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Climate of the Past Discussions

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

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We thank the reviewer for the conscientious and detailed comments and suggestions, which will help to improve the upcoming version substantially. We know how much work that is. Thanks a lot! Below, we give detailed answers to the comments and questions (denoted between <>) and what we will change in the revised version of the manuscript.

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

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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.>

Indeed, we mention that the data are available and forgot to specify “upon acceptance”. We apologize for this omission. Regrettably, we cannot comply with the reviewer’s suggestion to publish the data before acceptance as peer-reviewed publication. However, as soon as the paper is accepted we will upload all dinoflagellate cyst counts and the TOC data.

<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.>

Our interpretation of the record of O. centrocarpum sensu Wall et Dale 1966 is already very tentative. We point out that it concerns cysts of cosmopolitan species and that it indicates either nutrient-rich water or river outflow or both (page 1927, 5.6). The interpretation of the rerouting of the Cunene River to the Atlantic is based on the pollen record and is only mentioned here (page 1928, second paragraph). We agree that it is misleading to repeat that in the abstract and the conclusions. We’ll delete those references.

Below, under Dinocyst Interpretations, we answer the comments about the interpretation of L. machaerophorum and B. micropapillata.

<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).>

The H/A index and the dinocyst accumulation rates show different aspects than TOC. Since sedimentation rates decrease in Zone III and especially in Zone V, the relative

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amount of organic carbon is higher although less carbon is deposited. Additionally, other microorganisms (e.g. diatoms) could have been more important. Therefore, our interpretation is based on more than one proxy.

<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.>

Detailed comparison with Udeze & Oboh-Ikuenobe (2005) is difficult as the temporal resolution between the records is different. Also this record is from the other site of the BUS. We agree with the reviewer that a general comparison is appropriate. We'll add at page 1924 (5.3): "and Udeze and Oboh-Ikuenobe (2005) suggested increased upwelling during the Late Miocene based on dinoflagellate cysts analysis." and another reference at page 1927.

<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?>

Indeed, Agulhas rings influence the southern part of the BUS. Then they cross the Atlantic with the Benguela Ocean Current. Unless we assume a radically different pathway for the Benguela Ocean Current, we cannot expect Agulhas rings as far north as the Walvis Ridge. The effects of Agulhas Leakage on global climate might be considerable, but to our knowledge no data are available for the period under study. (This might change after the IODP Expedition 361 "Southern African Climate", scheduled next year, which has also a focus on the Agulhas system.) Neither are model simulations for the Miocene available that have sufficient resolution to investigate changes in Agulhas Leakage. Thus, a discussion about the Agulhas Leakage at this point would

be rather speculative.

DINOFLAGELLATE CYST METHOD AND INTERPRETATION COMMENTS METHODS

<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.>

The distance between ODP Sites 1085-1087 and 1081 is about 10° of latitude and the sites are situated at opposite ends of the BUS. Detailed comparison with Udeze and Oboh-Ikuenobe (2005) is not only hindered by the difference in geography and temporal resolution but also because raw data are not provided. Hence, we cannot compare our assemblages with those of Udeze and Oboh-Ikuenobe (2005) in detail. Nevertheless, we added qualitative references at pages 1924 and 1927. See also under points 1 and 4 of the previous section. Upon acceptation of the manuscript for publication in CP, we will publish the raw data in Pangaea, but not before.

<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.)?>

More literature has been consulted than the four papers that were mentioned as the more important ones used in the identification of the cysts. We'll add "among others" at page 1919, second paragraph.

<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,

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2013 – J Paleontol). Both species occur in the Miocene–Pliocene, so this distinction is essential.>

We recorded O. centrocarpum sensu Wall et Dale, 1966. We'll add the clarification at page 1919, end second paragraph.

<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.>

We agree that identification is difficult. However, we doubt that a photographic plate showing the best specimens would really provide more confidence. And documentation of the variability we encountered would be beyond the scope of the paper and maybe beyond the scope of CP. Instead, the first author consulted both Michael Schreck and Jens Mathiessen personally at the AWI in Bremerhaven (see acknowledgements). We'll specify this on page 1919, second paragraph.

