Reply to Reviewer #1 (Beatriz Bádenas)

Dear editor and author,

This paper is a solid work on the climate control on a Lower Jurassic hemipelagic succession in the Basque-Cantabrian Basin that contain interesting approaches to understand factors controlling its accumulation. Data, interpretations and discussion are very well organized (although some parts are not balanced: see comment 20; and the discussion is quite long and complex). Without a doubt, the paper deserves to be published. However, concerning descriptions (and related interpretations and discussions) four main aspects require to be deeply explained:

- hemipelagic character of the successions (see manly comments 1, 6);
- significance of color (see comments 10, 16, 18) and MS data (see comments 17, 18);
- criteria for definition of couplets (precession cycles) and bundles (eccentricity cycle) (see comment 15);
- characterization of the black shale package as a whole (see comments 7, 13, 26).

Other changes are suggested in order to state clear some concepts and description

Introduction

1. Pelagic rhythmites are presented as one of the key sedimentary successions recording orbital controlled climate changes (first paragraph). However, the studied succession is hemipelagic. It would be interesting to include: 1st) a brief definition of the term hemipelagic in the context of the studied BCB; 2nd) a brief explanation (and references) on the role of orbital-induce climate variations on this particular kind of sediments, compared to the pelagic ones

RESPONSE: AGREE. Additional explanations, as well as references, will be included in the revised manuscript.

Geological setting

2. Lines 83-84: “which connected the Boreal Sea with the southern Tethyan Ocean”. Better: “which connected the Boreal Sea with the northwestern Tethyan Ocean”.

RESPONSE: AGREE. The manuscript will be revised accordingly.

3. Line 87: “source area was located in the semiarid belt”. What do you mean thin “source area”, emerged land?, shallow platform carbonate source area? Please, explain better.

RESPONSE: AGREE. We refer to the emerged source area. The manuscript will be revised accordingly.

4. Specify if the distribution of the humid and semi-arid zones was stable for the entire Early Jurassic

RESPONSE: AGREE. Clay minerals from the Early Jurassic Peritethyan area (Dera et al., 2009; Deconinck et al., 2020) results were congruent with independent approaches (e.g. Rees et al., 1999; Arias, 2007), supporting the identification of paleoclimatic belts during the Pliensbachian–Toarcian interval. Anyway, the study area, being close to the boundary between two latitudinal climatic zones, was especially sensitive to astronomically driven climate change. In fact, periodic changes in orbital parameters force latitudinal displacements of this boundary (Martinez and Dera, 2015). Consequently, the study area could have suffered greater or lesser influence of the humid or arid zones during astronomical cycles. This information will be more clearly explained in the revised manuscript.

5. Use in Fig. 1, Early/Lower Jurassic instead of Lias.

RESPONSE: AGREE. Figure 1 will be revised accordingly.

6. Line 104: “Pliensbachian (192.9–184.2 Ma) hemipelagic successions of the BCB.”. I suggest deleting the time duration: I suppose the studied succession has not been time-calibrated so accurately.

RESPONSE: AGREE. The manuscript will be revised accordingly.

The sedimentary environment of the successions requires a deep explanation. Notice the term “outer ramp” appears for the first time in the discussion (line 778). See also lines 677-679 “restricted paleogeographic setting”).

RESPONSE: AGREE. The sedimentary environment will be more precisely explained in the geological setting of the revised manuscript.

Revise also lines 841-843 (“basins depleted in oxygen”: be careful, it sounds like a circular reasoning).

RESPONSE: AGREE. The manuscript will be revised accordingly.

7. Line 106: use “packages of alternating black shales and limestones/marly limstones” instead of “black shale intervals”. It is important to state clear these black shales do not include only shales but also intercalated limestone/marly limestones. I think the word Interval has a time connotation.

RESPONSE: PARTLY AGREE. The term “black shale interval” has been commonly used by previous authors for the studied deposits (e.g., Rosales et al., 2004, 2006; Quesada et al., 2005; and references therein) and we consider it appropriate (the term “interval”, in addition to the time connotation, also refers to the space between objects, units, points or states). However, in the revised manuscript it will be properly explained that the black shale intervals are packages of alternating black shale layers and limestones/marly limestone beds, which are separated from each other by decametric intervals devoid of black shale layers, in which only hemipelagic marls, marly limestones and limestones occur.

8. Lines 130-132: “and 1 km north-west of a coeval section studied by others at the train station in the same locality…with which a bed by-bed correlation can be readily carried out.” This sentence is more appropriate for the discussion (see also comment 26). In any case, it requires a deep explanation of how this correlation was made, without (I suppose) lateral continuity of outcrops.

