Excellent (1)
Good (2)
Fair (3)
Poor (4)

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 sedimentologal in nature) is constructed). The paleoclimatic and paleoceanographic conclusions are speculative.

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. 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?

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?

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.

Are the number and quality of references appropriate?

Yes. Perhaps add Levy et al. 2016 PNAS.

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.

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.

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.

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.

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

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 advice to end the abstract with the biggest (although perhaps speculative) conclusions. Not new information about the sedimentological/statistical description.

L137: Just say magnetic susceptibility of the bulk sediment.

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

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

L205: Which lab was used for this analysis?

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.

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?

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

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

L435: Perhaps compare to Levy et al?

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

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?

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

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.

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

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?

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

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

L749: Add “in the Wilkes Land Basin”

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

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.