<5. Reference is made to Lewis et al. (1990) for using a H/A ratio, but those authors use the number of gonyaulacean vs. peridiniacaean 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.>

We apologize for the inappropriate reference of Lewis et al. (1990) and we'll cite more recent literature on page 1922, instead (Verhoeven and Louwye, 2013; Bringué et al., 2014). We feel that the use of the H/A ratio relates better with productivity, as it is not exclusive compared to the G/P ratio. Heterotrophic and autotrophic groups are more comprehensive and better linked to productivity than gonyaulacacean and peridiniacean ones. We note that the mentioned papers display the ratio between heterotrophic cysts over the sum of heterotrophic and autotrophic cysts, while we calculate

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the ratio of heterotrophic over autotrophic cysts. As a simple ratio would put exaggerate emphasis on outliers, we log-normal transformed the ratio (will be explained on page 1919). The transformation is symmetrical around zero [ln(1), denoting as much heterotrophic as autotrophic cysts]. While we present this ln(H/A) ratio, we'll keep the arrangement in Table 1.

DINOCYST INTERPRETATIONS

<1. Lingulodinium machaerophorum can be related to upwelling relaxation and increased 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 – Paleoceanography, 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 âĹij6 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?>

To cover this point we'll add at page 1923, last paragraph the following sentence: "L. machaerophorum in an inner-neritic setting - which is not the case at ODP Site 1081 - has been associated with nutrient input by river discharge (Versteegh and Zonneveld, 1994; Bouimetarhan et al., 2009). Although the pollen record of ODP Site 1081 indicates more humid conditions on the continent, specific indicators for river discharge have not been found for the Late Miocene (Hoetzel et al., 2015).

The disappearance of L. machaerophorum in relation to the cooling of water masses has been mentioned at pages 1924 and 1927 (5.4 and 5.6). On page 1927 we write: "L. machaerophorum is, however, completely absent, indicating that the partly warm strat-

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ified conditions of the Miocene have been completely replaced by stronger upwelling, better mixing, and cooler conditions."

<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).>

The reviewer is right and we apologize for the mistake. Schreck and Matthiessen (2013) point out that the described B. micropapillata morphotype of Zegarra and Helenes (2011) (pers. communication) is not in the same complex as of Schreck and Mathiessen, while our type definitely is as the first author showed it to them (see above). We'll delete the interpretation at page 1923.

<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?).>

Thank you for the suggestion! We'll change the zoning scheme.

<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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No, we do not claim to identify Benguela Niños. We very carefully tried to formulate this point properly when we wrote: “It is possible that these Benguela Niño events were more common during Miocene times when the SAA and the trade winds were weaker.” We do not say that we identify Benguela Niños, but that we might see the effect of their possible more frequent occurrence in the Miocene. However, in the conclusions (6) our statement about the Benguela Niños is not as careful and not correct. We’ll change the statement into: “and more influence of the Angola Current in the region. . .”

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)?>

It was probably still weaker in the Miocene. We’ll change steeper to steepening.

<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).>

That is true. Our main point should have been the changes in the Atlantic Deepwater formation circulation and not the closure of the CAS. Therefore we’ll change the paragraph as follows:

“More important to the conditions in the BUS would be the strength and quality of the deep water formation in the Atlantic. According to Billups (2002), the closure of the CAS had influenced the ocean currents between 6.6 and 6 Ma by increasing the Atlantic overturning circulation (AMOC). However, the timing of the CAS closure is strongly debated (Osborne et al., 2014; Sepulchre et al., 2014; Montes et al., 2015). In any case, data sets show a cooling of Southern Ocean upper circumpolar and intermediate waters during the latest Miocene (Billups, 2002; Poore et al.; 2006). Poore et al. (2006) calculated the proportion of North Atlantic Deepwater (Northern Component Water) and describe an increase of Northern Component Water formation as well as a stronger

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AMOC for the period between 6 and 2 Ma related in the first place to changes in the overflow at the Greenland-Scotland Ridge. On the other hand, the influence of the closing of the CAS on the AMOC is underlined by models such as the one used by Butzin et al. (2011) who concluded that the formation of North Atlantic Deepwater (NADW) began when the CAS had shoaled to a few hundred meters during the Late Miocene."

We'll delete the misleading remark on page 1929 (section 5.7, second last paragraph).

<1916/7: "the datasets" which datasets are referred to here?>

The references to Billups (2002) and Poor et al. (2006) will be added (see above).

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

We'll repeat the appropriate references.

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

OK, we'll insert the alkenone-SSTs in Fig. 4.

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

The sedimentation rates that we used have been published by Berger et al. (2002) and shown in Figure 4.

<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.>

8 μm screen; we'll add the information.

<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

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Head, Louwye, McMinn, Matsuoka, Manum, etc.)?>

We'll add "among others" (see also above).

<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.>

We disagree (see above). The distinction between gonyaulacacean and peridiniacean is often as much an assumption as the heterotrophic/autotrophic one.

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

log-normal transformation is less sensitive to outliers and symmetrical around zero. It is often used in the presentation of elemental ratios measured with XRF.

