Paper summary:

Dummann et al. investigated the mechanisms driving organic carbon burial during the Aptian–Albian in the deep Cape Basin at Deep Sea Drilling Project (DSDP) Site 361. To that purpose, the authors produced abundant (bulk and molecular) geochemical data and conducted general circulation model runs using the ocean-atmosphere Kiel Climate Model. Dummann et al. identified two main temporal trends based on the sedimentary record: (1) a long-term trend toward more oxygenated conditions, as can be seen based on the changes in sedimentary facies from black shales to grey shales and red beds, and (2) a higher-frequency change, inside the Aptian black shale unit, between low-TOC black shales and high-TOC black shales (the latter comprising OAE1a, but also other layers). The authors explain the reported long-term change in deep-water redox conditions at DSDP Site 361 by a reorganization of the ocean circulation in response to the evolution of marine gateways, based on the findings of their previous study (Dummann et al., 2020). Regarding the high-frequency cycles, proxy data demonstrate a common geochemical signature of the high-TOC black shales, suggesting that the same mechanisms potentially drove the deposition of the successive layers. These data point toward an increasing contribution of marine organic matter during the deposition of these high-TOC shales. The systematic increase in the K/Al ratio during these periods suggests that the latter may correspond to periods of increased precipitation and runoff from the continent, bringing unweathered sediments and also nutrients to the ocean, thus explaining the increase in marine primary productivity and organic matter deposition under anoxic–euxinic conditions. In order to check this hypothesis, the authors conducted 2 climatic simulations at 600 ppm and 1200 ppm CO2, supposedly representative of Aptian background climate and OAE1a. Although total precipitation over the region of interest does not significantly increase in response to climate warming in the model, spatial contrasts strengthen and runoff during the austral winter doubles. From this point of view, simulations support the authors' hypothesis.

General comment:

As a paleoclimate modeler interested in proxy data but not expert in geochemistry, I really appreciated the manuscript. Geochemical analyses and results are exposed in a very clear and accessible way. The authors further use a climate model to validate the hypotheses established based on the analysis of the geological record. The manuscript is well organized and the statements are mostly very well supported by analyses/figures of high quality. I recommend rapid publication of this study after minor revisions.

Major comments:

1. Model-data comparison and the change in sediment supply between low-TOC and high-TOC shales. On lines 414–416, the authors "*propose that the shift in provenance from low- to high-TOC black shales reflects enhanced moisture supply to the continent's interior, augmenting river run-off in the upstream regions of the paleo-Karoo River and/or increasing chemical weathering intensity of basaltic rocks*". However, results of the climate model suggest that coastal S and SW coastal regions became more humid during OAE1a-like warming events, while the continent interior became more arid. These two elements – the hypothesis derived from proxy data and the results of the GCM – thus seem in contradiction. This discrepancy should be resolved.

2. The paleo-Karoo River. The existence of this river has a major role in the hypotheses drawn in this contribution. Are the arguments supporting (1) the existence of the river and (2) the location of the river mouth during the Aptian (especially lower Aptian) robust? Topography, in particular, is generally very difficult to reconstruct in deep-time periods, thus making the reconstruction of watersheds challenging, too. I suggest expanding the arguments supporting the existence of this river, of its watershed such as drawn in Fig. 3 and of the location of the river mouth. If arguments are solid, such discussion will strengthen the manuscript. If they are not, I still believe that the authors' arguments are very interesting but I think it would be good in that case to highlight these uncertainties as a limitation.

3. GCM simulation and OAE1a. The authors suggest that a pCO2 doubling from 600 ppm to 1200 ppm is well representative of the climate change that accompanied OAE1a. The choice of the pCO2 values is based on the argument that these values fall "*within the lower range of pCO2 estimates for OAE 1a (Naafs et al., 2016)"* (also lines 160–163). However, different climate models have different climatic sensitivities, i.e., a given change in pCO2 will not have the same effect depending on the climate model in use. Is the climate change simulated in the Kiel model between 600 and 1200 pm really representative of what we know of OAE1a? Interestingly, recent temperature estimates (Naafs and Pancost, 2016; O'Brien et al., 2017) provide an overview of the ocean warming that accompanied OAE1a. I think it would be instructive to provide a rapid comparison between the modeled temperatures and these recent proxy data. Such comparison would satisfactorily complement the interesting comparison of the modeled climatic belts with the lithological indicators of climate already proposed by the authors.

Minor comments:

1. Lines 441–442: the authors state that "*The climate zonation generated by our model indicates that Site 361 and the southern African continent were located in a temperate humid climate belt*". How was this climatic belt identified? Was is through some Köppen classification or similar? Please support this statement.

2. Lines 444–446: I suggest including a reference to the modeling study of Chaboureau et al. (2012), which specifically investigated the controlling mechanisms for the deposition of evaporites in the northern and central sections of the nascent South Atlantic Ocean.

3. Lines 464–466: I suggest comparing the precipitation simulated at 600 ppm with the precipitation simulated in modern S-Africa in the present-day model control run. This might be more instructive (consistent) than comparing to an independent dataset.

4. Lines 496: "*no substantial ocean upwelling occurred along the SW African margin during the Aptian*". Here as well, I suggest approaching the question of the limitations. Indeed, the authors are here reaching the limit of the modeling setup. Coastal upwellings s.s. are 10s of km wide, not 100s. As a consequence, they cannot be captured by the ocean model, the mean horizontal resolution of which is ca. 200 km.

5. Figs. 9–10: Since the position of Site 361 is critical, please state how its paleocoordinates were calculated and how the point was subsequently located on the mixed Blakey-Sewall map.

Technical comments:

- Line 129: "*The distribution of acyclic hydrocarbons (i.e. n-alkanes, acyclic isoprenoids) ~~were~~ was analyzed*"
- Line 207: "*Distribution of the redox-sensitive trace metals (TMs) Ni, Cu, V, and Zn in black shales ~~are~~ is presented as*"
- Line 417: "*furthermore*" is misleading because the new idea is not used to support the previous paragraph.
- Fig. 2 caption: Missing general title for the Fig.
- Fig. 9 caption: "Positive contours" > positives values (there are no contours).

References cited:

Chaboureau, A.C., Donnadieu, Y., Sepulchre, P., Robin, C., Guillocheau, F., and Rohais, S., 2012, The

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Naafs, B.D.A., and Pancost, R.D., 2016, Sea-surface temperature evolution across Aptian Oceanic Anoxic Event 1a: Geology, v. 44, p. 959–962, http://geology.gsapubs.org/lookup/doi/10.1130/G38575.1.

O'Brien, C.L. et al., 2017, Cretaceous sea-surface temperature evolution: Constraints from TEX 86 and planktonic foraminiferal oxygen isotopes: Earth-Science Reviews, v. 172, p. 224–247, doi:10.1016/j.earscirev.2017.07.012.