

Answer to Reviewer 1 for the interactive comment on “Late Oligocene obliquity-paced contourite sedimentation in the Wilkes Land margin of East Antarctica: implications for paleoceanographic and ice sheet configurations” by A. Salabarnada et al.

We apologize for the late response but I have been embarked on a research cruise in Antarctica. We would like to thank anonymous Reviewer 1 for his/her comments and constructive suggestions, which will help to improve the manuscript. Below are our answers to the comments in black ink and italic.

Does the manuscript represent a substantial contribution to scientific progress within the scope of Climate of the Past (substantial new concepts, ideas, methods, or data)?

Good

Scientific quality:

Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)?

Good

Presentation quality:

Are the scientific results and conclusions presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of English language)?

Good/Excellent

Does the paper address relevant scientific questions within the scope of CP?

Yes it does.

Does the paper present novel concepts, ideas, tools, or data?

Yes.

Are substantial conclusions reached?

Yes. Though, partially due to the nature of the data/research, many conclusions remain largely speculative.

Are the scientific methods and assumptions valid and clearly outlined?

Partially. I think that such a wide variety of data is presented, that integrating all lines of evidence is very complex. I think that the authors can improve on this point. Especially, by better outlining/introducing their approach (why each data set is presented and what it shows) and in their summary/conclusions (How the argument (largely sedimentological in nature) is constructed). The paleoclimatic and paleoceanographic conclusions are speculative.

*The high recovery of late Oligocene sediments during Expedition 318 provided an unique opportunity to study the environmental conditions at this site that is close to the Antarctic margin. No single indicator provides a clear picture of these past high-CO<sub>2</sub> world environments but the mutiproxy approach used here helps in testing out some of the environmental signals. The conclusions reached are by the nature of this*

*study speculative since they are reached with data from a single site. However, the paleoclimatic and paleoceanographic conclusions are not so speculative as they may appear when we take into account that the paleoclimatic conditions are supported by the Sea Surface paleotemperatures reported by the companion paper submitted to *Climate of the Past* by Hartman et al.; and the paleoceanographic conditions by the paper by Bijl et al. It is unfortunate that the reviewers did not have access to these other two papers.*

Are the results sufficient to support the interpretations and conclusions?

Yes, I think so. However this research comes with large limitations of course.

Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?

Yes.

Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

Yes.

Does the title clearly reflect the contents of the paper?

Yes. I think so. Though perhaps be more careful with the orbital interpretations. Good age control in these sediments is difficult to achieve. Perhaps replace “obliquity” with “astronomical”? Given the moderate recovery (many gaps), 1 million year length of the record, and relatively poor absolute&relative age control, I wonder if the generalization of the presumed obliquity pacing for the (entire?) Late Oligocene (as the title could suggest) is too much.

*We concur with the comment by the reviewer and will substitute obliquity with astronomical in title. We also changed “implications” with “insights”.*

Also, I wonder if contourite is the correct sedimentological description of these sediments. I realise that this argument is explored in great detail in this manuscript, however I am no sedimentologist and I wonder how these contourites compare to those from, for example, the Iberian margin. Levy et al. PNAS 2016 present 5 motives for a very proximal site. Could the lithological alterations at Wilkes Land not be linked to these motives as well? And are we still speaking of contourites then?

*We appreciate the candid comment of the Reviewer indicating that he is not a sedimentologist. Contourites in any setting and location refer to sediments deposited or significantly affected by the action of bottom currents, despite their origin. In the Wilkes Land Site U1356, the sediments deposited during glacial and interglacial cycles, which are dominantly gravity flows and hemipelagites, respectively, are reworked by bottom currents resulting in the sediments recovered at this site. Contourites from the Iberian margin are also the result of reworking of downslope and hemipelagic sedimentation. Contrary to turbidite deposits, contourites do not exhibit a “type contourite facies association model or motif” but contourite facies/structures (i.e., laminated vs bioturbated, etc) are common to all bottom current deposits (see for example the review paper by Rebesco et al., 2014). Levy et al., PNAS 2016, shows a stacking patterns of different motifs recovered from the McMurdo Sound coastal sector of the Ross Sea by the ANDRILL2A. Levy et al.,*

