

Interactive comment on “Bridging the Faraoni and Selli oceanic anoxic events: short and repetitive dys- and anaerobic episodes during the late Hauterivian to early Aptian in the central Tethys” by K. B. Föllmi et al.

K. B. Föllmi et al.

karl.foellmi@unil.ch

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Comments by Referee 1 and our comments and replies:

1. The title is too long and really not very sexy.

We shortened the title and propose: “Bridging the Faraoni and Selli oceanic anoxic events: late Hauterivian to early Aptian anoxic phases in the Tethys.

2. Abstract: What is RSTE standing for?

“redox-sensitive trace-element”: we changed “metal” in “element” in the abstract.

3. abstract: What are higher densities? That is explained later, but people normally read the Abstract first.

We agree with the reviewer and added a parenthesis: "(up to 50; calculated by the quantity and standardized thicknesses of organic-rich mudstone layers per time unit)".

4. The term northwest German Basin should be replaced by Lower Saxony Basin.

So we did.

5. Minor misspellings in the chapter Introduction. No line numbers, however.

We carefully went through the text and corrected the misspellings we found.

6. Chapter 4.3: Make clear tha you are talking about $\delta^{13}\text{CCaCo}_3$.

This is already very clearly stated in the original text, where we wrote: "The $\delta^{13}\text{C}$ record of the Breggia section shows comparable values and a consistent trend for both the carbonate and mudstone samples"; and "The $\delta^{13}\text{C}$ record of the Capriolo section is only shown for the carbonate samples".

7. Chapter 4.3: Delete the $\delta^{18}\text{O}$ discussion. Diagenesis here.

We followed the advise of the reviewer and omitted the description of the $\delta^{18}\text{O}$ records.

7. Chapter 5.3: Number the discussion wit same numbers as in the figures.

So we did.

8. Chapter 5.6: I am doubtful about the stratigraphic resolution, but a nice try and ok this way.

We agree. The stratigraphic resolution is directly dependent on the magnetostratigraphy and their translation into time.

9. The two unpublished BSc thesis can't be listed in the refs (Jammet et al, Bole et al.).

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Sorry, but these are not available and should thus not be cited. The editor has to think about a way of how the findings of these BSc students can be documented.

We omitted the references to the two unpublished BSc theses.

Fig. 1: Hm, Switzerland is not really indicated. Can you improve?

Not really, but the position of the two investigated sections is clearly indicated anyway.

Fig. 2: My version is difficult to read.

We checked the resolution and made sure that the figures are also well readable when highly magnified.

Fig. 6: The lowermost black shale in the Boreal succession is not quite correct, at least give a question mark here. Or you need to discuss the stratigraphy and correlation, which is a topic you better avoid.

We included a question mark for the lowest black-shale level.

Fig. 6: Explain the different lines better. Difficult to read and understand.

We rewrote parts of the figure caption.

Comments by Referee 2 and our replies (in italics): Abstract: - In some parts a bit repetitive, could be shortened without losing information.

We omitted a paragraph and shortened the rest of the abstract.

Introduction: - Regarding the introduction of the Faraoni-level: is it still a regional (say Tethyan event)? What is the climatic or paleoceanographic significance? This should be shortly discussed in the introduction and not only a list of locations.

We transferred a paragraph from chapter 5.7 to the introduction in order to show that possible equivalents of the Faraoni level are also known from basins outside the Tethys. We also added a paragraph including a general interpretation for the Faraoni and Selli events and a further sentence with our interpretation of the intervening anoxic events

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(i.e., during the Barremian and earliest Aptian).

Chapter 2: - How was the magnetostratigraphy “projected” onto the sections?

We did this by using two fix points: the top of the Maiolica Formation and the last appearance of *Lithraphidites bollii*. We compared our measured thicknesses between these two points with the thicknesses indicated in the published sections (with their magneto- and biostratigraphies).

- How can you use only a single point as a fix point to correct for differences in section thickness? Didn't it need at least two?

We used two fix points, the second being the top of the Maiolica Formation in both sections. We specified this in the revised text.

- The mention of Bachelor theses can be deleted since they are not available and the students are co-authors.

So we did.

Chapter 3.1: - What is the error against VP143h?

We corrected this phrase.

Chapter 4.1: - Lines 13-14: the higher TOC values in the lower Aptian are a result of one measurement of 14 permill. All other values are in the same range than the rest of the section. So how reliable is this statement?

The reviewer is right and we changed the phrase into "In the Capriolo section, all measured mudstone layers show relatively low TOC values, with the exception of one layer in the lower Aptian part of the section".

