with GMD/Copernicus policy ("references to public sources of information"). Citations to material that is not yet accessible should be avoided; for example, Fernandes et al. (2026) may be used once publicly available.

Citations to unpublished material have now been removed.

3.

The reviewers' suggestion to introduce and test the code on synthetic (controlled) examples should be carefully considered. This is especially important given that the manuscript currently does not present a direct comparison with previously published cyclostratigraphic interpretations of the same sections, which is essential and should be included in the revised version.

Yes, indeed. The code is tested on synthetic data (see figures 8, 9 and 10). The result from synthetic data is now introduced before applying it to the geological XRF dataset. Also, we have now added the published sedimentation rate-depth relation and the time-depth relation using traditional methods.

Response to Reviewer 1 (Dr Valentin Zuchuat)

Reviewer's comments

4. Autogenic processes and signal shredding.

I recommend the authors to acknowledge autogenic processes and signal shredding explicitly (rivers/deltas, compensational stacking; see commented pdf for references examples). It is also necessary to clarify how AstroComb distinguishes or remains agnostic to these signals vs. orbital forcing. The authors could consider adding a short paragraph in Introduction and returning to it in Discussion. An easy way to start with this is to take some of the text from the “limitations” chapter.

Thank you for this suggestion. We have followed your suggestion and acknowledged autogenic processes and signal shredding explicitly with reference to Paola et al., 2016 and Munnecke et al., 2001

5. I also suggest the authors to avoid the term “geological noise” for non-Milankovitch periods. The preserved signal might not be “noise”, but rather the true expression of the mix of autogenic processes, Milankovitch cycles, and other non-linear effects such as bundling of several cycles. It is also important to justify why AstroComb does not include such periods (lines 311-314).

We have now avoided the term 'geological noise', e.g. line 344 now reads:

“This means that a hypothetical, noise-free Milankovic spectrum will, in general, contain spectral components outside the main Milankovic frequencies Sinnesael et al. (2018).

6. Scope.

Other software are briefly mentioned (l. 31-33). Also this paper remains quite diplomatic about how much better AstoComb is in comparison to these other software, the reader of this manuscript could do with a figure that displays how the different software deal with the same input dataset.

This paper is a presentation of a new method, AstroComb, and a documentation of its correctness and of the new possibilities it presents. We support this by showing its performance on synthetic data with a very large noise level and by comparing results from a geological data set to what has been previously obtained using a more traditional approach. We believe this is sufficient documentation of AstroComb and that further comparisons and scrutiny of other methods should belong in a review paper.

7. The manuscript also only use single elements or several single elements together. A lot of published research (see references in the attached commented pdf) study change in Earth System conditions through the use of elemental-concentration ratios as well as enrichment factors. It could be useful to show how AstoComb responds to elemental-concentration ratios and enrichment factors.

Yes, we agree that it is interesting to investigate how AstroComb responds to elemental-concentration ratios, peak areas and enrichment factors. While we have done a bit of sensitivity testing on this topic, we judge that a larger comparative study will be overwhelming here and instead include it in future work on the algorithm.

8. Finally, it would be beneficial for the manuscript to compare the results from the conformable Alum Shale Formation to other sedimentary-cores XRF scans much more impacted by event beds, hiatus, or vacuity. This would allow the authors to highlight how AstroComb deals with these extreme changes in sediment supply. If this is not possible, please add a short demonstration in addition to your synthetic example, or at least discuss applicability in greater detail. Doing so would allow you to confirm, precise, or disagree with published sedimentation rates of other sedimentary units.

We respectfully disagree with the point raised here. This work presents a new methodology and, as such, we have chosen to limit our exposition to a description of the method, the software, performance on noisy synthetic data, and a comparison of AstroComb analysis of the Faageltofta-2 data with previously published results. Every single geological data set comes with its own challenges and deserves careful discussions, beyond what can be presented in a methods paper.

9. Methods chapter.

I would like the authors to add in the method chapter a paragraph on their XRF data collection workflow: add model, tube type, kV/mA, measurement time, temperature, calibration/normalization, etc. The authors could also cite the paper that generated these data in the first place.

We have mentioned how the data was collected and cited the paper in which it was generated. Line 264 now reads:

To illustrate the application of the multichannel, probabilistic Milankovitch period detector, AstroComb, we analysed high-resolution XRF data from a ~21-meter long interval (67.2520 m - 88.0744 m) of the Fågeltofta-2 drill core through the Alum Shale Formation in southern Sweden obtained by Sørensen et al. (2020). Briefly, the XRF data was collected using an ITRAX core scanner from Cox Laboratories with 30 kV voltage and 50 mA current on a Rh tube. The XRF measurements were collected with a high scanning resolution of 0.2 mm along the core and relatively long exposure time (7 sec/scan), producing signal reported as concentrations for 22 elements, incl. Al₂O₃, SiO₂, S, K₂O, CaO, TiO₂, V, MnO, Fe₂O₃, Ni, Cu, Zn, Ge, As, Rb, Sr, Y, Zr, Mo, Ba, Hf, and U. This extensive elemental dataset formed the basis for the spectral analysis.

10. Results and interpretations.

I would suggest to move beyond “visual examples” and describe the results in great details. For instance, the figures displaying the age-model: how does this relate to the sedimentation rates? Where do abrupt changes occur (e.g. line 326); how large are these abrupt changes; over what thickness/time do they occur; what geological interpretations are plausible? Why are some intervals “less Milankovitch” than others in your “information content” curve, tie all of this to core observations and geochemical data

Yes, we have now added an explanation on how the age-models are calculated based on the sedimentation rate probability distribution in the method section. Line 198ff now reads:

From the marginal sedimentation rate distribution

$$p(r) = \int p(r, \mathbf{a}) \, d\mathbf{a}$$

at each stratigraphic position, we can now compute a floating geological time, t , and its uncertainties as a function of depth, z .

$$t(z_0 + N\Delta z) = t_0 + \Delta z \sum_{n=1}^N \frac{1}{r_n}.$$

We have also explicitly elaborated on how abrupt changes in the sedimentation rates of synthetic data affects the AstroComb results. Line 279ff now reads.

Abrupt changes in the sedimentation rate of about a factor three occur at 70, 74, 81, and 83 m, all of which are accurately captured by the AstroComb algorithm (figure 3). However, the test also illustrates how artifacts (possible, incorrect sedimentation rates) are introduced by the noise, the variability of the true model, and the non-linearity of the problem. Some of these spurious parallel features could potentially be misinterpreted as representing true sedimentation rates. This highlights a key challenge in cyclostratigraphic analyses: a given cycle may be interpreted as corresponding to different target periods, resulting in a multimodal sedimentation-rate probability distribution. Nevertheless, the highest probability remains associated with the true reference model (figure 3).

We also discuss why the KL information content varies for some intervals in the synthetic data. Line 288 ff now reads;

The Kullback-Leibler information content varies along stratigraphic depth with high information content at intervals where the sedimentation rate is constant across the window size (3.3m) and low where the sedimentation rate changes within the window. In any case, we note that the algorithm identifies the correct (true) sedimentation rates despite the high noise level, and that it reconstructs the large discontinuities between layers of constant rates quite well, with lower probability near abrupt sedimentation rate changes.

11.

I would also recommend the authors to have all the figures 3-4-5-6-7 plotted as a one-pager figure together. This will allow for a much easier comparison between the single-element run and the multiple single-element run. Additionally, to tie this comment back to the previous one, please quantify the difference between the two runs, rather than having these results as visual examples. And add the elemental curves used for each runs to these figures. Doing so will allow the authors to expand the discussion to the interpretation of the succession they have studied in great detail.

We have now added the elemental profiles used for each run to the figures.

While figures 3-7 can be plotted on one page, we are concerned about the readability of the details in such a 1-page figure. Therefore, we do not recommend to compress all this into 1 page. We will leave it to the editor to give us advice on this matter.

