



EGUsphere, referee comment RC2
<https://doi.org/10.5194/egusphere-2022-74-RC2>, 2022
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Comment on egusphere-2022-74

Simon Andrieu (Referee)

Referee comment on "Shallow marine carbonates as recorders of orbitally induced past climate changes – example from the Oxfordian of the Swiss Jura Mountains" by André Strasser, EGU sphere, <https://doi.org/10.5194/egusphere-2022-74-RC2>, 2022

Article review:

"Shallow marine carbonates as recorders of orbitally induced past climate changes – example from the Oxfordian of the Swiss Jura Mountains" by André Strasser

General comments

In this well-written and interesting paper, the shallow Jurassic sedimentary series of Jura are interpreted as recording orbital eccentricity and precession cycles, leading to climatic changes controlling carbonate producers and production, as well as siliciclastic input. This article addresses a key theme for scientists using carbonate sedimentary rocks as a paleoenvironmental proxy: is the development of lagoon and very shallow deposits (widespread during the Middle and Late Jurassic in northwestern Tethys) primarily controlled by orbitally induced climatic variations? If yes, how orbital cycles are recorded and how do they control depositional sequences and facies?

The study focuses on the Late Oxfordian deposits of the Jurassic carbonate platform, exposing lagoon, tidal flats and ooid bars facies. A review of the sequence stratigraphic context is proposed, both at local and global scales. Three sedimentary field sections are presented, among the 19 on which this study is based (all coming from the same article: Strasser, 2007). Long-term, short-term and elemental depositional sequences are identified, and related to the 400, 100 and 20 Kyr Milankovitch cycles, respectively. Finally, the role of different climate factors on the development of the platform and the different depositional sequences is discussed, and two models detailing the climate control on elementary sequences (20kyr) are proposed (in low and high insolation periods respectively).

This manuscript has the potential to be of interest to the scientific community and to be impactful, but I would foremost raise several areas of concern that should be addressed before its publication:

- My main area of concern relates to a better argumentation of the Milankovitch cycles control on the observed sequences. This is the keystone of the manuscript since the entire discussion on the interpretation of climate control relies on this assumption. However, as it stands, the argumentation is somewhat expeditive, when it should be a major point of the discussion (or preceding the discussion if you think it was demonstrated by previous studies). In restricted environments such as lagoons, and as the author rightly reminds us, local changes in paleoenvironments, sedimentation rates etc impact substantially accommodation filling and facies, and then sedimentary cycles. So what are the powerful arguments to say that the long-term, short-term and elementary cycles are all controlled by global climate and orbital cycles? See more details in my specific comment on the "*Definition and correlation of depositional sequences*" section.
- The only presented data are three sedimentary sections, and I think the paper would benefit from presenting more data, or presenting it differently. As it stands, I identified several issues. Firstly, none of the sections are focusing on the interval from Ox6 to Ox7, which is yet part of the study (I did not find the information for Hautes Roches A and B sections, where are they located in the global sequence stratigraphy context?). Secondly, to support the fact that the described sequences are controlled by global climate, it is essential to show how these sequences correlate on a large scale; not only the long-term ones, but also the short-term and elementary ones. I saw some nice correlation diagrams in Strasser (2007). Why not showing here a correlation diagram between a few selected, representative and extensive sections? It would allow addressing both issues I raised above, and also help to better argue the Milankovitch control on the observed cycles.
- The introduction loses an opportunity to sell the pitch. The gaps and scientific questions that this article aims to address are not presented. Why is this work necessary? What are the key findings that make this study worth publishing? (there is for sure an interesting model proposed and a climatic review conducted, but it needs to be sold). This introduction needs also to state how this paper differs from Strasser (2007) as both articles rely on the same sections? It is obvious that our understanding of past climate processes improved in the last 15 years, probably allowing to go further in the interpretations. But it needs to be said. What was Strasser (2007) demonstrating? What is demonstrated here that was not known in Strasser (2007) and how (as the data are almost similar)?
- Finally, the model proposed at the end of the discussion, linking facies and climatic cyclicity at the scale of an elementary sequence is interesting. I have however two areas of concerns. Firstly, and to echo my remark in point 2, I did not see any lateral correlation of elementary sequences between the different sections in Strasser (2007). So how can you relate them to a global climate control? If precession was the main factor controlling those sequences, you should be able to correlate them at the large scale. If you cannot, does not it mean that autocyclic processes are the main controls? Moreover, the number of these elementary cycles in the short-term cycles is, as the author wrote, highly variable (2 to 15). Secondly, the facies successions (for model A especially) are never found in their entirety within the three sections presented. So how were these successions determined? Did you find the complete succession in other sections? Then, why not presenting those sections?

