

Interactive comment on “Miocene–Pliocene stepwise intensification of the Benguela upwelling over the Walvis Ridge off Namibia” by S. Hoetzel et al.

Anonymous Referee #1

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Miocene–Pliocene stepwise intensification of the Benguela upwelling over the Walvis Ridge off Namibia

By Hoetzel et al.

This paper presents an interesting contribution on the history of the Benguela upwelling during the late Miocene to Pliocene, a topic that has been in focus lately. The authors investigate the Miocene and Pliocene of ODP Site 1081 with the aim to reconstruct sea surface conditions in the Benguela upwelling region and identify forcing mechanisms during the Late Neogene global climate cooling. The paper is well-written and the applied methods (dinoflagellate cyst assemblages and TOC) are very much suit-

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able for this purpose. Nevertheless, I have concerns about the dinocyst methods and interpretations that should be addressed before final publication.

GENERAL COMMENTS/QUESTIONS

1. The authors mention the data is available via pangeae.de, but this was not the case at the time of review. It is also not clear what data will be made available (sedimentation rates, dinocyst counts, TOC?). The authors have a track record of publishing their data online, but the absence of the raw data has hindered the review.
2. The dinoflagellate cyst interpretations are somewhat selective, and appear not to make use of the entire available literature (see further). Especially the interpretations of the taxa *L. machaerophorum*, *O. centrocarpum* and *B. micropapillata* complex can be questioned. More careful consultation of the available literature is necessary here. This may have its consequences for the upwelling history reconstruction.
3. It is not clearly discussed why the different proxies for upwelling do not show the same evolution over the studied interval. TOC is increasing over the studied interval, but dinocyst accumulation rates and H/A index are merely fluctuating (Figure 4).
4. There is also no comparison with the very relevant dinoflagellate study of Udeze and Oboh-Ikuenobe (2005), which presents an interpretation of the upwelling history of the Benguela Upwelling region from three ODP Sites drilled during the same Leg as ODP Site 1081.
5. Discussed forcings that potentially influenced the upwelling region include uplift of Africa, effects of the Mediterranean Salinity Crisis and ocean gateway closure on AMOC and NADW. Why was the Agulhas system and its influence on AMOC (e.g. Beal et al. 2011) not considered to have an effect on the oceanography at the study site?

DINOFLAGELLATE CYST METHOD AND INTERPRETATION COMMENTS

METHODS

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1. The absence of the raw data make a critical review of the entire dinocyst assemblage impossible. It is not clear whether the same taxa were recorded as from the nearby ODP Sites 1085, 1086 & 1087 within the Benguela Current system by Udeze and Oboh-Ikuenobe 2005 (Palaeo-3). A comparison with U&O-I2005 should be made in this study.

2. It appears that only a part of the available Miocene–Pliocene literature has been used for identifications (1919/10–11). Were really only four papers used to identify the dinocysts? Why was the entire Miocene/Pliocene dinoflagellate cyst literature not used (e.g. publications by Head, Louwye, McMinn, Matsuoka, Manum, etc.)?

3. It needs to be clarified which *Operculodinium centrocarpum* is recorded. Is this *O. centrocarpum* s.s. (Matsuoka et al. 1997 - Palynology) or cysts of *Protoceratium reticulatum* (aka *O. centrocarpum* sensu Wall and Dale, 1966) (Paez-Reyes and Head, 2013 – J Paleontol). Both species occur in the Miocene–Pliocene, so this distinction is essential.

4. The different taxa of *Batiacasphaera* (*B. micropapillata*, *B. hirsuta*) are difficult to identify and can be misidentified easily (see discussion in Schreck and Matthiessen (2013). A photographic plate showing these and other essential taxa for interpretations would provide more confidence in the determinations.

5. Reference is made to Lewis et al. (1990) for using a H/A ratio, but those authors use the number of gonyaulacean vs. peridiniacean species to make a P-G/P+G ratio to interpret upwelling. It would thus be better to use a P/G ratio here also, since this is more routinely done (see for example Versteegh, 1994 - Mar Mic; Sluijs et al. 2005 - Earth-Sci Rev). But be aware that the original P/G ratio (Harland 1973) is based on number of species, not specimens.