And finally, we respectfully disagree with the reviewer that our two runs are just "visual examples". These plots are equipped with scales and color bars providing a complete quantification of the results.

12.

Figures and presentation.

I would invite the authors to highlight and indicate more features on their figures: for example, where are the sedimentation rates changing “abruptly”, where are the “spurious features” that “could be interpreted as true sedimentation rate” (l. 289-290), etc.

Yes. We have clarified “spurious” and rewritten the paragraph, so it highlights features observed in figure 3. Line 281ff now reads:

However, the test also illustrates how the probability distribution outside the correct sedimentation rate is not invariable or zero. In fact, artifacts (mathematically possible, but incorrect sedimentation rates) are introduced by the noise, the variability of the true model, window size, and the non-linearity of the problem. Some of these artifacts show up as spurious parallel features that could potentially be misinterpreted as representing true sedimentation rates. This highlights a key challenge in cyclostratigraphic analyses: a given cycle may be interpreted as corresponding to different target periods, resulting in a multimodal sedimentation-rate probability distribution. Nevertheless, the highest probability remains associated with the true reference model

13. I would suggest adding element-curves to Fig. 3-6-10 next to the information content curve, which x-axis need to have a unit or an explanation as to why it doesn't have one. Add proposed sedimentation rate curves on all probability plots, just like you have done on Fig. 10. The authors are also invited to quantify the difference between the two runs.

As also mentioned above, element curves are now added to figures 3, 6 and 10. The information content has no unit[AS1], see definition in equation (13).

14.

The authors are also invited to combine Fig. 8-9-10 in a one-page figure as well. Please rotate the Fig. 9 to have the depth as a vertical axis, just like all the other figures. Please quantify the impact of “However, the test also shows how artifacts are introduced by the noise, the variability of the true model, and the non-linearity of the problem” (caption of Fig. 10), which are concept that should be discussed in greater detail by the authors, especially with regards to the results of the studied core Fågeltofta-2.

We respectfully disagree here. We are concerned about the readability of the details of the result, if fig. 9 was rotated. Again, we will leave it to the editor to give us advice on this matter.

15. Please check the journal reference formatting, as there seem to be many unnecessary parentheses when multiple references are cited in the text.

Yes. We have corrected (all of?) these..

16. Please chose and stick to “ky”, “kyr”, or “kyrs” for consistency. Similarly, always call the core Fågeltofta-2, rather than a mix of Fågeltofta-2 or Fågeltofta (e.g. caption of Fig. 2, or l. 304, 326)

Corrected. We now use 'ky' and 'Fågeltofta-2' everywhere.

17. Please use the official chronostratigraphic series name Furongian as opposed to late/upper Cambrian.

We corrected line 280 and note that later Cambrian sometimes refer to Miaolingian-Furongian in a looser sense.

Response to Reviewer 2 (Matthias Sinnesael)

Reviewer's comments

18. Recognition, discussion, and comparison of previous related work: especially the (evolutionary) average spectral misfit work presented by (Meyers & Sagemann, 2007, AJS: <https://doi.org/10.2475/05.2007.01>) should be incorporated. Also see other suggestions below, e.g. Sinnesael et al., 2016 on instantaneous estimates.

We revised the reference to the Evolutionary average spectral misfit work by Meyers & Sagemann, 2007 in line 32. It now reads:

“Traditional astrochronologic approaches typically rely on matching repetitive spectral features in stratigraphic depth-series data to a target set of orbital frequencies, either through spectral filtering (Hays et al. (1976); Weedon (2003); Meyers et al. (2008, 2001)), or (evolutionary) average spectral misfit against numerical astronomical solutions (Meyers and Sagemann (2007), TimeOpt: Meyers (2015), TimeOptMCMC: Meyers and Malinverno (2018); AstroGeoFit: Hoang et al. (2025)) or evolutionary spectral correlation (eCOCO: Li et al. (2018), eTimeOpt: Meyers (2019)).

19. A general case could be made for why multi-channel and probabilities might be good to introduce, it does not need to be motivated by (a challenging) Cambrian case study alone.

We agree. In our introduction (see line 65) we write:

"This framework is implemented in a newly developed algorithm, AstroComb, which builds upon and extends the concepts behind earlier astrochronological tools,...".

We see this as a general motivation, rather than one rooted in the Cambrian case study only.

20. Explain explicitly what uncertainties are reported here (error on the fit only), or not. What about uncertainties in the astronomical component selections, etc...

In line 167, it is explicitly stated that Milankovic cycles are known. In the definition of the likelihood (starting at line 174) it is explained exactly how the uncertainty of the fit between observed spectrum and model spectrum is related to the local standard deviation of the spectrum (which, in turn, is derived from the standard deviations of the data).

21. More user guidance in the script and/or “Readme” file would be beneficial for user experiences.

We agree with the reviewer's observation. While the current README file contains all the necessary information, it could be made more user-friendly in terms of presentation and accessibility.

At present, our priority has been to ensure that the codebase provides sufficient detail to allow full reproducibility of the results reported in this paper. In parallel, we are actively developing a faster and improved version of the algorithm, for which we plan to provide a more comprehensive and user-oriented manual in the future.

Given these ongoing developments, we believe that our efforts are best focused at this stage on delivering a well-documented and reproducible implementation, while directing users to the latest updates and iterations of the code via our Zenodo repository.

22. Explain more clearly, maybe in different wording, what exactly is meant by “non-linear”, “discontinuities”, ...

Corrected. For example, in section 3.1, 2nd paragraph:

“A severe test with artificial data forced by a sinusoidal Cambrian Milankovitch forcing overlain by 200% red noise and showing significant sedimentation rate discontinuities,...”

We have reduced the use of “Non-linear” whenever it could be replaced by abrupt changes in sedimentation rate profile or alike. Now, we talk about “non-linearity of the problem”, meaning that the equations relating the sedimentation rates to the elemental data are not linear equations.

23.

The real case study represents a re-analysis of a record that is already interpreted in terms of cyclostratigraphy. It would be valuable to explicitly compare both. What is potentially different when using AstroComb? Or not. Geological implications? Etc...

Corrected. We have now added previously published sedimentation rate-depth relation and a time-depth relations calculated using more traditional cyclostratigraphic approaches.

24.

Maybe most importantly for a paper presenting a new method/algorithm. The Discussion opens with “*Extensive tests were carried out to study the accuracy and stability of the algorithm.*” But the current manuscript starts with the presentation of a (Cambrian) real case study, briefly discusses a synthetic example in the discussion, but a more systematic documentation of testing with various synthetic examples, a maybe simpler real case study, to end with a complex Cambrian case study could be valuable to really document how the new method works. Comparison with existing methods that do similar/related analyses might also be informative. Part of such work could be presented in supplementary materials.

Corrected. The results now start off presenting successful tests on synthetic data and follows up with real data. The real data test is actually “simple” in the sense that Milankovitch cycles have already been detected and the lithology is fairly uniform with optimal lamination.

Reviewer 2's Detailed comments (with main comments highlighted in bold font):

25.

L43-49: Shales typically have a lower sedimentation rate compared to many other sedimentary deposits. Is low sedimentation rate what is meant with “shorter stratigraphic thickness representing shorter time scales”?

This expression has now been deleted.

26.

From the current motivation it is not clear why these particular type of records would benefit from a probabilistic perspective? Can there no argument be developed for the advantages of a probabilistic approach in cyclostratigraphy in general? And then a particular case study can be presented to illustrate the newly developed method?

Yes! This comment resonates strongly with our core principle: any physically meaningful estimate should be accompanied by an assessment of its uncertainty. In this sense, a probabilistic perspective is fundamentally advantageous for data analysis.

For example, if a non-probabilistic method yields a sedimentation rate of 6 mm/ky for a given interval, but provides no uncertainty estimate, it becomes impossible to assess the reliability of that value. Without quantified uncertainty, one cannot determine whether the true rate might plausibly be close to 0 mm/ky, 10 mm/ky, or somewhere in between. A probabilistic framework, by contrast, explicitly characterizes such uncertainties and thereby enables more informed interpretation of the results.

