20. May 2026

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 fea tures 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 Sageman (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 1analyses 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 rhytmicity (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.

Theis 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 elements 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 occuring 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 Milanovitch 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* = <*F*> + \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-liner” 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 discontinuos 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 <u>minor</u> 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, ⟨F⟩ 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*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 pz (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 pz (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 probabilty 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.