If these points of uncertainty are removed, the manuscript will be more convincing, and really worth publishing. I therefore suggest major revisions.

Specific comments

My remarks are detailed in comments along the PDF.

Abstract

Line 11: the abstract misses a problem statement.

Line 12: The focus is made on the middle to late Oxfordian, but the biostratigraphy of the Oxfordian/Kimmeridgian is a complex story... On Fig. 3, I see that the focus of the study is on the entire Bimammatum ammonite zone, which is located in the late Oxfordian only (not middle Oxfordian here). Looking at Figure 4, the Hesselbo et al. (2020) chronostratigraphic chart locates the new Hypselum (Hypselum was before considered as a sub-zone of the Bimammatum zone) and Bimammatum ammonites zones respectively in the latest Oxfordian and earliest Kimmeridgian. So it would imply that this study focuses on the late Oxfordian and early Kimmeridgian. So why giving this middle/late Oxfordian focus?

Introduction and climatic controls on shallow marine carbonate systems

As it stands, the introduction misses the opportunity to raise questions and to indicate gaps that will be filled by this study. What remained unclear that necessitated conducting this study?

From what is written in this introduction, it seems obvious that the platform will record orbital cyclicities. If it is the case, why is it necessary to conduct this study? I have the feeling that, in fact, there are major questions about the respective impact of allocyclic versus autocyclic processes on shallow carbonate facies deposit. For examples, don't the autocyclic processes risk to be superimposed, even to mask the orbital cyclicities in shallow carbonate platforms? If these orbital cyclicities are indeed registered (or the major controlling factor), how do they affect the system? How do they modify the facies and sedimentary successions? The article suggests answers to those major scientific questions, which have to be introduced. I feel that the introduction misses the opportunity to sell the pitch, to explain how this study is important and shows something new. The purpose of the study, principal findings and structure should be presented at the end of the introduction.

Moreover, “2 Climatic controls on shallow marine carbonate systems” should be part of the introduction as it presents what we know on how orbital cycles control shallow carbonate systems. Remaining questions should be raised after this presentation.

Oxfordian case study

Stratigraphy and palaeogeography

Figure 4: how are the changes in the location of sequences boundaries compared to ammonite zones between Hardenbol et al. (1998) and Ogg et al. (2012) explained? For example, Hardenbol et al. (1998) locates Ox7 in the Bimammatum zone, while Ogg et al. (2012) locates the same sequence boundary in the Bifurcatus zone. It may be due to an update of the ammonite zonations, I guess. Maybe this could be shortly mentioned/explained in the Figure caption.

Figure 5: Drawing the present coastlines would help to find our way around this figure.

Definition and correlation of depositional sequences

Line 225: I read the abstract of Strasser (2007) and skimmed the article, whose data seemed to me very similar to that article. How does this new review differ from Strasser (2007)? It needs to be explained in the introduction.

Lines 234-257: this paragraph is the keystone of the entire article. Here, the author argues that the formation observed sequences (long-term, short-term and elementary) is controlled orbital cycles. The entire discussion that follows flows from this assumption, so it is a fundamental paragraph for this article. However, as it stands, I find the argumentation light, and I have several questions:

- Firstly, are the Oxfordian/Kimmeridgian deposits of Jura the only sedimentary series where lagoon/shallow platform sequences are interpreted as resulting from 400, 100 and 20 kyr orbital cycles? What is the literature saying about this in other shallow platform case studies? Do we have some other convincing examples in Jurassic times or Mesozoic times in general? This could be part of the introduction.
- The author's reasoning is based on dividing the interval duration by the number of sequences, which resulting duration seem to correlate with the different orbital cycles of 400, 100 and 20 kyr. The duration of the time interval between Ox6 and Ox8 is about 1.2 Ma. It includes 2 long-term sequences (mean duration of 600 kyr). When I look at Figure 4, I see that Strasser (2007) suggests a duration of 800 kyr from Ox7 to

Ox8 and 400 kyr for Ox6 to Ox7. However, Ogg et al. (2012) suggested a duration of 600 kyr for each one of those two sequences. So why taking in account the proposed duration of Strasser (2007) and not the more recent one of Ogg et al. (2012)? This is very important, because it controls all the interpretations on the smaller-scale sequences durations, and then on how they are interpreted or not as related to orbital cycles.