We added a sentence about this. Now Line 59 reads:

“Our primary objective is to estimate sedimentation rates within stratigraphic sections while explicitly quantifying their associated uncertainties.”

27.

L65-67: What about the average spectral misfit (ASM) work, and the evolutive adaptation (eASM), done by S. Meyers and B. Sagemann (e.g. Meyers & Sagemann, 2007, AJS: <https://doi.org/10.2475/05.2007.01>). This seems to be a key reference that is missing and deserves incorporation in the introduction/discussion.

Reference is now included in line 41:

“Early methods, such as Average Spectral Misfit (ASM) evaluates spectral peaks against astronomical frequencies across sedimentation rates and was later extended to moving-window analysis (eASM; Meyers and Sageman, 2007)”

28.

L76: in the case of for example XRF multi-channel data, many elemental variations are probably correlated, is this considered in the “full probability distribution”?

Yes, this is an important topic that we now address more carefully. There are several reasons for why channels can be correlated. First, elements that are mainly hosted in the same mineral, e.g. Fe and S, will be correlated. Second, Milankovitch forcing influences several elements with the same imposed rhythmicity (that's what we are after). Third, one major element may dilute the other, creating a negative correlation on one element even if the element is not Milankovitch forced. Fourth, two channels can be mathematically dependent, for example Si and 1/Si or Si and Fe/Si if the Fe variance is small compared to Fe.

This is a topic which is best resolved with “expert knowledge” on a case by case basis. The way, we investigate whether multichannel truly adds information is by choosing

- a) elements that are not strongly coupled mineralogically or in same molecular compounds
- b) elements that are not simply diluting each other (e.g. positively correlation).

Non-Milankovitch (periodic) forcing may create correlated elemental variations, but we rely on the algorithm to not “comb out” these as Milankovitch period ratios.

Here, we have chosen to Al, S, and Mn. These are elements that are not mainly hosted in the same mineral. Although Al and S concentrations are negatively correlated and might dilute one another, our previous studies have concluded that each element has been Milankovitch forced simply because there are intervals in the Alum shale where the negative correlation and Milankovitch periods are recorded in one of the elements. That is, we observe periodicities occurring in only one of the elements in one interval and in the other element at other intervals.

We clarified this in line 303ff:

Often elements or element ratios are used as channels. Yet, XRF elements also covary because they share mineral hosts, are affected by dilution, or are linked through mathematical relationships, and such covariance can create a misleading impression of increased certainty rather than reflecting independent signals of Milankovitch forcing. For this reason, it is important to choose elements that are not cohosted, not simple dilutions of one another, and not strongly coupled, a choice that relies on expert knowledge of mineralogy and geochemistry. Al, S, and Mn meet these criteria in the Alum Shale. Although Al and S often show negative covariance, earlier studies demonstrate that each element records Milankovitch periodicities in intervals where the other does not (Sorensen et al 2020, Zhao et al. 2022), indicating that their orbital signals are not only products of dilution or other non orbital processes.

29. L78: The “Fernandes et al. (2026)” reference can currently, 26/03/2026, not be found on the mentioned GMD website, or is not available to the reviewers? This is not according to the guidelines of the journal.

This reference is now removed.

30. L100; “detect Milankovitch signals”. Does AstroComb aim at identifying potential Milankovitch cycles, or can it be applied when it has been demonstrated/suggested that certain Milankovitch cycles are present in a signal/record?

Like any other cyclostratigraphic toolbox, AstroComb can both be applied to validate and to detect Milankovitch cycles.

31. L101: it would be interesting to know what exactly the (sources of) “uncertainties” are. E.g. uncertainties on the fit? Uncertainties in component identification? Uncertainties in the periodicity of the astronomical cycles for the Cambrian?

This is now explained in section 2.4: Computing the Likelihood of a Sedimentation Rate) line 126ff:

In our computation of the likelihood, data uncertainties are provided by the user as the probability that a peak in the observed spectrum is a Milankovitch peak. Existing data-fitting-based methods (Meyers and Sageman (2007); Malinverno et al. (2010), TimeOpt: Meyers (2015), TimeOptMCMC: Meyers and Malinverno (2018); AstroGeoFit: Hoang et al. (2025)) are purely based on misfits between data (elemental records) and synthetic signals (generated from assumed Milankovitch periods), but AstroComb is instead aimed at simulating the data uncertainty evaluation carried out by practitioners analyzing observational data. In traditional misfit calculations you not only need experimental uncertainties, but also components of the data that are not modeled by the Milankovitch cycles. The latter are significant, but not directly available (for example, The TimeOptMCMC algorithm Meyers and Malinverno (2018) estimate them through a Hierarchical Bayes approach). However, in our approach we are automatically taking all data uncertainty sources into account, including the practitioner’s confidence in identifying spectral peaks.

Theory and Methods:

32.

L121: “certain”, so it is not important that all the individual channels contain an imprint of astronomical forcing? E.g. I can feed all the XRF core scanning elemental data to the algorithm, no matter if they have an astronomical component or not, and it will not affect the performance/uncertainty estimates?

No, not all channels need to contain Milankovitch modulations. And yes, you can feed all the XRF data to the algorithm. This is why any such limitation are not described in the text.

33. L129: **is this “stretching” and “compressing” done linearly or not?** And could this be a different with the ASM approach of Meyers & Sagemann (2007). Fig.1 looks a lot like Fig.1 in M&S 2007, and seems to suggest a linear scaling? L165 and Eq. (2) suggest a constant r per window as well.

As shown in Equation (2), the “stretching” and “compressing” is linear in each analysis window. But the obtained sedimentation rate applies only to the center of the window and the windows centered at points immediately above and below are treated independently. This means that the sedimentation rates computed from windows with nearby centers do not vary linearly with depth.

34. L177: **in what units are a and b expressed?** Spectral power? Significance level – but what noise model is then used? E.g. see discussion in Meyers (2012, PP):

<https://doi.org/10.1029/2012PA002307>, or Weedon (2022, ESR):

<https://doi.org/10.1016/j.earscirev.2022.104261>

As shown in section 2.3, in line 7 of item 1 in the explanation of the likelihood (now on page 5), the units of a and b are the same as the amplitude spectrum F of the considered signal (for instance: $a = \langle F \rangle + \sigma_F$).

35. The MATLAB script contains a relative long list of input variables, with only a short comment (e.g. not discussed in readme file) like “% *Relative uncertainty input basic elements data*” it is not clear to the user (reviewer in this case) what this exactly means. The determination of every parameter should be clearly explained to a new user who might not be familiar with the method, or particular dataset used here.

Corrected.

The comment “% *Relative uncertainty input basic elements data*” should actually be “% *Relative uncertainty of input basic elements data*” and refers to data uncertainty from repeated scans of the same interval.

The revised AstroComb code will be made available upon acceptance of the manuscript.

Also, we note that we plan to create a user-friendly manual in forthcoming versions of the AstroComb code that will explain each input variable more carefully.

36. Results:

Would it be more educative to first show how the new algorithm works on a simple synthetic example, instead of one real-world case study. One might wonder how the algorithm might perform with a series of different input parameters for the same case study, or how an application to a different case study might look like. One application with one (optimized) set of parameters might give a limited overview of some of the potential model sensitivities. One can always design more sensitivity tests of course, but (the reporting of) none is rather limited when presenting a new method.

Corrected. We have changed the order. The results section starts with synthetic data and follows up with application to a real geochemical dataset.

37.

L244: the model is presented as multi-channel, after which an analysis of a single element (Al) is presented. Maybe a few sentences could be added explaining the reasoning behind this (step-by-step) approach?

We present both single-channel and multi-channel data in the paper. This is now explained

38. L266: Why S, Al and Mn? Why not all elements? See previous questions on element selection. What do these proxies mean? Correlated or not, in relative variations?