DINOCYST INTERPRETATIONS

1. *Lingulodinium machaerophorum* can be related to upwelling relaxation and in-

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creased stratification as was done in this study. However, it has also been considered as an inner neritic species (Versteegh & Zonneveld 1994 – RPP; Mertens et al. 2009 – MarMic) associated with nutrient input via rivers (e.g. Gonzalez et al. 2008 – Paleocenaography, Bouimetarhan et al. 2009 – MarMic, and refs therein). The latter option has not been discussed, while it may provide an alternative explanation for the occasional high abundances up to ~6 Ma and the following absence. Can the absence indicate reduced riverine input and hence reflect that the hinterland became drier (e.g. Dupont et al. 2013)? What do the pollen records (Hoetzel et al. 2013, 2015) tell about possible riverine input? Additionally, *L. machaerophorum* is a species preferring warm-water conditions. Could its decrease be related to a cooling of the surface water masses?

2. *B. minuta* and related morphotypes in Zegenes and Helenes (2011) only occur in low abundances and do not correspond to the recently described *B. micropapillata* complex. See discussion of the Zegenes and Helenes records on p. 295–297 in Schreck and Matthiessen (2013). The interpretation of *B. micropapillata* complex as an indicator for warm nutrient-poor conditions may not be warranted, since the species is tolerant of a wide range of SSTs (see discussion in Schreck and Matthiessen).

3. The zones could be more formally defined than just based on visual inspection (1920/5). The criteria for recognising Zone III are poorly constrained and especially the boundary with Zone IV seems to be randomly placed at 5.5 Ma. A boundary around 5 Ma when *Brigantedinium* increases and *B. hirsuta* disappears would maybe be a better choice. Zone V could last from 4.4 to 3.5 Ma, based on the high abundance of *O. centrocarpum*. This species has considerably lower abundance after around 3.5 Ma, while *Brigantedinium* and *Spiniferites* become more dominant. This can be interpreted as a shift back to more upwelling conditions (Zone VI?).

4. Can Benguela Niño conditions, an inter-annual phenomenon (1918/18-19), really be identified in the low-resolution sampling of this study (1923/23-29)? Is there any information about the dinoflagellate assemblage signature of a Benguela Niño conditions in the modern ocean?

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SPECIFIC COMMENTS/QUESTIONS TO THE TEXT

1915/8–10: How does a steeper meridional Miocene gradient relate to the Pliocene weak meridional gradient (e.g. Fedorov et al. 2013)?

1916/4–15: Recent new insights into CAS history and effect on NADW production and AMOC are not discussed: e.g. Montes et al. 2015 (Science), Sepulchre et al. 2014 (Paleoceanography), Osborne et al. 2014 (Paleoceanography).

1916/7: “the datasets” which datasets are referred to here?

1916/20–24: Please rephrase, to make clearer which papers discuss which site and time interval.

1917/4: Please show the alkenone record (on Figure 4?).

1918/25–26: Please provide a table with the calculated sedimentation rates. These are essential for the accumulation rates.

1919/5: Provide detail on the material used for sieving and its mesh size (see discussion on importance in Lignum et al. 2008). Getting rid of the fraction 10–15 μm may have removed small acritarchs.

1919/10–11: Were really only four papers used to identify the dinocysts? Why was the entire Miocene/Pliocene dinoflagellate cyst literature not used (e.g. publications by Head, Louwye, McMinn, Matsuoka, Manum, etc.)?

1919/16: “classified after its assumed metabolism mechanism” An assumed metabolism is not a good criterion for subdividing dinocysts. In fact, several of the autotroph taxa can be considered mixotroph (e.g. *L. machaerophorum*). Use P/G ratio instead.

1919/18: Please explain why the H/A ratio requires \ln transformation – a practice I have not encountered before.

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1920/1: There are very few taxa recorded ($n=36$). Is this normal for upwelling regions, the Miocene southern Hemisphere? How does this compare with Udeze and Obokunobode 2005?

1920/9-12: Odd phrasing

1920/12: *N. labyrinthus* is a cyst-based name. Remove “cysts of”.

1920/13: *Impagidinium* sp. 2 of De Schepper and Head (2009) is the correct name for this taxon. Check throughout.