- Hang (2018) identified three 400kyr eccentricity cycles from Ox6 to Ox 8. If eccentricity controls long-term sequences why don't we have three sequences?
- When I look at Strasser (2007), I see indeed that short-term sequences (interpreted as resulting from 100 kyr orbital cycles) correlates at the Jura platform scale. But what about elementary sequences? If they are really controlled by precession, they should correlate laterally at a large-scale. Otherwise, it means that autocyclic processes were the main influence on those sequences and hiding the global climate control.
- This second hypothesis is supported by the fact that "The small-scale sequences are composed of individual beds, the number of which varies between 2 and 15 per small-scale sequence (Fig. 6)" (lines 244 and 245). Is that not an argument to say that those beds are not orbitally controlled? Otherwise, small-scale cycles should be composed in a large majority of 5 elementary sequences (or a number very close to this)?

Definition and correlation of depositional sequences

See the point 3 of the General comment. Three sedimentary sections are presented here. However, none of them are focusing on the interval from Ox6 to Ox7, which is yet part of the study (I did not find informations on how Hautes Roches A and B sections relate to the global sequence stratigraphy context). Moreover, to support the fact that the described sequences are controlled by global climate, it is essential to show how these sequences correlate on a large scale; not only the long-term ones, but also the short-term and elementary ones. I saw some nice correlation diagrams in Strasser (2007). Why not showing here a correlation diagram between a few selected, representative and extensive sections? It would help addressing issues I raised for the "*Definition and correlation of depositional sequences*" part about better arguing the Milankovitch control on the observed cycles. Is that possible to correlate elementary cycles laterally? If not, why?

Discussion

Sea level

Line 531: There are important uncertainties on water depth estimates, which affect the following sea-level reconstruction. For example, why choosing 1 m for grainstone? Why not 5 m? Or even 10 m? Why packstones and floatstones with abundant fauna are set at 2 m? Is there not a larger possible range of water depth? Estimating those uncertainties and representing their repercussion on sea level and accommodation reconstruction in figures 10 and 11 seem important for more transparency (what is the band of uncertainty on the presented curves?).

Line 560: You suggest here that the translation of insolation changes to sea-level change "was mainly via thermal expansion and retraction of the ocean water". However, variations of insolation may also modify aridity/humidity pattern (as you suggest in your final model) and then aquifer level. Besides, the part 4.3 of the discussion correctly demonstrate that the humidity/aridity gradient was varying through times. So can we really argue that translation of insolation changes to sea-level change was mainly via thermal expansion and retraction of the ocean water?

Figure 11: 1) I am surprised to see that the surfaces on the orange curve fits exactly with the surfaces on the decompacted sedimentary section. Accommodation variation (or gain) over a time interval T being the sum of decompacted sedimentary thickness and water depth variation, I would expect to see small differences in values between the section (decompacted sedimentary thickness only) and the long-term accommodation variation curve (because of water depth variations). Otherwise, it means that the water depth was identical at the top over every sedimentary bed? Is it the case?

2) Concerning the "sea level deviation from average accommodation gain" curve, in the lower part of the figure, it would be interesting to have a vertical scale to appreciate the amplitude of water depth variations.

3) As written in my comment above, it seems important to represent an estimate of uncertainties on those curves.

Figure 12: 1) vertical scale is missing; 2) uncertainties are missing on sea-level variations, 3) the caption may indicate that the insolation curve is from the last 500 kyr, 4) Please indicate sea-level rise and fall on the δO^{18} curve

Combined climatic effects at orbital scale

Figure 12: This is an interesting model. However, I don't see the complete succession of facies within a sequence that is detailed here in any of the three sections presented in this paper (at least for case A). So, two questions: 1) how did you determine the facies succession described here? From other sections? Facies is the input for the proposed model. 2) If this model effectively describes the typical facies succession during a 20 kyr cycle, why is it never entirely preserved?

Moreover, how do you estimate sea-level variations and sedimentary thicknesses in models A and B (respectively 3m and 1m)? Why does it differ between the two models?

How does this model relate to the relative sea-level curve proposed in Fig. 11 and to the

large-scale sequence stratigraphy context (Ox6 to Ox8): to which specific time intervals models A and B respectively relate? I could be added on the Figure.

Finally, a caption for the facies should be added (both for colors and allochems).

Technical corrections

See comments at line 20, 23, 43, 213, 292, 455, 634 in the PDF.

Please also note the supplement to this comment:

<https://egusphere.copernicus.org/preprints/2022/egusphere-2022-74/egusphere-2022-74-RC2-supplement.pdf>