See point 28 above. We had to make a choice of elements. We have now added a justification and a guideline for choosing independent lines of data for multi-channel cyclostratigraphy.

39.

L287: what about the discontinuities? Or does that mean, “sudden changes in sedimentation rates”? “Discontinuities” might also be interpreted as breaks in sedimentation, hiatuses. Is this what is meant with “non-linear” in the title as well? If so, this is not so clear in its current formulation, and might be worth reformulating, spelling out a bit more.

There is a discontinuity in a property when the property shows a large change over a short interval of the independent parameter (depth, time, etc.). A hiatus would also generate an abrupt change

We clarify what is meant by discontinuous sedimentation rates in the synthetic example, line 237ff

The synthetic sedimentation rate model (figure 3) is characterized by layers with sedimentation rates separated by significant discontinuities which could reflect abrupt changes in the climate such as going from coarse grained quartz sand to fine-grained mudrock and/or a pause in sedimentation (hiatus) that may also leave a geochemical imprint on the XRF data.

40. L304: there is no explicit comparison with the previous published cyclostratigraphic interpretations from the same sections (see two papers referenced). Why not add to current Fig.7 the sedimentation rate profiles from those papers? And discuss how these compare. Did AstroComb find the same, or not? Especially as several co-authors were involved in the previous and present work. Is it better or not? Etc...

It would also be valuable to analyse the same record with existing techniques to see how the AstroComb performance relates to existing algorithms (e.g. eASM, eTimeOpt, Spectral Moments etc...

We have now added a sedimentation rate-depth relation, and a time-depth relation calculated from previous cyclostratigraphic analyses (Sørensen et al. 2020).

41.

It might be nice to plot figures 4 and 6 together, so that one can easily assess the difference, without having to look at different pages. Same for Figures 5 and 7 – they can easily be combined.

Agreed. We now show the pdf's for Al vs Al-Mn-S on the same page. Also, we show the age-depth model for Al vs Al-Mn-S on the one page.

Reviewer 2's minor detailed comments:

42

Line(L)17: why do the anchor points need to be astronomical, and for example not radio-isotopic dates?

They dont. Corrected/adjusted

43.

L26: "... these Milankovitch cycles". The previous sentence is very general and does not introduce the concept of Milankovitch cycles.

Corrected. Deleted.

44.

L28: would it be specific spectral features, or repetitive features in a depth series, but not repetitive features in a spectrum?

This expression has now been deleted from the introduction.

45.

L29: astronomical frequencies would be a wider term here, as strictly speaking the orbital parameter is limited to eccentricity (i.e. actual changes to the orbit of the Earth around the Sun).

All the involved planets have orbits, so we believe 'astronomical frequencies' is adequate in what is now line 45..

46.

L31-33: Does TimeOpt not rather assess a fit relative to certain astronomical frequencies (optionally combined with the assessment of amplitude modulation patterns in the converted time series) rather than comparing against astronomical solutions?

It is based on fits between measured data and computed Milankovitch cycles. The latter can, in our view, be termed 'astronomical solutions'.

47.

L41: diagenesis happens after sediment accumulation

Yes, we have now clarified that sedimentation rate is, in fact, used as all mass accumulation throughout the paper. Line 55ff now reads:

Sedimentation is an inherently episodic process (Kemp and Sexton, 2014), influenced by hiatuses, erosion, rapid depositional events, and lithologic transitions. Further, final mass accumulation is affected by diagenetic processes, including diagenetic cements, authigenic minerals and recrystallized phases. Milankovitch insolation forcing may influence deposited grains as well as diagenetic products. Other cyclic processes, whether forced by biogeochemical resonances or autogenic processes (Zhao et al., 2023; Paola, 2016; Munnecke et al., 2001). These processes may introduce

abrupt local variations in mass accumulation rates (in the following simply termed 'sedimentation rates') that are difficult to capture using approaches that assume smoothly varying sedimentation.

48.

L55: on the topic of instantaneous (non-stationary) sedimentation rate estimates (with a limited component of uncertainty estimates) in cyclostratigraphy, see for example Sinnesael et al. (2016, GMD) and references therein: www.geosci-model-dev.net/9/3517/2016/

Reference to Sinnesael et al. 2016 is now added in Line 48.

49.

L59: explain how a piecewise continuous process is different from for example windowed approaches, some of which cited earlier in the manuscript, or other methods suggested to be able to deal with abrupt changes in sed rates (e.g. Sinnesael et al. (2018, PP) and references therein: <https://doi.org/10.1029/2017PA003293>)

Reference to Sinnesael et al. 2018 is now added to line 49.

The discussion of how a piecewise continuous process is one way to model local, abrupt changes is left implicit in our introduction with citations to these references.

49b

L68: specify “high-speed computing” and “large datasets” in this context. It might be nice to already mention the programming language used as well.

These expressions have now been deleted.

50.

L69: see earlier suggestion on windowed approaches

See above

51.

L71: are these “discontinuities” detected by AstroComb, or can they be included by the users based on external information, important to explicitly formulate.

Corrected. See also comment above.

52.

L77: clarify why the sub-precession scale is so important here?

We have removed this statement, since it refers to not-yet-published work.

53.

L78: explain what “ProBE4T” is.

We have removed this, since it refers to not-yet-published work.

54.

L79: upper Cambrian... Formation

Corrected

55.

L80: give thickness (m)

Corrected.

56.

L82: is this “so extremely slow”, e.g. in pelagic settings?

Expression deleted.

57.

L87: “reveal” as is newly detected, not interpreted in the previous studies?

Yes, but we deleted this expression.

58.

L89: not clear what “chemical oscillations” mean here.

Expression deleted.

59.

L93: if it is optimised for a specific application, what would a suitable minimal “high-resolution” be? E.g. x samples per Y cycle?

This part has now been deleted.

60.

L101-112: these do not seem to be four components of the method.

We are not sure what this comment refers to, but we assume section 2.2 Target Astronomical Spectrum. The four components was listed twice in the previous version of the manuscript with distinct age assignments (494 Ma and 497 Ma). This has now been corrected in line 227ff.

61.

L113-117: mostly repetition

Yes.

62.

L128: there is some uncertainty for Cambrian “predefined target astronomical spectra” – how does this affect AstroComb?

Yes, the Milankovic cycles are only well-known for the past ~50 My. In a forthcoming version of AstroComb, we are including an estimation of the astronomical cycle periods with uncertainties and will then be able to answer this question.

63.

L150-156: an orbital solution will typically only consider the eccentricity part, not for example obliquity and precession, “astronomical solution” might be more appropriate.

Corrected. Line 124 now reads:

Because no closed form astronomical solution exists beyond about 50 Ma

61b

The F2022 and W2015 solutions have different estimates for Cambrian values, and R. Zeebe also has different solutions available (<https://www.sciencedirect.com/science/article/pii/S0012825224002873>, and: <https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2024PA004861>). How exactly are the suggested values determined. Is the uncertainty in the astronomical solutions reflected in the “full” AstroComb uncertainty estimate? It should be specified what the assumptions and approximations are (e.g. the different astronomical components are not single sinusoids).

See also comment 62 above. We have now stated directly that we are currently using these periods without uncertainties.

64.

L180-182: define “local”. Are these the mean and standard deviation from the different channel spectra? If so, see previous question on inclusion of channels without astronomical signals.

As explained in line 163ff, $\langle F \rangle$ is a local mean of the spectrum around the considered wavenumber k . That is, F is averaged over an interval in the spectrum, centered at k . Currently, the interval length is 1. We clarified this so the sentence now reads:

Typical values are $a = \langle F \rangle + \sigma F$ and $b = \langle F \rangle + 3\sigma F$, where $\langle F \rangle$ is a local mean of the spectrum around the considered wavenumber k over an interval of length 1, and σF is the local standard deviation of the spectrum

65

L190: what are the window sizes used for the calculations of the spectra? The provided MATLAB script mentions “16384”? But what is the unit? Number of samples? mm? What is the influence of the window size for example?