*interpret the sedimentary cycles represented by the motifs in terms of advances and retreats of the ice sheet grounding line forced by eccentricity. Therefore, the motifs in the Levy et al paper result from sedimentary processes associated with the **direct influence of grounding line advances and retreats in a coastal setting**. Our record is a **distal marine record**. Therefore, our site receives sediment input from the continent (which provides an indirect record for continental glaciation) and the rain of hemipelagic materials that are then reworked by ocean currents. In both Levy's et al. paper and ours, we interpret the alternation in motifs and facies to be astronomically forced.*

Does the abstract provide a concise and complete summary?

Improvements can be made. Please see below.

Is the overall presentation well structured and clear?

In general it is a very long paper with many (complex) lines of evidence. I feel that this could be outlined (signposted throughout the manuscript) a bit better. Perhaps introduce when new datasets are presented and why these data are important for this study. What questions will they help answering?

*We understand the multiproxy approach used in this study can make it hard to follow the different lines of evidence. At present, each of the indicators used for this study and their relevance is explained in the Material and Methods section. However, to address this concern of Reviewer 1, we will introduce a brief outline of the relevance of the indicators used in each of the subsections in the Results.*

Is the language fluent and precise?

Yes.

Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes.

Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

I think that making the manuscript more concise/focussed would help with getting the main points across.

*We will work to make the revised version of this manuscript more concise.*

Are the number and quality of references appropriate?

Yes. Perhaps add Levy et al. 2016 PNAS.

*In our paper, we have established comparisons with the environmental setting between the Wilkes Land and the Ross Sea. We have focussed on coeval records to those we are studying, both coastal (CPR, Barrett, 2007) and distal (DSDP Site 270, Kemp and Barrett, 1975) sites. We will introduce the reference to Levy et al 2016 in the "4.2 Ice sheet configuration during the warm late Oligocene" chapter, in line 553, by adding "Also, a dynamic ice sheet is described for the early Miocene coastal section of AND-2A with glacial and interglacial advances and retreats of the EAIS (Levy et al., 2016), that could have a similar paleotopographic configuration to that*

*for the Oligocene.”*

Is the amount and quality of supplementary material appropriate?

I have not been able to find the supplementary data online. I have not reviewed this.

*It is unfortunate that it appears that the Reviewer did not have access to the Supplementary material when these were available online as they were submitted with the manuscript.*

Further comments:

L43: I think that the link between the data presented in this paper and ice sheet configuration is speculative at best. I would not start the abstract with such a bold claim. Delete, or move to the final line of the abstract and say something like: “we speculate on the ice sheet configurations of the Wilkes Land Basin from between 25 and 26 million years ago.

*We will proceed in the revision as advised by the Reviewer. However, our claim on the retreated ice sheet is reinforced by several lines of evidence: (1) How the late Oligocene interval studies compares to the rest of the Oligocene and early Miocene sediments (presented in the supplementary materials to which the Reviewer unfortunately did not have access). Earlier Oligocene and Miocene sections contain Ice Rafted Debris, suggesting an extended ice sheet. No IRD was found in the studied interval and we argue this could be indicative of less extensive ice sheets. (2) As referred in the paper and more extensively covered in the companion paper to this one by Bijl et al., dyncocists indicate that during the studied interval there is no evidence for sea ice suggesting a warmer setting and reduced ice sheets during the studied late Oligocene interval. Sea ice species are however present in other Oligocene and Miocene intervals from U1356 core. (3) High Sea Surface Temperature reconstructions as shown in the companion paper to this one by Hartmann et al. that support the sea ice free scenario. (4) Reconstructions derived from fossil pollen in Site U1356 suggesting high terrestrial temperatures (Salzmann et al., 2016; Strother et al, in prep).*

L46: Physical properties are only magnetic susceptibility. I would just say that. I would also be more precise about what geochemical techniques are presented. Key paleoceanographic/ice sheet indicators, such as fish tooth and detrital Nd are not presented. Make that clear in the abstract.

