

Interactive comment on “Temperate Oligocene surface ocean conditions offshore Cape Adare, Ross Sea, Antarctica” by Frida S. Hoem et al.

Anonymous Referee #1

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Dear editor, authors,

Hoem et al., submitted a manuscript to Climate of the Past Discussions titled Temperate Oligocene surface ocean conditions offshore Cape Adare, Ross Sea, Antarctica. In this manuscript, the authors present newly generated gdat, biostratigraphic, magnetostratigraphic and palynology data from DSDP Site 274, located offshore the Ross Sea continental margin, near the Antarctic continent. The records span the early Oligocene, are characterized by a relatively coarsely resolved bio-magneto age model, poorly preserved PMAG signals, relatively high reconstructed SSTs/temperate (sub)surface water conditions, and dinoflagellate cyst assemblies that vary in their composition throughout the three selected Oligocene intervals. These results are used for paleoclimatic, paleoceanographic and Antarctic cryosphere reconstructions. The text

C1

is well written and the figures look good.

Given what is possible with old DSDP material and the patchy nature of records proximal to the Antarctic ice sheet, the authors have done a thorough job. The strongest point of this study are the reconstructed SSTs. I must admit that I am not very impressed by the bio-magneto stratigraphic age control. Also, the presented component analysis leaves more than half the variance unexplained. Yet, despite these limitations the authors provide a very extensive interpretation. I feel that in some instances the authors stack speculation on top of speculation, which may lead to tunnel-vision to make their results fit those from other sites and/or the literature.

Despite these issues, I recommend publication, mainly because as a paleoclimate community we need to work with whatever natural archives are available. However, I would like to ask the authors to address/rebut the issues I raise here. I leave it to the editor to decide how far the authors should go in adapting the manuscript (I do not need to see the manuscript again).

Major comments:

1: One of my major concerns is that the authors make a point about improving the age model, but show their results in the depth domain. With such an elaborate paleoclimatic interpretation in the discussion, I believe that the authors should show their results in the age domain. This also includes providing some age uncertainties.

2: I strongly recommend to delete or very strongly tone down section 5.4. To me this reads as a step too far in interpreting SST and dino results on a sketchy age model. Speculation stacked on speculation. What do the authors think dinos are/are not a proxy for? Alternatively, if the authors insist on keeping this section, then formulate some testable hypotheses based on these rather speculative interpretations.

3: I do not think the orbital part of the story can be backed up by the data.

Minor comments (per line number/section):

C2

29, 30: “orbital states”. This is rather vague. The records do not resolve any orbits, not even close to any orbital frequency. These interpretations thus rely on the analysis of Levy et al., based on the record of Pålke et al., and the authors attempt at making a coherent story out of both their own data and these previous results. In principle, this is an admirable effort, but I wonder if their data the author’s present could also be interpreted to disagree with Levy et al., if an opposite phase relation to the orbits is found (if/when better age constraints become available)?

41, 42: “Oi-1”. I suggest the author’s follow Hutchinson et al. 2018, nomenclature for EO events. Oi-1 becomes EOIS.

47: “Oligocene”. give approximate ages. The Oligocene is not one uniform time slice.

53: “still constricted”. What is meant here. Surface connection only, no connection?

62: “colder temperatures”. Choose: either colder waters, or lower temperatures, but not colder temperatures. Just like, you are heavy and weigh much, but cannot weigh heavy. Fix throughout.

64: perhaps not call it a gradient with the Tasman straight in between. Call it a temperature difference between the two sites.

64: add “(sub)surface” before “ocean temperature”

71: “before”? Do you mean “in front of”?

74: replace “classic” by “more standard inorganic geochemical”?

75: “strong links”. This is the weak part of dino-based interpretations. Can you quantify these strong links? Those I find in this paper are rather weak! PCA-1 only explains 31%. A lot of interpretations and speculation are based on these strong links. I would tone this down.

79: any indication of the average age uncertainty/diachrony of bio events would be helpful here. How good are these constraints?

C3

80: “improve the age model”. Ok, but by how many depth-to-age tie-points was the age model improved. I just saw a paper come online by Jovane et al., 2020, who also improved the ages for Site 274. How do your new ages compare? Can you include their results and improve the age model further?

99: “diatom-rich”. Could these not be used for dating?

117: “early” instead of “lower”, because ages are given in brackets

121: Could rad or diatom stratigraphy help improve the age model further?

164: add...focus when interpreting the results. Section 3.3.2.: I would move this to/integrate in the discussion.

219: delete “in”, “general” should be “generally”

220-228: I would delete the PMAG results. They don’t add much to the story or age model. Alternatively, integrate the new results by Jovane et al., 2020, to make more of these age constraints. Somewhere between 229 and 248: Explain how many depth to gts2012 age tie-points are added compared to the previous shipboard age model.

289: Sentence starting with “Given that...” needs a citation.