The MATLAB script says that the "Length of sliding window for spectral analysis" is 16384. The length of the window is the number of elements in the corresponding vector. In the line below it says that the "% Borehole sampling distance [mm]" is 0.2. Hence, the window length is $0.2 \cdot 16384 = 3276.8$ mm (that is, 3.3m as stated in line 266).

66.

L232: give exact core depths of studied interval

Corrected. The length is actually closer to 21 m than 20 m, so this is also adjusted.

67

L238: if it is 18 m, one can just use 18 m (instead of ~20m, ~19 m...)?

Well. The full length is 21 m, but we lose half a window-length at each end because of the spectral analysis in the windows.

68

L240: ~~area~~

Corrected

69.

L247: why a window of 3.3 m?

See above.

70

L251: the Kullback-Leibler information measure should be introduced and explained in the Methods, not within the Results.

The Kullback-Leibler information measure is now explained in the Methods section line 200ff.

Finally, we calculate the Kullback-Leibler information measure at each depth z showing how much Milankovitch information is present in the data as a function of depth (Kuhlback & Leibler 1951). This is obtained from:

$$\mathcal{I} = \int_R p_z(r) \ln \left(\frac{p_z(r)}{u(r)} \right) dr$$

where $p_z(r)$ is the calculated probability distribution of r at depth z , R is the range of sedimentation rates, and $u(r)$ is a uniform (constant, and hence non-informative) distribution. Theoretically, the Kullback-Leibler information measures the expected number of yes/no questions needed to update our state of information from what is given by $u(r)$ (here total ignorance) to what is given by $p_z(r)$ (here, the posterior distribution).

71.

L255: what does this uniform distribution mean? How is it defined?

It means constant. Now explained in the text.

72.

L255-256: “Figure 3 shows that Milankovic cycles information is present in the core”. This is not intuitively clear, please explain in more detail.

Revised. This sentence has now been deleted and the AstroComb results for one channel and multiple channels are now explained in two subsections 3.2.1 and 3.2.2.

73.

L267: which “resolution”?

Revised. In this context, ‘resolution’ was used as ‘spectral resolution’. In the revised text, this expression is no longer used.

74.

L277: See earlier comment, I might be nice to document some of these tests in the supplementary information of the paper.

See comments above. We have now added previously published a sedimentation rate-depth relation, and a time-depth relation. We believe the updated information is now sufficient to document how the new method works.

75.

L278; “severe” is a bit of an arbitrary term

Now line 241. The exact noise level (200%) is given immediately after.

76

L278: what type of noise? White noise?

Red noise, as described in line 241.

77.

L281: should refer to Fig. 9?

Corrected. Figure numbers have been updated.

78.

L287; any idea why you get these artefacts? (e.g. other ratio`s that could fit a certain combination of astronomical components as well)?

Yes. This is likely the case. Line 256ff now reads:

However, the test also illustrates how the probability distribution outside the correct sedimentation rate is not invariable or zero. In fact, artifacts (mathematically possible, but incorrect sedimentation rates) are introduced by the noise, the variability of the true model, window size, and the non-linearity of the problem. Some of these artifacts show up as spurious parallel features that could potentially be misinterpreted as representing true sedimentation rates. This highlights a key challenge in cyclostratigraphic analyses: a given cycle may be interpreted as corresponding to different target periods, resulting in a multimodal sedimentation-rate probability distribution.

79.

L294: it would be nice to see the uncertainty component applied for the artificial test case.

The full probability distribution (full uncertainty landscape of the sedimentation rates) is plotted in Figure 3.

80.

L297-300: Indeed, see earlier comments. Could this sensitivity be tested?

See above.

81.

L310-311: see for example discussion on noise contributions to spectral properties (for spectral moments approach) in Sinnesael et al. 2018

Reference added. See comment above.

82.

L326: “discovered abrupt changes in sedimentation rate”, what are these? Geological implications? This aspect deserves to be detailed more.

Clarified in line 488ff;:

Sedimentation rate shifts in the Alum shale Formation (incl. Billegrav-2 and Fågeltofta) are correlated to relative sea level changes obtained from sequence stratigraphy Sørensen et al. (2020). Yet, previous sedimentation rate estimates obtained using eCoco and a smoothing spline fit on visually profound periods in the wavelet spectrum unavoidably induced some degree of smoothing of the sedimentation rate, which may give a false impression of the actual sedimentation rate changes. With AstroComb, sedimentation rate changes are not necessarily changing smoothly across stratigraphy.

83.

L335: the multi-channel component is presented as one of the major innovations of this study (next to the probabilistic component), but its features are only discussed in a minimal way in the present manuscript (i.e. one of the three presented analyses). It would be valuable to show in more detail what this approach adds, or not, and document related sensitivity studies. It could also be worth spelling out if the multi-channel component is an averaging of single channel information, or the algorithm actually looks at the relationships between the various analyzed channels (or not).

It is important to distinguish between the method and the data here. What the multi-channel approach adds is a data issue. How the multi-channel information is processed is a methodological issue. As is clearly described in Section 2.4: 'Computing the Likelihood of a Sedimentation Rate', line 187-195, is not an average of information from single channels. Each channel is analyzed individually, and the results from the channels are combined using well-described, probabilistic rules.

84.

L336; Explain what is meant with “quality control”.

Revised. L363 now reads:

It generally provides several plausible solutions for the sedimentation rates at given depths, allowing the user to choose, based on geological/stratigraphic knowledge."

L399: L406: Cambrian

We do not see an issue in lines 399 and 406. We checked– 'Cambrian' is now correctly spelled throughout the paper.

Response to Reviewer 3

85.

This study proposes a quantitative statistical methodology for astronomical time scale calibration and development, designated as AstroComb. The objective of the study is an important one that has broad relevance to the geoscience community. Astrochronology has become a central approach in the calibration of the geological Timescale (GTS 2020: Gradstein et al., 2020), the evaluation of Earth System evolution (Hays et al., 1976; Pälike et al., 2006), and the reconstruction of the history of the Solar System, including Earth-Moon history (Ma et al., 2017; Meyers & Malinverno, 2018). Consequently, improvements in astrochronologic methodologies, especially uncertainty quantification, can result in advancements across multiple domains of geoscience and also astronomy. I'm delighted to see new work on this topic, and to provide feedback.

In evaluating statistical methods such as the one presented in this manuscript, there are three criteria that are essential to consider:

Criteria 1: Does the proposed methodology have a solid theoretical statistical foundation (in other words, is the method theoretically valid)?

Criteria 2: Does the proposed method differ substantially from existing published methods, and does the scholarship recognize the prior related methods?

Criteria 3: Are the concepts of the methodology as described in the manuscript implemented appropriately in the coded software, such that it can correctly accomplish what it proposes to do.

I appreciate that the authors have provided access the AstroComb Matlab code on Zenodo (<https://zenodo.org/records/17966228>), which permitted a comprehensive evaluation of all three criteria. I will provide a detailed assessment below, but first I present a brief summary. In principle, the methodology looks to have solid statistical foundation (Criteria 1), although there are some crucial details that need to be addressed in the manuscript before this can be fully demonstrated. The proposed method is conceptually similar to a method previously published by Malinverno et al. (2010), which is not referenced in the present manuscript (Criteria 2). An important innovation of the present study is the evaluation of multi-channel data. Finally, the presentation of the methodology in the manuscript is not yet sufficient to appropriately convey what the AstroComb Matlab code is doing (Criteria 3). Once these issues are addressed, as well as a number of other improvements to the manuscript presentation for the sake of clarity and accuracy, I would recommend acceptance of the manuscript by Geoscientific Model Development, as an appropriate venue for publication.