*We will follow the suggestion by the Reviewer. For the physical properties however, in addition to the magnetic susceptibility, we also use density.*

L51-54: Not a sentence. I would first present a short summary of the sedimentological result. Then say how these are interpreted. Best not to mix these up.

*We will follow the advise of the Reviewer*

L58: Why lowlands? Why not topographic highs? Could your data not support both options?

*We see the confusion caused by the way the sentence is written. Of course the ice caps and glaciers occupied as well topographic highs. We wanted to mainly emphasize the different topography of the Wilkes subglacial Basin compared to today, which in the Oligocene was not yet over-deepened. We will try to clarify this by rephrasing the sentence to indicate "These observations, supported by elevated sea surface paleotemperatures and the absence of sea-ice, suggest that between 26 and 25 Ma open water conditions prevailed and therefore glaciers or ice caps occupied the topographic highs and lowlands of the now over-deepened Wilkes Land subglacial Basin."*

L64-65: The line about spectral analysis is stuck on the end of the abstract. A strange place to present new results/interpretations. I would advise to end the abstract with the biggest (although perhaps speculative) conclusions. Not new information about the sedimentological/statistical description.

*We will rewrite following the advice of the Reviewer.*

L137: Just say magnetic susceptibility of the bulk sediment.

*We will rewrite as advised.*

L184: Cite individual chapters of the Gradstein volume. In this case Vandenberghe et al. (the Paleogene chapter).

*We will cite as advised.*

L191: I have not been able to find supplementary information online. Did I miss anything?

*It is unfortunate that the supplementary materials were not found since they are online and were submitted at the same time as the rest of the manuscript. The Supplementary information provides more detail regarding the spectral analysis applied to our datasets and also explains in more detail the sedimentary section from the early Oligocene to the early Miocene.*

L205: Which lab was used for this analysis?

*CT-scans were done at the Kochi Core Center (KCC) lab (Japan). It was already stated in the text (L201) but we clarified it.*

L235: Al counts are often very sensitive to coring disturbances. I think this should be mentioned and that the authors should be careful with the interpretation of Al counts from heavily disturbed sediments.

*We agree with the reviewer. Although we don't have core disturbances all along our studied section, we detected that Al and Si elements collected by the continuous X-Ray Fluorescence (XRF) scanner present more than one order of magnitude gains although the sediments were not deformed, and therefore they were not used. To overcome this problem, XRF analyses in discrete samples from non-deformed intervals were also conducted and are the ones used in our research. We clarified in*

*the text that the interval for which we collected XRF data did not show core disturbances (L233).*

L256: Crucial point. How was the data anchored (tuned) to obliquity? This point needs to be described and explored in much more detail. What assumptions are underlying the tuning? The readers need to know how certain the authors are about the age model/tuning etc. What is the room for improvement?

*The information requested by the reviewer is contained in the supplementary materials to which, unfortunately, the reviewer did not get access. To avoid further confusions, we also will add a sentence in the main text of the manuscript to provide information about anchoring the time series. For the main research we used the Evolutive Average Spectral Misfit method (Meyers, 2014) for the astrochronologic testing, that was evaluated using ETP (eccentricity, obliquity and precession) target from La04 (Laskar et al., 2004). Afterwards, an astronomical tuning is done by using the Frequency domain minimal tuning (Meyers et al., 2014) where spatial frequencies are afterwards converted to sedimentation rates using the average period of 41 Kyr/obliquity. Time series is afterwards anchored to our paleomagnetic tie points. We added a sentence in methods section and also in the result section. In the supplementary data, there is also another tuning done for initial evaluation of the time series, where we tested with Analyseries method (Paillard et al., 1996) by filtering our data in depth scale and comparing it to the Obliquity solution of La04 (Laskar et al., 2004).*

L256 and L260/261 mention two different tuning targets. One based on obliquity, the other on eccentricity, obliquity and precession. Please clarify.