1921/23: life cycle

1921/25: de Vernal [small d]

1922/8: Lewis et al. (1990) use a P-G/P+G ratio to interpret upwelling, not a H/A index. Also, why is it necessary to use the \ln of H/A in Figure 4?

1922/9: reference for competition with diatoms?

1922/16: Specify “exceptional conditions” in terms of palaeoceanography

1923/1: rephrase to make clear whether you mean the presence of one species and absence of another

1923/6-10: The claim that *B. micropapillata* is consistent with SSTs of 26–27 degC is not warranted. See discussion of the Zegenes and Helenes records on p. 295–297 in Schreck and Matthiessen (2013).

1923/11: Please show the alkenone SST record in Figure 4.

1924/13: Can an ocean front be weak/strong? What is meant here? Terminology describing fronts is vague throughout the manuscript: a front is a zone or a boundary, so it is unclear what is meant when a front is qualified with descriptive terms as weaker, steeper, more intense, etc. (see also 1924/18, 1928/11, 1928/19)

1924/23: Specify “exceptional conditions”. What does this mean in terms of upwelling?

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1925/14: maximum (not max)

1925/14-15: providing pictures of *H. tectata* and *B. hirsuta* would provide the first illustrations of both taxa for the southern hemisphere. Please include a photographic plate of the dinoflagellate cysts discussed in the text.

1925/26-28: How can the different interpretations from heterotrophic dinocysts and H/A vs TOC be explained?

1926/7: I don't see how high abundances of *B. hirsuta* suggest downward mixing and poorer quality of upwelling. Please explain.

1926/16: Diatoms are not the only source of food for heterotrophic dinocysts.

1927/1-3: Please show the discussed SST records in a figure.

1927/16-18 and 1928/6-7: *O. centrocarpum* is not a good indicator for river outflow (e.g. Bouimtarhan et al. 2009; Holzwarth et al. 2010) and is also not consistent with more intense upwelling (e.g. Marret and Zonneveld 2003). Intense upwelling can be inferred from around 3.5 Ma onwards, when *O. centrocarpum* decreases in abundance and *Brigantedinium* becomes important again.

1928/17-18: "Slightly warmer" in comparison to what?

1928/18-23: A northward shift of the meteorological equator or an expansion of the tropics?

1929/15: rephrase "representations"

1929/20: Please refer to most recent literature (Montes et al. 2015 - Science, Sepulchre et al. 2014, Osborne et al. 2014 - Paleocyanography)

1930/12: See earlier discussion on *L. machaerophorum*

1930/13-14: "...shown by decreases of warm water taxa and increases in indicators of cold and nutrient rich conditions" This has not been explicitly discussed. Which are the

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warm water taxa, which are the cold water taxa was not mentioned in the main text.

1930/15: Is Messian Salinity Crisis really the only possible explanation for the record between 6.8 and 5.2. A bit more caution may be warranted.

1930/25-27: The record of *O. centrocarpum* is not straight forward to interpret and a link with river discharge needs better support.

1931/4: Matthiessen

1931/6: Please provide doi and publish all raw data

1932/4–18: Spelling of author names needs checking throughout MS: de Vernal, de Verteuil

1936/8: examples

1936/12: Garcia

1937/1: Oboh-Ikuenobe

1937/1: Matsuoka (although this is misspelled on the original publication)

Table 1.

Group the species according to gonyaulacoids or protoperidinioids, not autotroph/heterotroph.

Please write all species names in full.

The correct names for following taxa is: *Impagidinium* sp. 2 of De Schepper and Head (2009), Cysts of *Pentapharsodinium dalei*, *Selenopemphix conspicua* (see Louwye et al. 2004), *Trinovantedinium ferugnomatum*, *Sumatradinium soucouyantiae*

Figure 1b – caption last sentence not clear: "... full uplift minus one without..."

Figure 2, 3.

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Can Figure 2 and 3 not be combined into one?

Figure 4.

What does the horizontal line represent in the Cyst accumulation rates log and Heterotrophs vs Autotrophs $\ln(H/A)$ graphs?

Include the discussed SST records (Hoetzel et al. 2013, Rommerskirchen et al. 2009) in this Figure.

Figure 4 caption

L6: Angola Benguela Front [add t]

L7: Roberts

L10: Seaway

Interactive comment on Clim. Past Discuss., 11, 1913, 2015.