I hope that the authors find the suggestions in this review helpful, and I would be happy to serve as a referee for a revised version of the manuscript that incorporates the recommendations.

Thank you for the enthusiastic response, helpful review, and constructive comments. We have added references and revised the text accordingly. See details below.

86.

2 The historical context of astrochronologic testing and calibration methods, with respect to the proposed AstroComb methodology.

Below I provide some background on the history of astrochronologic method development – for which there is no thorough up-to-date review available (to my knowledge). I do not expect the authors to incorporate all of this review in their manuscript; it is presented here to communicate to the authors how their proposed method ‘fits’ within the existing methodologies, for appropriate scholarship/citation.

Thank you! Inspired by this, we have revised the introduction to cyclostratigraphic methods. Line 40ff now reads:

Over the past two decades, several classes of inverse astrochronologic methods have been developed to address this problem. Early methods, such as Average Spectral Misfit (ASM) evaluates spectral peaks against astronomical frequencies across sedimentation rates and was later extended to moving-window analysis (eASM; (Meyers and Sageman, 2007)). Bayesian spectral methods later formalized this framework by evaluating the power concentrated in astronomical frequencies relative to a noise model formulated as a likelihood function (Malinverno et al., 2010; Peng et al., 2023; Trayler et al., 2024). Parallel developments introduced time-domain approaches including TimeOpt (Meyers, 2015) and time-variant sinusoidal modeling (Sinnesael et al., 2016; ACE v.1) along with extensions (Sinnesael et al., 2018; Meyers and Malinverno, 2018; Malinverno and Meyers, 2024; Hoang et al., 2025), while correlation-based methods such as COCO and eCOCO evaluated agreement between measured and theoretical power spectra (Li et al., 2018).

87

2.1 SUMMARY: THE FOUR CLASSES OF METHODS

The problem of calibrating and testing cyclostratigraphic records in ‘deep time’, beyond the limits of the theoretical astronomical solutions (approx. 60 Ma; e.g., Laskar et al., 2004; Laskar, 2020), is an important one. There are essentially four classes of inverse approaches that have been proposed to evaluate timescale uncertainty and simultaneously derive an optimal sedimentation rate in such ‘deep time’ cyclostratigraphic data. (Note: there are many other quantitative cyclostratigraphic methods that are not summarized here, but those listed below are inverse methods that constrain timescale simultaneously while evaluating the fit of data to astronomical frequencies, beyond the 60 Ma limit of the theoretical astronomical solutions. They do not require initial assumptions relating a particular bedding cycle to a particular astronomical period.)

CLASS 1: The first type of ‘deep time’ inverse approach for comprehensively evaluating timescale uncertainty in cyclostratigraphic data was developed by Meyers & Sageman (2007), known as “Average Spectral Misfit” or ASM. This approach identifies statistically significant power spectrum peaks in cyclostratigraphic data, and tests the identified frequencies (cycles/meter) against predicted astronomical frequencies (e.g., originally from Berger et al., 1992), using a grid of sedimentation rates to evaluate plausible timescales. The ASM method accounts for resolution limits of the data and astronomical target frequencies, and also provides a Monte Carlo null-hypothesis significance test. The method was adapted to use a moving window (eASM) to allow evaluation of records with unsteady sedimentation rates (Meyers et al., 2012).

CLASS 2: The second type of ‘deep time’ inverse approach for comprehensively evaluating timescale uncertainty in cyclostratigraphic data was developed by Malinverno et al. (2010), and this was the first formal Bayesian inverse method. Instead of identifying the location (frequency) of significant power spectrum peaks (as in ASM), the Malinverno method calculates the power spectrum and evaluates the power concentrated in astronomical frequencies, relative to a noise model (formally, as likelihood). Trayler et al. (2024) adapted the Malinverno et al. (2010) approach, further incorporating radioisotopic constraints (AstroBayes). Another method that explicitly evaluates the concentration of power at astronomical frequencies following calculation of the power spectrum is the ‘power ratio accumulation method’ (Peng et al., 2023).

CLASS 3: The third type of ‘deep time’ inverse method for comprehensively evaluating timescale uncertainty in cyclostratigraphic data was developed by Meyers (2015; TimeOpt), and later adapted and enhanced (Meyers & Malinverno, 2018; Meyers, 2019; Malinverno & Meyers, 2024; Hoang et al., 2025). Below, I present a history of the development of this class of methods.

- o Meyers (2015) introduced the regression model framework for astrochronologic testing in the “time domain”, rather than the “frequency domain” (in contrast with CLASSES 1, 2, 4). Meyers & Malinverno (2018; TimeOptMCMC) further developed the approach as a Bayesian inversion methodology “to quantitatively link astronomical theory with geologic observation, allowing a reconstruction of Proterozoic astronomical cycles, fundamental frequencies of the solar system, the precession constant, and the underlying geologic timescale, directly from stratigraphic data.” TimeOptB and TimeOptBMCMC (Malinverno and Meyers, 2024) introduced improvements/enhancements of the original TimeOptMCMC method. All of these methods require relatively stable sedimentation in the analysis interval.

- o The TimeOptTemplate and eTimeOpt methods (Meyers, 2019) were designed to evaluate unsteady sedimentation rates within the analysis interval (non-Bayesian), and the AstroGeoFit method (Hoang et al., 2025) introduced a genetic algorithm coupled to the TimeOptMCMC/TimeOptBMCMC Bayesian regression model framework to evaluate unsteady sedimentation histories.

- o Note that TimeOpt (Meyers, 2015), TimeOptTemplate (Meyers, 2019), TimeOptB (Malinverno & Meyers, 2024) and AstroGeoFit (Hoang et al., 2024) also provide a nullhypothesis significance test, to help guard against false attribution of observed stratigraphic signals to astronomical forcing.

CLASS 4: The fourth type of ‘deep time’ inverse approach for comprehensively evaluating timescale uncertainty in cyclostratigraphic data was developed by Li et al. (2018; COCO and eCOCO). This method compares the shape of the measured cyclostratigraphic power spectrum to a theoretical astronomical target power spectrum, using a Pearson correlation coefficient, following the calibration of the measured power spectrum across a grid of different plausible timescales (sedimentation rates). The method also provides a Monte Carlo null-hypothesis significance test. I will note that a fundamental challenge in the execution of this approach is the lack of knowledge, a priori, about the appropriate ‘shape’ of the astronomical target spectrum (the relative importance of eccentricity, obliquity and precession components). The other methods listed above (CLASS 1-3) do not have this limitation.

Considering this historical context, the proposed AstroComb method fits into CLASS 2, and is similar in its fundamental concept to the method of Malinverno et al. (2010), which evaluates a range of sedimentation rates and seeks to maximize spectral power (formally, as likelihood) at the astronomical target frequencies. More on that is presented in the review below.

We have revised the introduction and given a more careful introduction to the available methods in Line 40-55.

88

3. An evaluation of the AstroComb methodology

3.2 AstroComb methodology described in the manuscript

- The manuscript presents a description of the AstroComb methodology in Section 2. A key step is the generation of a “synthetic spectrum” (Equation 2), as a function of sedimentation rate (scalar, r), and the amplitude of the astronomical target frequencies (vector, a). With respect to this, the discussion in the manuscript gives an incorrect impression of how the analysis is actually conducted. Evaluation of the Matlab code indicates that Equation 2, in its full form, is not being used to generate the “synthetic spectra” as suggested (lines 164-169), as the amplitudes are not considered (vector, a). Furthermore, there is discussion in the manuscript regarding the shape of the spectral peaks in the synthetic spectra (vector, w), but the shape of spectral peaks is not considered in the “synthetic spectra” generation in the Matlab code. Instead, the code is concerned with sedimentation rate, and its impact on the location of the target astronomical frequencies (step 3.2).

There is an important nuance in reviewer 3’s understanding of AstroComb that we need to correct.