Chapter 4.3: - The entire paragraph about $\delta^{18}\text{O}$ is not necessary. It is diagenesis and you do not use it later in the discussion (see Chapter 5.3 line 13).

We omitted this paragraph (see also the remark by the first reviewer).

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Chapter 4.4: - To clarify this chapter it might be useful to show the mean values in the figure so that it is more clear where the values deviate from the mean. But: how significant are these mean values given the large spread of values?

We think that the data are sufficiently closely spaced to show the general trends and departures in both figures. We provided the mean values just as additional information and are happy to omit them, if it is found confusing.

Chapter 5.1: - Lines 18-19: usually, the values are below 4%!

This is true for the Capriolo section, but not for the Breggia section, where higher values are found. We adapted the phrase.

- Line 25: I am not an expert in this, but is a somewhat higher Tmax value of 430°C in contrast to 427°C really a signal?

Not significantly, but it is just an additional indication.

- What I am missing in this chapter is the meaning of all these data. What does it tell us to come up with a scenario about the formation of these organic-rich layers?

In this chapter, we discuss the TOC values and compare them to other sections. We furthermore discuss the HI and OI values. A more general discussion follows in chapters 5.5 to 5.8.

Chapter 5.2: - For clarification, the Redfield ratio could be included in the figures?

We did so by including a dark-blue line at 106.

- The entire chapter is more or less a repetition of the results. What is the meaning?

In the revised version, we omitted the descriptions and added a phrase with an interpretation of the Corg:Ptot molar ratios.

Chapter 5.4: - The scenario discussed at the end of the chapter is interesting. Could be a hint towards different bottom-water masses (probably warm saline waters) that

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precondition the basins and then a more local effect (e.g., enhanced productivity?) gives you the regional differences described herein?

Yes, a combination of these factors together with eventual differences in topography may have been involved. We added a phrase at the end of the chapter to invoke these possibilities.

Chapter 5.5: - 1st paragraph: please cite other studies that also show these species as characteristic features of the Faraoni.

We added a reference to the publication by Tremolada et al. (2009).

- If nannofossil work and stratigraphy was done, why is it not included in the figures?

The nannofossil analyses were only on a small selection of samples and served merely the purpose of verifying our correlations.

- Last paragraph: Why is Faraoni not called a single event? What is the evidence for that apart from two enriched layers in one of the studied sections? These organic rich layers are clearly of regional importance only and therefore vary from section to section.

The Faraoni event is not a single event, which was already shown by Coccioni, Baudin and others in central Italy, where minimally four thin and closely spaced layers occur. In the sections analyzed here, thin and organically enriched layers precede and follow the Faraoni levels, which may indicate that the two Faraoni levels are part of a larger period of environmental change.

Chapter 5.7: - Lines 12-13: That the presence was not excluded doesn't necessarily mean that it is there!

We agree but that is the way, how it was stated in this publication.

- 4th paragraph: if you mention that the drowning slightly predates the onset of the Faraoni then what is the time? You use an age model for calculation of OML density.

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Why is it not used to discuss the time differences or synchronetities between the platform drowning, the Faraoni and other occurrences of organic-rich layers in other regions

The onset of platform drowning predates the Faraoni event and starts already in the seitz zone (Vermeulen, 2005), which is equivalent to the younger part of the balearis zone (Hoedemaeker et al., 2003). We indicate this in the revised manuscript. The difference in the onset in platform drowning and anoxic conditions related to the Faraoni event is taken into account in chapter 5.8.

(Chapters 5.7 and 5.8). I know that this is a tough job, but looking at Figure 6, it seems to me that there is some kind of matching between the platform drowning events and the black layers, but it is not really good and identifying the time difference is crucial here to really argue for a common trigger. If there is a million years between the Faraoni and the platform response, it might be not the same mechanism. So given this, I would like to see a bit weaker statement at the end of the paragraph, because the periods of high OML density only fit somehow to the platform drownings but not good enough to really correlate them and to argue for a common trigger.