<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 Obohlkuenobe 2005?>

U&O 2005 recognized 35 dinoflagellate species and 3 acritarchs. Lewis et al. (1990) recognized only 23 at ODP Site 686 in the tropical southern Pacific upwelling area offshore of Peru. We do not consider 36 taxa to be a low number and is in fact fairly average for this latitude.

<1920/9-12: Odd phrasing>

<1920/12: N. labyrinthus is a cyst-based name. Remove "cysts of".>

will do

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

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done

<1921/23: life cycle>

thanks

<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?>

see above

<1922/9: reference for competition with diatoms?>

Smayda and Reynolds, 2003; we'll add the reference.

<1922/16: Specify "exceptional conditions" in terms of palaeoceanography>

We do specify the exceptional conditions as "increasing TOC and warmer SST", which is an unusual combination.

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

We'll delete that part of the sentence.

<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).>

Yes, we'll delete this part (see above).

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

will do

<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,

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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)>

Meant is the temperature gradient over the frontal system. We'll change the sentences, accordingly.

<1924/23: Specify "exceptional conditions". What does this mean in terms of upwelling?>

See above. We use the section 5.4 to explain and specify what we mean with those exceptional conditions. We'll put "exceptional conditions" in quotation marks.

<1925/14: maximum (not max)>

thanks

<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.>

We feel that would be beyond the scope of the paper and not fitting for a journal with such a wide public as CP.

<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.>

Differences between H/A and TOC may be explained by an increase in other microorganisms (including everything which ends up as amorphous organic matter) or a reduction in sedimentation rates (clastics).

The downward mixing at ODP Site 1085 (Rommerskirchen et al., 2011) might have affected the subsurface waters at the southern end of the BUS and that these altered subsurface waters changed the quality of the upwelled waters at the northern end of the BUS and consequently the flora of the northern BUS.

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

Heterotrophic species do feed on diatoms even if they might eat other things as well.

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

see above.

<1927/16-18 and 1928/6-7: O. centrocarpum is not a good indicator for river outflow (e.g. Bouimetarhan 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.>

The presence and dominance of O. centrocarpum between 4.3 and 3.4 Ma (Zone V) . . .. would indicate nutrient-rich and well mixed waters representing conditions adjacent to strong upwelling and/or river outflow (Dale et al., 2002).

<1928/17-18: “Slightly warmer” in comparison to what?>

to after 8 Ma.

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

Could be both, but the effect would be the same.

<1929/15: rephrase “representations”>

We’ll drop “the representations”.

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

We’ll delete the sentence. Dating of the closure of the CAS is beyond the scope of the paper (see also above).

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<1930/12: See earlier discussion on L machaerophorum>

<1930/13-14: ". . .shown by decreases of warm water taxa an increases in indicators of cold an nutrient rich conditions" This has not been explicitly discussed. Which are the warm water taxa, which are the cold water taxa was not mentioned in the main text.>

We'll change the text as follows: "A weak pressure system and an ABF located further south might have resulted in more frequently occurring Benguela Niño-conditions before 7.8 Ma. The meridional temperature gradient would have steepened afterwards inducing a northward migration of the ABF and more influence of the Angola Current in the region. This resulted in the occurrence of L. machaerophorum, a species blooming in warm stratified nutrient-rich waters after upwelling relaxation. L. machaerophorum disappeared from the Benguela upwelling system after 6.5 Ma."

<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.>

We'll add "probably"

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

Yes, we'll delete the last sentence from the conclusions.

1931/4: Matthiessen

thanks

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

As soon as the paper is accepted.

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

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done

<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), Trinovantedinum ferugnomatum, Sumatradinium soucouyantiae>

Many thanks for checking and spotting the typing errors. However, as mentioned above, we'll keep the grouping to explain the H/A ratios used in Figure 4.

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

We'll rephrase as follows: "Changes in surface wind in m/s and sea surface temperature (SST) in °C calculated from the difference between two runs oft he CCSM3 model one configured with present-day mountain elevation (full) and the other with half present-day elevation (full minus half)"

<Figure 2, 3. Can Figure 2 and 3 not be combined into one?>

To combine the figures would result in one that is too long for a page. We might ask to put them on the same page in the final version.

<Figure 4. What does the horizontal line represent in the Cyst accumulation rates log and Heterotrophs vs Autotrophs ln(H/A) graphs? Indeed it is not important and maybe misleading.>

The horizontal division is the mean.

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<Figure 4 caption L6: Angola Benguela Front [add t] L7: Roberts L10: Seaway>

Thanks again; obviously someone's spelling checker had been switched off.

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