*The synthetic spectrum is encoded in 3 mathematical objects: (1) the vector of Milankovic wave-numbers $\mathbf{k}$, (2) the vector $\mathbf{w}$ with the shape of the spectral lines, and (3) the vector $\mathbf{a}$ of amplitudes of the spectral lines. For each value of the sedimentation rate, we first compute the vector of Milankovic wave-numbers. This vector is convolved with $\mathbf{w}$ and multiplied with the amplitudes in $\mathbf{a}$. The computation of $\mathbf{k}$ can be found directly in the code (in the function 'computeSedRatePdfs'). The vector with the shape of the spectral line cannot be found directly, because $\mathbf{w}$ is automatically applied through the specification of the number of samples - including zero padding - used in the spectrum (the more samples, the "broader" the spectral line). The amplitude vector $\mathbf{a}$ is not input to the computation, but implicitly determined during the process of computing the likelihood. Hence, it is not directly visible in the code. **Importantly, we stress that we are not interested in the amplitudes, only in the wave numbers.***

To clarify this further, we added a sentence to manuscript line 420:

The amplitudes of such periods could be significant, but they are ignored by our algorithm, since we are not interested in the amplitudes, only in the wave number.

89

It is important for the manuscript text to be clearer about what is being done, in terms of how the algorithm is conducting the analysis and evaluating the fit of the “data” to the “model”. For example, there is no indication about the quadratic detrending of the data series in each window, or the spectral background estimation using a sum of 3 decaying exponentials, etc. (see Section 3.1). The methodology description in the manuscript should be sufficient such that the code can, with appropriate expertise and time, be reproduced. In other words, a reader should not be required to consult the code for an accurate description of the essential methodology.

It is important to distinguish between what is belonging to the new methodology presented (and hence necessary for the method to operate according to its principles), and what are minor numerical operations that are replaceable without changing the core methodology. We consider the detrending in data and spectral domains as minor numerical operations that could be replaced, without changing the core method. In our most recent version of AstroComb (revised after submission) we have, in fact, changed the spectral background estimation. The effect was almost invisible, but the computational efficiency higher.

The latest version of the AstroComb algorithm will be curated and made available from this Zenodo website: <https://zenodo.org/records/17966228>

90

The AstroComb method can be considered an adaptation of the method of Malinverno et al. (2010). Reference to that study, as providing a precedence, is essential. A key innovation of AstroComb is the simultaneous evaluation of multichannel data. The only other instance I know of doing so is in a 2023 GSA conference abstract by Trayler et al. (2023; <https://gsa.confex.com/gsa/2023AM/meetingapp.cgi/Paper/393427>). The AstroBayes approach also uses an adaptation of the method of Malinverno et al. (2010).

Citation to Malinverno et al. (2010) is now added in Line 216.

91

When implementing the multichannel approach, it is important to note that covariance of geochemical data is common, while elements also express variable signal/noise. Appropriate treatment of the covariance/likelihoods is typically important when combining multiple datasets. It would be helpful if the authors spoke more to this issue.

The elemental variations can be correlated in two ways: (1) Because the experimental measurements introduce (possibly unavoidable) correlations. We have not incorporated these correlations, because they are unknown to us. (2) Because the elemental variations are generated by the same, underlying sedimentation rate mechanisms. Those correlations are included in our analysis through the forward model.

92

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). I found it interesting that AstroComb uses an L1 fit of the sum of 3 decaying exponentials for the spectral background fitting, as this is not typical in cyclostratigraphy. This may be a valid approach for some data sets, but the fit of the model to the data spectrum background (“spectral continuum”) must be validated for the probabilities to be reliable (e.g., see Malinverno and Meyers, 2024).

Agreed. 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.

93

Please provide additional theoretical justification for the probability estimation approach

(e.g., Equations 4-7), which is used instead of the more typical approach for spectra (e.g., Malinverno et al., 2010).

Additional theoretical justification for the probability estimation approach is now added in line 214ff:

In our computation of the likelihood, data uncertainties are provided by the user as the probability that a peak in the observed spectrum is a Milankovic peak. Existing data-fitting-based methods (Meyers and Sageman (2007); Malinverno et al. (2010), TimeOpt: Meyers (2015), TimeOptMCMC: Meyers and Malinverno (2018); AstroGeoFit: Hoang et al. (2025)) are purely based on misfits between data (elemental records) and synthetic signals (generated from assumed Milankovic periods), but AstroComb is instead aimed at simulating the data uncertainty evaluation carried out by practitioners analyzing observational data. In traditional misfit calculations you not only need experimental uncertainties, but also components of the data that are not modeled by the Milankovic cycles. The latter are significant, but not directly available (for example, The TimeOptMCMC algorithm Meyers and Malinverno (2018) estimate them through a Hierarchical Bayes approach). However, in our approach we are automatically taking all data uncertainty sources into account, including the practitioner's confidence in identifying spectral peaks.

94

Since AstroComb is a moving window approach, the results are smoothed at the scale of the window size (as is the case with eASM, eTimeOpt and eCOCO); this is like a lowpass filter. Thus, the lack of smoothing claimed in the manuscript text appears overstated (lines 13-15 of abstract).

Yes. Corrected. Line 111ff now reads:

Rather, than assuming a constant or globally smoothed rate model, our approach treats sedimentation as a piecewise continuous process, allowing for abrupt or non-linear changes across the stratigraphic column.

You are right in your statement about the inherent smoothing due to the window size, but there is no further smoothness assumption on the sedimentation rate vs. depth function. Two windows with neighboring center locations are not prevented from giving widely different maximum a posteriori sedimentation rates. This is in contrast to classical least-squares fits that impose smoothness on the solutions.

95.

AstroComb uses an overlapping moving window, while the method of Malinverno et al. (2010; see also AstroBayes of Trayler et al., 2024, which builds on this method) defines the analysis segments, based on an initial assessment of stable stratigraphic intervals. There are advantages to both approaches. Explicitly defining the “layer boundaries”, can help to optimize detection using prior stratigraphic knowledge and evaluation, and can guard against overinterpretation. The overlapping window approach can help to resolve shorter term shifts in sedimentation rate variability.

It is tempting to use initial assessment of stable stratigraphic intervals. However, in this way you "use the same data twice", thereby putting additional weight on (part of) the data. This is incompatible with correct uncertainty quantification, because it effectively reduces the error bars on (part of) the data.

96.

Related to the last comment, prior to the application of AstroComb and other such methods, it is important to evaluate bedding stability using time-frequency methods (e.g., spectrogram or evolutive harmonic analysis; Meyers et al., 2001), and to consider the stratigraphic context (including biostratigraphy, available geochronology, etc.), to inform the analysis (e.g., potential hiatus, etc.).

We agree.

97.

The Kullback-Leibler information measure (Equation 13) is an intriguing addition to the methodology. I suspect that it will be unfamiliar to most readers, so it would be helpful to provide discussion of what it means, in a more generally accessible manner. For example, explain what is meant by “information content”.

Yes. See also comment above. We have added a note and reference to the original paper. L205ff now elaborates on the meaning of the KI information measure:

Theoretically, the Kullback-Leibler information measures the expected number of yes/no questions needed to update our state of information from what is given by $u(r)$ (here total ignorance) to what is given by $p_z(r)$ (here, the posterior distribution).

98.

A potential challenge for AstroComb and other methods that seek to reconstruct variable sedimentation rates (eASM, eTimeOpt, eCOCO, AstroGeoFit) is that, due to changing signal/noise, one may be inferring astronomical signals through intervals where they are not present. While some methods provide hypothesis testing in an attempt to guard against this, nonetheless, it is not unusual to see intervals with dubiously low signal/noise. It will be important for the authors to speak to this issue, to guide users on when the AstroComb results are reliable. For example, it seems that the Kullback-Leibler information measure is useful for this purpose, but more guidance on this would be helpful .

Kullback-Leibler information is a measure of the reliability of the detection of astronomical signals. We believe that this is now explained in the text where we present the Kullback-Leibler information, and also in the caption of Figure 3.