*Related and answered with the previous comment.*

L270: Please clarify how your sedimentological descriptions are better than the shipboard description. How did you improve?

*Shipboard, sedimentologists describe the sections as cores are opened. Shipboard descriptions, although thorough, are preliminary since there is no time to look at the cores in the detail and the context is often lost because of changing work shifts and describers. Shipboard descriptions interpreted deposition during the studies interval to be dominated by hemipelagic and turbidity flows/bottom current processes. Post-cruise, we had a chance to re-describe all core sections in detail and by the same group of people, which included experts in turbidite and contourite deposits (not always easy to differentiate). This resulted in the very detailed lithological log presented in this paper. In addition, the integration of the detailed lithological log with magnetic susceptibility (collected shipboard), continuous/discrete XRF data, high-resolution images and CT-Scans and SEM images (obtained in the frame of this study), allowed us to further characterise the facies.*

L435: Perhaps compare to Levy et al?

*In this part of the discussion we focus our comparisons to facies from different settings that are similar to site U1356, mainly around East Antarctic margin. AND-2 from Levy et al., obtained sediments from a coastal site.*

L520: Could there be other reasons why there is no IRD at your site? (Absence of evidence is not necessarily evidence of absence)

*We agree with the reviewer that the absence of IRD cannot be directly linked to a retreated ice sheet. However, as mentioned earlier, our interpretations regarding the lack of an extended ice-sheet similar to the one existing in the earlier Oligocene is not only based on the absence of IRD's but in several lines of evidence, as mentioned before, which include: (1) How the late Oligocene interval studies compares to the rest of the Oligocene and early Miocene sections (presented in the supplementary materials to which the Reviewer unfortunately did not have access). Earlier Oligocene and Miocene sections contain Ice Rafted Debris, suggesting an extended ice sheet. No IRD was found in the studied interval and we argue this could be indicative of less extensive ice sheets. (2) No evidence for sea ice indicated by dinocists and reported in detail in the companion paper to this one by Bijl et al., suggesting a warmer setting and reduced ice sheets. Sea ice species are however present in other Oligocene and Miocene intervals. (3) High Sea Surface Temperature reconstructions reported in the companion paper to this one by Hartmann et al. that support the sea ice free scenario. (4) Reconstructions derived from fossil pollen in Site U1356 suggesting high terrestrial temperatures (Salzmann et al., 2016; Strother et al, in prep). In addition, we compare the environmental setting during the studied late Oligocene interval to iceberg modelling studies conducted in Pliocene sediments from the Wilkes Land margin by Cook et al (2014). The modelling shows that despite the high sea surface temperatures during warmer climate periods of the Pliocene, iceberg armadas were able to travel as far as to the continental rise sites in this margin.*

L580: I do not understand how the authors conclude that ice was present in the lowlands. Are topographic highs not a much more likely location of land ice?

*We agree with the Reviewer. ice sheets and/or glaciers would occupy both high- and lowlands. This agrees with the pollen assemblages in sediments from this interval (Ulrich Salzmann personal communication). Although what we wanted to note is that ice would be occupying the non-overdeepened Wilkes subglacial basin. We will rephrase this in the revised version to make sure it is clear.*

L603: do the authors mean that the palynomorphs are partially oxidized/poorly preserved? Please clarify if that is the case.

