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Climate of the Past
Discussions

Interactive comment on "Driving mechanisms of organic carbon burial in the Early Cretaceous South Atlantic Cape Basin (DSDP Site 361)" *by* Wolf Dummann et al.

Anonymous Referee #2

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This manuscript argues that variation between high TOC and modest TOC Aptian black shales at DSDP 361 in the Cape Basin was forced by changes in precipitation patterns over southern Africa. These relatively short-term climatic variations are proposed to be separate from a longer-term tectonic shifts that led organic carbon-rich deposits to be replaced organic carbon-poor deposits from the Aptian through the Albian at this site. Primary evidence cited in support of the climatic interpretation is biomarker analyses, REE distributions, and modelling results. In addition, elemental abundances and other bulk geochemical results are discussed relative to potential diagenetic overprints and redox conditions in the water and sediment column. Data are largely limited to a portion of the lithologies present at one site that seemingly had quite incomplete recovery.

The interpretation is interesting and consistent with the data presented, but to fit the observations, the interpretation does invoke a rather specific set of conditions (e.g., changes in seasonality and geographic focus of precipitation leading to more precipitation and nutrient input from chemical weathering in the interior but more physical weathering along the coast) that are difficult to test. The study also leans heavily on Dummann et al., 2020, EPSL, for data and documentation to the point that I wonder if the last sentence of the first paragraph of the discussion of the EPSL paper refers to the data split off and presented in this manuscript. Regardless of decisions made by authors about whether and how to partition findings among publications, this manuscript would be stronger if the unique contributions of this study were featured and less prominence was given to the broader and longer-term topics addressed in the EPSL paper.

Emphasizing and expanding presentation of sedimentological data would be one route to this end especially given the importance of deposition patterns and sediment sources in the conclusions. Currently the lithologic data presented are relatively sparse. The paper proposes 3 (or 4) lithostratigraphic units are present that can be differentiated based on color and TOC of the finest grained lithologies. Is there any information on mineralogy, bedding characteristics, or sedimentary structures (physical or trace fossils)? Description of the samples analyzed, their context, and why they were chosen is effectively missing (both for the 'complete set' and the presumably subsets thereof in plots with < than 131 data points). What about the coarser grained lithologies? It seems these lithologies are an important part of the depositional system, but they are mentioned mostly for having been excluded from consideration. The graphic column, on the other hand, seems to show changes in the proportion of lithologies through time (as well as substantial coring gaps). How does the stratigraphic pattern of different lithologies fit into the provenance/sediment routing interpretation proposed? Petrographic and geochemical examination of the coarser lithologies could provide information about provenance and transport. Do K/Al and Si/Al measures of sedimentologically identified distal turbidites support the assertion that hemipelagic deposition and turbidites can be separated by the metrics used at this site. How was the 5% organic carbon threshold chosen? Are there really two different lithologies (<5% and >5% Corg) within fine-grained portions of the "black shales" or is there a continuum? What is the stratigraphic character of the most TOC-rich intervals– discrete beds (how thick?) or trends over some thickness (cm? dm? m?). One way to illustrate/test this classification would be a plot of K/Al vs. %organic carbon (cf. Fig. 3b). Statistical tests of apparent geochemical patterns would enhance confidence in the relationships proposed. Were there efforts to separate lithogenic from hydrogenic contributions to the REE profiles measured? Could the differences in 3c-d between bulk analyses reflect oceanographic/diagenetic conditions correlated with organic carbon content rather than changes in sediment provenance causing changing in oceanographic/depositional conditions? Some of this information is in the EPSL manuscript and I assume some is in the DSDP volumes cited, but this study would be stronger if more supporting sedimentological and stratigraphic information was summarized and synthesized in the manuscript.

Along similar lines, the presentation of the modelling and modelling results is limited although those results arguably provide the only independent assessment of the model proposed. Description of the model (2.4) is brief and and the changes in bathymetry between the Dummann et al. 2020 EPSL paper's and the bathymetry used in this paper is not illustrated. The paleogeographic figures in the EPSL paper are superior to the rather simple Fig. 1 in this manuscript. In figures 9 and 10 it would be nice to see a panel that has the 1200 ppm CO2 results shown rather than simply showing 600 ppm results (9b, 10a) and a difference (9c, 10b). The idea that an intermediate water mass isolates the deep Cape Basin promoting organic carbon accumulation is an interesting model-based suggestion. Could cross sections across the basin showing model predicted water properties that indication a saline layer divides the water column be provided as well as sensitivity tests of the persistence and extent of such a layer under the two climatic modes proposed to explain the distribution of TOC in the black shales? Could discussion and supporting graphics demonstrating the models have stabilized be provided especially relative to the deep basin?

Finally, the manuscript suggests that increased CO2 might be the cause of the proposed changes in precipitation leading to nutrient input and enhance physical weathering proposed to explain organic carbon enrichment associated with OAE1a. The manuscript also suggests orbital variations might be the cause of the other organic carbon rich black shale intervals. Are the authors proposing that orbital variations cause a change in CO2? If so, how? If they instead are suggesting both orbital variations and CO2 increase cause the same climatic changes, this would be an idea that seems worth including in section 4.6. Would the authors hypothesize what orbital configuration would be expected to mimic high CO2 precipitation patterns and why? Such a prediction could be tested when/if an orbital timescale is generated for the Aptian Cape Basin.

55- does the mid-ocean ridge represent a barrier to circulation? 57- how is global excess OC burial defined/calculated 59- Angola Basin not shown in Figure 63- which data- TOC percentages? 87- beyond TOC (and color?) are there sedimentological differences among lithostratigraphic divisions, gray shales and low-TOC black shales do not seem different based on the criteria used 95- describe sample set 126, 235- don't start sentence with lowercase letter, write around 144- not sure this is a method, odd to exclude sandstones for provenance work 149- These data 177- very different precision suggested for characteristic AS Si/Al and Zr/Al ratios 180- difference seems small given variance- is it statistically supported. What does a plot of TOC vs. K/Al look like? 254- is proposed difference statistically supported?

Figure 2- using red shading for organic carbon rich black shales and orange shading for red beds seems like an odd choice; panels d-f are labeled grain size/mineralogy, but the plots show elemental ratios.

Supplementary information could include some description of the plots.

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