291: Dinocyst sp. 1 could not have been reported previously, as it is defined in this study

293: “argues for in situ production” Why? Does ocean transport break down cysts?

299: absence of evidence is not evidence of absence. Does the high g-cyst abundance not argue for the opposite?

307: replace “temperature” with “gdgts”. This is still the results section. Leave the interpretations for the discussion.

Section 4.3.4. Potential delete. These results don’t add much, and are not really discussed.

C4

318: 46% is rather low in my opinion. Together with the rather poor age control, this is the main issue with this data set. No very clear signals.

324: given the low variance explained, I think “gives confidence” is a bit of an over statement. I understand that the authors attempt to sell their data, but I would stick with the facts. . . . The data isn’t very clear. And much more data is needed to test many of the hypotheses and interpretations of the authors.

331: by how many tie-points? With respect to what?

339: “. . . , a period. . .600 ppm”. This reads like a tag on to the rest of the sentence. Make the link to CO2 relevant/explicit.

342: Can the authors briefly explain/recap to non-dino folk what P-cysts and G-cysts are ‘proxies’ for in general/this setting/according to the literature?

350/351: I understand that G-cysts are proxies for upwelling and oligotrophic conditions. Ok. How well-established is this (perhaps give some modern-day r^2 values, PCA percentages between these cysts and processes)? Or refer to the literature that makes this interpretation obvious/standard/accepted.

359: to my reading the interpretation that the Eocene dinocysts are reworked is largely/only based on the interpreted, rather poorly resolved age model. Are there no alternative explanations possible? Could the age model be wrong and the Eocene in situ? If not, why not? Make explicit.

Section 5.2.2; What is the (average) sampling resolution in the age domain? Do the authors still feel confident to go orbital with their interpretations? To what level? 405 kyr eccentricity? 100 kyr? Or even 40 kyr obliquity and beyond? My feeling is that the data is not suitable for such astronomical interpretations later on in the manuscript, and would advice the authors to stick with comparing/contrasting Oligocene climate “states”. “Orbital states” is not a thing, because the system never equilibrates to the relatively short lasting/high frequency orbital configurations.

C5

369: start of new section. Remove “also”.

371/385: replace “proto. . .noid” with P-cyst. This has already been abbreviated.

386-389: surely gdgts are more easily reworked than dinocysts. Yet the authors argue the opposite.

390: “high temperatures”, not “warm temperatures”. Alternatively, “warm sub surface waters”.

392: can you provide an R^2 for this covariance?

399-400: remains rather arm-wavy. Can some of these relationships be quantified?

405-406. G-cysts are relatively more abundant because of a decrease in P-cysts. Make the effects of such a closed sum effect on interpreting your data explicit.

Section: 5.3: This reads like a review paper. Quite a lot of speculation based on limited data. I guess this is the nature of the game, but you might lose the attention of some readers when several levels of speculation are stacked. Perhaps be more cautious?

437: “ocean crustal connection”. Do the authors mean a deep-water connection?

462: “we can now exclude. . . .”. Remind me again, why that is?

465: “related to orbital cyclicity”. This is a very vague statement. What orbit? The data presented cannot support this.

471: Perhaps mention winnowing as a reason too?

480: Regarding the point about heightened obliquity sensitivity. This is solely based on the Levy interpretation of the partially obliquity tuned Päläike data. I understand that this fits your interpretation and may give context to understanding the results from Site 274, but the newly presented data cannot confirm or refute or support the Levy hypothesis. I would make this point.

487: Could winnowing have removed diatoms and dinos?

C6

529: “precession driven top down melting”. Pls remove. There is no data presented to support this statement.

533: When? During the entire Oligocene?

538: “gradient”. The authors argue that a gradient does not make much sense with a Tasmanian gateway in between these sites.

541: Sentence starting with “During cold phases, ...” I can see how this argument works for heat, but for moist you’d expect the opposite. Again, I would refrain from using dinos to interpret cryospheric conditions.

I wish to congratulate the authors with a well written and nicely illustrated paper.

References:

Hutchinson et al., The Eocene-Oligocene transition: a review of marine and terrestrial proxy data, models and model-data comparisons, *Climate of the Past*, 2020, <https://cp.copernicus.org/preprints/cp-2020-68/>

Jovane et al., Magnetostratigraphic Chronology of a Cenozoic Sequence From DSDP Site 274, Ross Sea, Antarctica, *Frontiers in Earth Science*, 2020, https://www.researchgate.net/profile/Luigi_Jovane/publication/345947592_Magnetostratigraphic_Chronology_of_a_Cenozoic_Sequence_From_DSDP_Site_274_Ross_Sea_Antarctica/links/5fd787d7a6fdccdc8c58a88/Magnetostratigraphic-Chronology-of-a-Cenozoic-Sequence-From-DSDP-Site-274-Ross-Sea-Antarctica.pdf

Interactive comment on *Clim. Past Discuss.*, <https://doi.org/10.5194/cp-2020-139>, 2020.