*We will address the text in order to make clear that Palynomorphs have good preservation, and that don't show notable changes in their preservation between F1 and F2 (companion paper to this by Bijl et al.,).*

L681: What is the evidence that northern component waters were reaching this site that is located so far south in the modern and in the Oligocene? The evidence for NCW in the Oligocene needs to be better explained/this point needs to be presented/supported in a much better way.

*We will improve our discussion regarding this point in the text. We consider that as Circumpolar Deep Water is a mixture of AABW and also the NADW and the northern*

*component waters (NCW), we interpret that during warmer times, and also due to the influence of the shifted Polar Fronts to the South during interglacials, NCW would have a higher influence on the proto-CDW, and thus, shifting the chemical characteristics towards a carbonate friendly environment. The presence of preserved coccoliths in such southernmost positions in Antarctica, and in the continental rise is rare, as many studies correlate coccoliths with the presence of a carbonated and warmer water mass shifting south, being the NCW or the NADW in the actual configuration of the ocean.*

*We rephrased our manuscript in order to make clearer the argument as follows: "Circumpolar Deep Water (CDW) is a mixing of abyssal, deep, and intermediate water masses, that includes AABW and NADW nowadays (Johnson, 2008). During warmer interglacials, the influence of more northern-sourced water masses into the proto-CDW, relative to Antarctic-sourced, could enable carbonate productivity as seen in the interglacial facies with coccolithosphere remains (Fig. 7c)."*

**L689: Noise and gaps in time series are two different things. Please correct.**

*This will be corrected on the revised version of the manuscript.*

**L697: Why would precession suggest a dynamic ice sheet? Are there other mechanisms that could be thought of to explain a potential precession beat in your data?**

*We agree with the Reviewer that precession can have different interpretations in our record. Although highly speculative, as our record captures the precession frequencies, we suggested that high latitude summer insolation during late Oligocene had an influence on the continental terrigenous fraction suggesting ice melt and rapid ice-sheet volume changes as Patterson et al., (2014) also suggested for core U1361 in Wilkes Land during the Pliocene. However, given that this interpretation does not add to any of the relevant point of the manuscript and is highly speculative, we will remove it.*

**L711: More caution needs to be taken when interpreting tuned records. Many assumptions are implicit.**

*We agree on the reviewer, but we consider that, although low-resolution, we have the best age model possible for the late Oligocene for Site U1356 at this time. The age model proposed by Tauxe et al., (2012) for this time period has two solid chrons (Chron C8n.1n (o), 25.260 Ma, at 643.37 mbsf; and C8n.2n (o), 25.900 Ma, at 678.98 mbsf). With the tuning we maintained the bounding chrons, which correlate very good with the tuned time series using two different models (see Supp. Materials). We therefore consider that, although low-resolution, it is not incorrect to make assumptions with the age model and correlate the events with global events in a large timescale.*

**L744: Nd evidence is needed before this can be suggested with any level of confidence. This is just speculation in my opinion. Please rephrase.**

*We agree with the reviewer that Nd isotopes are a good evidence of distinct water masses. However, no fish teeth were recovered from this interval to conduct these*



*studies. The chemistry of the water mass influences the elemental concentrations and also can give paleoceanographic information. For example, bottom waters chemistry affects the preservation of carbonates in sediments. Here, we postulate that the presence of nannofossils in site U1356 is enhanced due to more carbonated and warmer waters, less corrosive to carbonate, as are the warmer north component waters (NADW-like), that are entrained and mixed within the circumpolar deep waters (proto-CDW), that bath the basins of Antarctica.*

L749: Add “in the Wilkes Land Basin”

*Corrected.*

L753: how is this conclusion supported by the data? No ice volume estimates are presented.

*We agree with the reviewer. We left that there is a retreat of the ice sheet in the Wilkes Land Basin but we took out the processes that control the melting of the ice sheet.*

Despite my (hopefully) constructive criticism, I am very supportive of this paper. I hope to see it published soon in *Climate of the Past* and wish to congratulate the authors on a very nice study.

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