RESPONSE: AGREE. The manuscript will be revised accordingly and new supplementary figure 1 will be added, in order to illustrate the correlation between both sections. Bed by-bed correlation between separate and discontinuous outcrops was carried out on visual grounds, by the identification of key beds with distinctive sedimentary features (mainly lithology and thickness) and characteristic bed arrangements in the succession.

9. Lines 132-137: Please state clearer the location and thickness of the studied succession. As far I understand the studied succession is 22.5 m thick and includes: the uppermost 2.5 m of

the Puerto Pozazal Formation and the lowermost 20 m of the Camino Formation (including the first x-thick black shale package of this unit). However, in line 140 “30.40 m thick” is mentioned.

RESPONSE: AGREE. We apologize for conveying misleading information in our original manuscript. As pointed out by the other reviewer (Sietske Batenburg), the thickness of the studied section was not clearly presented. We analysed a 30.4-m-thick section in the outcrop, but the lowermost 7.9 m were excluded from the cyclostratigraphic analysis because of poor exposure. Thus, in our original manuscript the top of the stratigraphic log was located at 30.40 m, but it started at 7.9 m. This means that the studied succession is actually 22.5 m thick. In order to present this information more clearly, the bottom of the studied succession will be established at 0 m and the top at 22.5 m in the revised manuscript (Figs. 2, 4, 6, 7, 9, 10, 11, 14 and the supplementary material will be modified accordingly).

Materials and methods

10. The average color of samples is used for cyclostratigraphic (spectral) analysis. However, there is not any analysis to elucidate the sedimentary vs. diagenetic significance of this feature.

RESPONSE: DISAGREE. As shown in lines 255-260 of the manuscript and in Fig. S1 (Figure S2 in the revised supplementary material), there is a great positive correlation between the colour and %$CaCO_3$ of rock powder samples from Santiurde. Consequently, rock powder colour can be considered a good representation of %$CaCO_3$. In fact, colour measurement of rocks, as an indicator of rock composition, is a relatively cheap, fast and non-destructive technique commonly used for cyclostratigraphic analysis (Olsen et al., 1999; Dinarès-Turell et al., 2003, 2018; Batenburg et al., 2014; Lauretano et al., 2015; Li et al., 2023; Martínez-Braceras et al., 2023; Wan and Wei, 2024). As with any other compositional proxy, the cyclostratigraphic analysis of colour data series can be carried out regardless of whether the rocks retain their original colour/composition or this was subsequently affected by diagenesis. In fact, the result of the cyclostratigraphic analysis will help elucidate whether the original sedimentary composition is retained: if an orbital forcing can be readily identified, this will imply that the succession retains the original (primary) sedimentary signal (as in our case study); if no orbital influence were deduced, this could imply that either the original succession was not orbitally forced or, alternatively, that the orbital signal was tainted by diagenesis. In our case study, the primary sedimentary origin of %$CaCO_3$ is widely discussed in section 5.1. Both physical (sedimentology, orbitally modulated bed arrangement, etc.) and geochemical (inorganic isotopes, major and trace element content, etc.) evidence corroborate that our calcareous rhytmites (as defined by their colour and %$CaCO_3$ content) responded to primary environmental variations and do not reflect diagenetic overprinting.

11. Thin sections are mentioned in results, but not included here.

RESPONSE: DISAGREE. The original manuscript mentioned that petrographic analysis of one sample per bed was carried out. In order to make things clearer (comment 12 about line 177), we will specify that 19 samples were analysed in the revised manuscript.

12. Please, explain the lithology of the studied bundle and samples: line 167: fifty-seven samples, include also here the values of x samples/bed; line 177: central part of each bed, include here also the total number of samples.

RESPONSE: AGREE. The manuscript will be revised accordingly.

Results

13. Lines 209-210: Concerning lithological terms, “limestones or marly limestones” and “marls or shales”. Do you have calcimetric analysis of the entire succession to differentiated these lithologies?. Concerning the term “shale”, please see previous comment 7. The black shale package has to be presented.

RESPONSE: AGREE. The lithologies of the entire succession were defined on visual and sedimentological grounds. It will be better explained in the revised manuscript (section 4.1.1) that this can be readily done in the field by taking into account rock colour, hardness (expressed by weathering), internal lamination, and fossil content. As clearly stated in the manuscript, calcimetric analysis was only performed in the interval studied in detail, where bed composition was determined quantitatively. The calcimetric results confirm that the visual description of facies is accurate (Fig. S1; Fig. S2 in the revised supplementary material). A presentation of the Black Shale interval studied herein will be included in the revised manuscript.