We agree with the reviewer. The problem is that we are obliged to indicate the oldest possible age for the onset of platform drowning and that platform drowning may effectively have started later. The dating is based on ammonite stratigraphy and particularly on the presence of Pseudo- and Parathurmannia. Their biostratigraphic ranges start in the younger part of the balearis zone and extend well into the ohmi zone, which is the youngest zone of the Hauterivian, or all the way to the Hauterivian-Barremian boundary. Ammonites exclusively indicative of the balearis zone are lacking and the onset of drowning is probably younger than this zone, but we cannot totally exclude an age as old as the younger part of the balearis zone (cf. Bodin et al., 2006). In spite of these uncertainties in dating, which are also given for the different occurrences of the Faraoni levels and their equivalents (Philip Hoedemaeker, personal comment), we remain convinced that the two phenomena are linked. Ammonites indicative of the latest Hauterivian are present in the condensed sediments related to platform drowning

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and confirm that drowning occurred during the Faraoni event. The offset in the onset of platform drowning and OAE's is also observed in the Valanginian and Aptian and indicates that the carbonate platforms are most vulnerable to environmental change and are amongst the first ecosystems, which were affected (and the last, which recovered....), whereas more time is needed for an OAE to develop.

Chapter 5.8: - Line 25: add references.

So we did.

- The $\delta^{18}\text{O}$ data presented show no consistent trends and I don't think that they are really necessary and help to understand the scenario that is promised in the headline.

We prefer to leave the $\delta^{18}\text{O}$ data: they are a source of additional information on paleo-environmental change, especially in the case when the bulk-rock and belemnite $\delta^{18}\text{O}$ records converge.

- Here I got a bit disappointed with the manuscript. The heading of the chapter proposes a scenario of paleo-environmental change for the local organic-rich layers. But it is only a repetition of the other chapters and some statements about $\delta^{18}\text{O}$ data sets without any hint towards a scenario. The authors speak about teleconnections but never say what they mean with this. Here, I would like to see a (off course speculative) discussion about possible mechanisms that fit to their data. What about changes in productivity to form the organic-rich layers? What about water-mass changes sensu Friedrich et al (2008; Nature Geoscience) and Berrocoso et al (2010; Geology) for the tropical Atlantic and therefore a better preservation of organic matter? What about orbital forcing? The paper is a really good study of organic-rich layers outside the well-known OAEs and therefore should at least come up with some speculations about the formation of these layers (and probably their differences to OAE formation). Going further along this line: Why are the authors think that all of these black shales are triggered by the same mechanisms? There are plenty of studies out there that show large differences of even closely spaced local black shale layers e.g. in the Aptian of

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the Vocontian Basin (e.g., studies by Breheret 1997, Herrle et al 2003, Friedrich et al 2003, to mention only a few).

We do not agree with the reviewer in all details... New in our contribution is the observation that shorter dys- to anaerobic episodes occurred between the Faraoni and Selli events, which are probably correlated on an interbasinal base and also with environmental change in shallow-water environments. We cannot go much further in our interpretations since we don't have the corresponding data. We would very much like to speculate about circulation pattern in the Atlantic and changes therein as suggested by the reviewer, but before doing so, we need to have better data from this area. For the moment, organic-rich layers are known from this area for the time period under consideration; they are, however, poorly dated and cannot be correlated on a bed-to-bed scale. Also for the other themes proposed by the reviewer, we feel that we do not have the corresponding data to substantially contribute to those subjects. In order to partly satisfy the requests by the reviewer, we added a paragraph in which we stressed the role of nutrients and climate change, and its effects both on shallow-water carbonate platforms as well as on deeper basins.

Conclusions: - How does the proposed synchronicity between platform drowning and black shale formation works? What is the trigger mechanism? See comment above about the missing scenario.

We added a paragraph in the last chapter and completed the conclusions.

- I disagree with the statement in lines 18-20: There is no real "lead-up" to the OAEs. Especially before OAE 1a, there is a time of low OML-density. For me the OAEs are still looking like singular events. But probably it is a more cyclic pattern that can be seen. Looking at Figure 6, it seems a bit like a cyclicity that forms intervals of higher OML-density at some times and OAEs at others.

OAEs are preceded by repeated intervals of dys- to anaerobic conditions, by the onset in platform drowning, and partly also by Nannoconus crises in deeper water. These

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observations show us that OAEs are not the result of catastrophic events, but originated during longer periods of environmental change.

Figure 1: - Is it Stein et al 2011a or b?

Stein (2011).

Figures 2 and 3: - Where are the open circles? Writing is very small and hard to read. Overall, the figures I have are very hard to read. Please re-draw in a more reader-friendly way.

Originally we designed separate figures for each measured parameter. After internal review, we decided to combine all data in two figures. We checked the figures and they have sufficiently resolution to show all the data in detail. We will verify that this resolution is maintained in the final version.

Figure 6: - Again very hard to read. Please identify the different lines more clearly.

We changed the caption and added text and hope that this figure is clearer as such.

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