Reviewer 3's more detailed comments

99

Lines 13-15: As noted above, the sedimentation rates derived from AstroComb are smoothed at the scale of the window. This provides a limit on the resolution of sedimentation rate changes throughout a stratigraphic record, similar to other ‘windowed’ approaches (e.g., eASM, eTimeOpt, eCOCO).

See our comment above.

100

Line 17: The use of the term “astronomical tie points” might be a bit confusing to readers. In principle, the derived “floating time scales” can be anchored to reliable radioisotopic/biostratigraphic/chemostratigraphic/magnetostratigraphic data. For the last ~60 Ma the theoretical astronomical solutions (“astronomical tie points”) also provide a means for anchoring astronomical time scales (given other appropriate constraints, e.g., paleomag data).

Yes. We revised the text to distinguish between geological time and geological age modeling. Line 15ff now reads:

By detecting and statistically constraining Milankovitch cycles preserved in stratigraphic signals, the algorithm seeks a floating age-depth model that reconstitutes geological time and can be anchored to deliver absolute ages, where astronomical tie points or radiometric ages are available.

101

Lines 19-22: As noted earlier in this review, numerous approaches have been previously developed that combine cycle detection and stratigraphic modeling, and the approach here is similar to the method of Malinverno et al. (2010). It is critical to discuss this in the manuscript.

Reference added.

102

Line 20: The claim of “preserving fine-scale depositional variability” is overstated here, as the scale is fundamentally limited by the size of the moving window. For example, the approach cannot resolve precession-scale variability. Note that methods to resolve such “bedding-scale” sedimentation variability have been published (e.g., Schiffelbein and Dorman, 1986; Gamma method of Kominz and Bond, 1990; TimeOptTemplate of Meyers, 2019; Alpha method of Ma et al., 2020).

Yes and no. We clarified in Line 257 the abrupt change is reconstructed well in the synthetic data (no smoothing of sedimentation rate variation along stratigraphy), but the position of where the abrupt shift occurs is uncertain:

Abrupt changes in the sedimentation rate of about a factor three occur at 70, 74, 81, and 83 m, all of which are accurately captured by the AstroComb algorithm (figure 3). Sedimentation rates are not “smoothed”, but the timing of the abrupt change appears uncertain.

103

Lines 27: For appropriate scholarship, please add citation(s) for astrochronology as a methodology (e.g., Hinnov, 2013 and references therein).

Reference has been added to Line 27.

104

Lines 28-33. The description here of methods could be improved. Based on the references, I would replace “repetitive spectral features” with “repetitive bedding cycles”, and replace “spectral filtering” with “spectral analysis”. The passage mentions numerous other methods

(TimeOpt, TimeOptMCMC, AstroGeoFit, eTimeOpt, eCOCO, etc.) all of which are inverse methods that tune to astronomical target frequencies, in some fashion or other. The passage is missing key methodologies, including the one that is most similar to AstroComb, which is Malinverno et al. (2010). See Section 2 of this review for more information.

Corrected. See also comment above A careful introduction to cyclostratigraphic methods is summarized in line 40-55

105

Line 34: Change “Despite sedimentation is” to “Despite the fact that sedimentation is”

Line 58 now reads:

“Sedimentation is an inherently episodic process (Kemp and Sexton, 2014)”

106

Line 45: change “has been” to “was”

This paragraph has been revised and the expression deleted.

107

Lines 34-49. The statements noted here about the challenge of variable sedimentation are indeed key issues that need to be carefully considered. In this regard, the AstroComb method has the same fundamental limitations as many other published moving window methods (e.g., eASM, eTimeOpt, eCOCO).

Agreed. This paragraph has been revised. See lines 40-55.

108

Lines 47-49. For appropriate scholarship, this passage should reference prior work that makes the same claim (see Section 2 of this review).

Agreed. This paragraph has been revised. See lines 40-55.

109

Line 58. As AstroComb is a moving window method, fundamentally it imposes a smoothing (constant sedimentation rate within the analysis window).

Not quite. Recall, the sedimentation rate obtained in a given window is by AstroComb only used to assign a sedimentation rate to the midpoint of the window. It may seem surprising (at first glance, it was to us), but abrupt sedimentation rate change arises when data requires it. See synthetic data.

110

Lines 60-64: As noted earlier, the primary innovation here is the use of multichannel data, while the basic framework builds on the method of Malinverno et al. (2010) (see Sections 2 & 3).

AstroComb innovates both in terms of a new probabilistic formulation and applicability to multi-channel data. See comments above.

111

Lines 77-78: Here, sub-precession scale changes in sedimentation are mentioned with reference to another algorithm that is not yet published (“ProBE4T”; Fernandes et al., 2026). I have not been able to locate ProBE4T online.

We have removed the reference to the unpublished paper by Fernandes et al., 2026.

112

Line 86: remove “here”

Deleted

113

Lines 101-112: This section of the manuscript doesn’t sound like a “workflow of AstroComb”, but rather a description of the outline of the manuscript.

Section 2.5 describes the AstroComb algorithm and how the code computes it. We added what information is supplied by the user, incl.

Step 2: *(an interval with length, e.g., 2–4 m chosen by the user)*

Step 3: *(the threshold of which is defined by the user),*

Step 5: *Also, an age-depth model is derived when the user defines an age-depth anchor.*

114

Line 111: change “evaluate” to “evaluates”

Corrected.

115

Lines 113-115: The use of multichannel data is a key innovation (the only other instance I know of is in a 2023 GSA conference abstract by Trayler et al. (2023); <https://gsa.confex.com/gsa/2023AM/meeIngapp.cgi/Paper/393427>)

Thanks. This paper is now cited in the introduction

116

Line 121: The manuscript correctly notes that the AstroComb method works “under the assumption that certain stratigraphic signals (e.g., elemental variability) reflect astronomical forcing.” Therefore, before interpreting AstroComb results as reliable, it is important to test this assumption, which is a key component of many of the published methods (ASM, TimeOpt, TimeOptTemplate, TimeOptB, COCO, AstroGeoFit, etc.). It should be noted in the manuscript that initial hypothesis testing is prudent, before applying AstroComb, and a range of published methods are available to do so.

Indeed, there are several ways to test whether observed periodicities are Milankovitch periods. This is beyond the scope of the current version of the AstroComb algorithm. We have revised the text accordingly. Line 77ff now reads:

The current version of AstroComb is not designed to further validate whether identified Milankovitch periods are indeed driven by insolation variations (e.g. eccentricity-driven amplitude modulations on precession and/or obliquity cycles).

117

Lines 145-146: For the discussion of the distortion of spectra, it would be good to reference past studies that have addressed this, such as Schiffelbein and Dorman (1986), Ripepe and Fischer (1991), Herbert (1994) and Meyers et al. (2001).

The references are now added. Indeed, these papers do address the distortion of spectral peaks.

118

Line 149: Note that there are two components to the theoretical astronomical solutions: the orbital part and rotational component (see Laskar, 2020).

Yes. We know. We are not using the closed-form astronomical solution, but the target astronomical spectrum as described in section 2.3 (line 231ff).

119

Line 166: Change “computed data spectra” to “computed synthetic spectra”

‘data’ is now replaced with the better ‘synthetic’.

120

Equations 4, 5, 7, 8: Please use a different notation for “a”, which has already been used as the vector of amplitudes (Equations 2 and 3).

In equation 2, ‘a’ is now replaced with ‘a’.

121

Figure 2: Please indicate the depths on the y-axis.

Corrected.

122

Figure 3: It would be valuable to show a time-frequency analysis (spectrogram, evolutive harmonics analysis) with the same window size. Are the time-frequency results compatible with the AstroComb results?

A comparison between AstroComb and previous methods is now added to figure 7 and 9

123

Lines 264-265 and 337: Change “elements” to “elemental”

Corrected

124

Line 291: Change “beteen” to “between”

Corrected