14. Description of lithologies and texture. In Fig. 2 (log), marly limestones of limestones with different texture are not drawn. I suggest to draw them. Also state clear the description of each lithology separately (also limestones and marly limestones; do they have bioturbation?) and then compare their main differences.

RESPONSE: AGREE. Figure 2 and 4 will be modified accordingly. More accurate descriptions of the main lithologies and textures will be presented in section 4.1.1 of the revised manuscript (also see the response to the previous comment), including bioturbation.

15. Lines 236-244 on couplets and bundles. This paragraph has to be separated in a subsection. The criteria for differentiating couplets are unclear: why the couples marl/shale to limestone/marly limestone (and not at the contrary)?; the “lithological contrast” for bundles is also very unclear (see also comment 13 on carbonate content of the entire succession). Do you see significant features at the boundaries of couplets or bundles or any trends withing couplets or bundles?.

RESPONSE: AGREE. A new subsection “4.1.2 Bed arrangement” will be added in the revised manuscript. The criteria used for the definition of couplets and bundles will be more clearly explained. As both couplets and bundles are cyclic arrangements of beds, they do not have objective boundaries with significant features. Thus, it is irrelevant whether couplets contain marls/shales below and limestones/marly limestones above, or vice versa, providing that the criterion is coherent throughout the succession. Bundles also show a symmetrical vertical trend in the arrangement of their component couplets. As defined herein, the lithological contrast of the beds that make up successive couplets increases progressively from the bottom to the middle part of the bundles (marl/marly limestone couplets at the bottom of the bundles, shale/limestone couplets in their middle parts), and then gradually decreases again (bundles ending up with marl/marly limestone couplets at their tops). Thus, bed boundaries are sharper in the middle part of bundles than at bundle boundaries, most likely due to the greater lithological contrast between successive beds in the former. Otherwise, neither couplet boundaries nor bundle boundaries show any significant features.

16. Color trends: lines 244-258 “The variations in colour values are more significant in the central couplets of bundles than at bundle boundaries. This suggests that, as shown in previous studies… colour values are representative of the carbonate content of the samples.”. See

previous comment 15 on “lithological contrast” for bundles (not well explained” and also comment 10 (significance of color). To use the similar trend in color and carbonate content in C35 to C44 as supporting criterion, it is necessary to discuss there was not a diagenetic imprint in both color and carbonate content.

RESPONSE: AGREE. The manuscript will be revised accordingly; see responses to comments 10, 13, 14 and 15.

17. Did you perform analysis of susceptibility-temperature (k-t) curves to know the type and abundance of magnetic minerals? The following sentence is not clear (as far I understand you interpret the presence of ferromagnetic minerals indirectly): Lines 264 “The MS of hemipelagic deposits is commonly determined by their paramagnetic components (mostly detrital clays; Kodama and Hinnov, 2015). However, in Santiurde this parameter does not show a great correlation with colour (r: 0.48, $p<0.001$, all section; Fig. S1) or calcium carbonate (r: 0.36, $p<0.001$, between C35 and C44; Fig. S1). Therefore, the Santiurde relationship suggests that the MS signal is more likely controlled by ferromagnetic minerals, such as magnetite (Fig. S2).” Revise also lines 750-755.

RESPONSE: UNCERTAIN ABOUT THIS COMMENT. As stated in the original manuscript, susceptibility-temperature (k-t) curves were obtained, and the result of a representative sample presented in Figure S3 (Figure S4 in the revised supplementary material). The thermomagnetic curve confirms the presence of magnetite, which is thought to be the main MS driver.

18. Spectral analysis of MS data (lines 283-285). MS data do not correlate with color and carbonate content; however, their spectral analysis corroborate the results of the spectra analysis of color. Please, explain this apparent contradiction.

RESPONSE: AGREE. An explanation is given below and will be incorporated into the caption of Fig. S3 (Figure S4 in the revised supplementary material). Limestones usually present higher magnetic susceptibility values than adjacent marls/shales. However, the MS data series displays a greater dispersion and a spikier appearance than the colour and %$CaCO_3$ series, which very likely explains the low correlation coefficient between the MS data series and the colour and %$CaCO_3$ data series (Fig. S1; Figure S2 in the revised supplementary material). As explained in the response to comment 17, the MS signal is mainly carried by magnetite content, which could be either detrital in origin or related to postdepositional changes in redox state. The influence of early diagenetic processes, such as partial replacement of pyrite with iron oxides at more oxygenated conditions, might explain the high variability of the MS curve. Notwithstanding the potential flaws of the MS data series, the spectral analysis shows that it records a significant periodicity with an average thickness equivalent to that of precession couplets. Despite being less prominent, cycles correlatable with those attributed to obliquity(?), short eccentricity (bundles) and long eccentricity in the colour spectral analysis series can also be identified in the MS spectra.

19. Lines 310-311. “In general, %$CaCO_3$ fluctuates in line with lithology, limestones and marly limestones (average: 66.36%) being richer than marls and shales (average: 34.86%). What do you mean? In fact, carbonate content is the criterion to differentiate these lithologies.

RESPONSE: AGREE. The lithology of the entire succession was defined on visual grounds. The high-resolution calcimetric analysis of Bundle 9 (C35-C44 interval) corroborates the visual lithological identification. This will be more clearly explained in the revised manuscript.

20. In 4.2. Detailed analysis of Bundle 9 (C35-C44 interval), pure descriptions are included in 4.2.1 to 4.2.4; however, 4.2.5 and 4.2.6 contain interpretation/discussion of the results, including the interpretation of oxic/anoxic conditions of the different lithologies (without any reference to the other results). This imbalance should be corrected.

RESPONSE: DISAGREE. In sub-section 4.2.5, the enrichment factors of several elements and some palaeoceanographic indices are calculated and presented. In order to understand why these (and not other) indices and elements are analysed, we consider it necessary to explain their palaeoenvironmental meaning and significance (with references to others' works). However, the specific results of the elemental enrichment factors and the paleoceanographic indices from Santiurde are not interpreted in this sub-section (this is done later in section 5), only their general trends are described.

Similarly, only the results of a factor analysis are presented in sub-section 4.2.6. There was only one interpretation at the end of this subsection in the original manuscript (referring to orbital forcing), which will be modified in the revised manuscript in line with the reviewer's comment. There are no other interpretations of the results obtained in our study in this subsection. Other statements that may resemble interpretations (the palaeoenvironmental meaning of the most representative elements or group of variables extracted from the factor analysis) are, again, simple reminders of the basic concepts introduced in the preceding subsection, which intend to help the reader follow our line of reasoning.

Discussion

21. Line 459. "Origin of inorganic sedimentary fluctuations". I suggest deleting "inorganic". This term is obscure.

RESPONSE: AGREE. The manuscript will be revised accordingly.

22. Lines 470-472 (secondary cements..), line 474 (bed geometry): these descriptions should be explained also in Results.

RESPONSE: AGREE. The manuscript will be revised accordingly, transferring several characteristics of the succession mentioned in the discussion into section 4.1.1. (Sedimentology and petrography of the general Santiurde section).

23. Lines 476-478. "Quite the opposite, the characteristics of the beds are continuous for more than 1 km between the Santiurde motorway and railway sections". See comment 8.

RESPONSE: AGREE. This sentence will be modified in line with reviewer's comment 8 (see above).

24. Lines 485-487: "In general, the diagenetic characteristics observed in the Santiurde rhytmites are typical of processes related to organic matter decay during burial (Rosales et al., 2001). This sentence is not informative. Please explain in which way.

RESPONSE: AGREE. The sentence will be deleted.

25. Lines 488-493 about periodicities. Do you have data on the time span of the studied succession to compare with your results? I would be interesting to know how many cycles are then represented in the entire succession and BS package.

RESPONSE: AGREE. We have estimated the duration of the studied interval and of the Black shale interval 1 by counting orbital cycles. However, we will not include this at the position pointed out by the reviewer (in the discussion about the primary or diagenetic origin of the rhythmite), but in section 5.3 about orbitally modulated environmental changes.

26. The discussion lacks a proper explanation of the BS package as a whole (how many precession or eccentricity cycles includes, what short- and long-term factors controlled its accumulation.

RESPONSE: PARTLY AGREE. The duration of the BS package has been estimated based on the number of orbital cycles, which will be added in the revised manuscript (response to comment 25). The orbitally modulated environmental factors that controlled the fluctuating sedimentation when the Black Shale interval was being accumulated are widely discussed in section 5.3. However, the factors the determined the formation of the entire Black Shale interval cannot be elucidated with the data available in this study. As stated by Rosales et al. (2006), the Pliensbachian Black Shale intervals of the BCB accumulated during second order sea level rises.

Regards,

Beatriz Bádenas

Cited literature (not included in the manuscript):

Arias, C.: Pliensbachian–Toarcian ostracod biogeography in NW Europe: evidence for water mass structure evolution, Palaeogeogr. Palaeoclimatol. Palaeoecol., 251(3-4), 398-421, https://doi.org/10.1016/j.palaeo.2007.04.014, 2007.

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