Dear Editor,

we kindly thank reviewer #2 for their constructive feedback on our paper. Below we respond to all comments and indicate how we intend to adapt our paper based on them.

Sincerely, also on behalf of my co-authors,

Dominique Jenny

Jenny et al present a review of the Oligocene, an 11-million-year long time interval. The review is concerned with:

(1) a general introduction,

(2) Chronostratigraphy and event nomenclature,

(2.1) Planktonic foraminifera zones,

(2.2) Calcareous nannofossil zones,

(2.3) Radiolarian biostratigraphic zones,

(2.4) Dinoflagellate cyst zones,

(3) Boundary conditions for Oligocene climate,

(3.1) Geographical boundary Conditions,

(3.2) Ocean circulation,

(3.4) Carbon cycle,

(4) Climate Proxy data,

(4.1) Temperature,

(4.1.1) Continental Mean Annual Temperature,

(4.1.2) Sea Surface Temperature,

(4.2) Precipitation,

(4.3) Data-model comparison,

(4.3.1) Temperature proxy to model comparison,

(4.3.2) Precipitation proxy to model comparison,

(4.4) Ice sheets,

(5) Flora and Faunal changes,

(6) Discussion,

(Note, 6.1 is missing).

Reply: Numbering will be corrected

(6.2) Temperature trends and variability,

(6.3) Precipitation, and

(7) Conclusions.

This review constitutes a very ambitious, admirable, and important project, that necessarily needs to make broad-brush summaries and interpretations about the main climatic and oceanographic factors that determined the Oligocene palaeoclimate system. In principle, I am sympathetic to this effort. The paper is well written.

However, I believe that for this paper to become an authoritative reference paper much more work is needed. I am no expert on all topics reviewed here. But on topics on which I do consider myself reasonably knowledgeable I find that they are often marked by too many inaccuracies, some outdated discussion points, and several key omissions. By proxy, I have therefore little faith that the parts of the review concerned with topics on which I am no expert, accurately capture the current state of knowledge. I am left with the impression that this paper 'could have been so much better'.

To me the paper suffers from not having included enough Oligocene workers, many of whom have dedicated their professional lives to understanding topics ranging from age models, marine microfossils (calcareous nannofossils, foraminifera, radiolaria, etc.), macrofossils (animals and plants), ice sheet modelling, palaeogeography, etc. Compare, for example, this Oligocene review to Steinthorsdottir et al., 2021, who consulted 3 to 4 times as many experts for their Miocene review. (Here my criticism is mainly directed at coauthors Bijl, Huber, and Sluijs; all experienced and senior workers, some of whom have been involved in the Steinthorsdottir effort. To me, and as far as I can judge, it seems that with greater input from these authors, also in study design, the manuscript could have been lifted to a higher level already. Jenny's efforts in leading this study, as a PhD student, is admirable to say the least.)

I believe that revising this manuscript will result in a much better paper. To me the authors have two options: Option 1) shorten the manuscript considerably by removing much of the review text that is not pertinent to the temperature/precipitation compilation and model comparison, i.e. what I consider the main strength of this paper, and expertise of the authors, or Option 2) include more expert knowledge, work with the text and develop the discussion to lift this paper to a higher level. I recommend the second option, and I believe it could make, after substantial revisions, for an authoritative Oligocene reference paper. Ultimately, it is the author's choice.

I will provide some below and wish the authors much success with revising this document and planning the next steps ahead. I am sorry that I am not more positive in this instance.

Reply: We agree and choose for the reviewer's Option 1. We will be shortening the manuscript to the sections that are only important to understanding the newly presented compilation of temperature and precipitation data, and data-modelling comparison and only retain the background information that is important for their context. Thus Chapter 2, large parts of Chapter 3, section 4.4 and Chapter 5 will be removed. Relevant information will be included in the introduction or discussion chapters. This makes a much more concise paper, focused on the main issue identified in the reformulated (also considering the comments by Reviewer #1) introduction.

Major comments

1. Structure of the paper: I believe that inclusion of subheadings concerned with magnetostratigraphy, astrochronology, oxygen and carbon isotope stratigraphy would strengthen the paper. Perhaps compare to Steinthorsdottir et al., 2021, to see how they went about their Miocene review. If this comparison is unjustified, please say so in the rebuttal.

Reply: Sections 2.1-2.4 will be removed from the paper.

2. Doubthouse/Unipolar Doubthouse: Most workers use "Doubthouse" to describe the latest Eocene (between the MECO and EOT), where the climate system was cooling and "doubting" to jump from a "Greenhouse" state to a "Unipolar Icehouse" state. I much prefer to reserve the phrase "Doubthouse" for the Late Eocene, and the usage of "Unipolar Icehouse" to describe the state of the Oligocene to Pliocene time interval. As an aside, I also prefer "Unipolar Icehouse" over the phrase "Coolhouse". The usage of "Unipolar Doubthouse" in your title is a mixture of "Unipolar Icehouse" and "Doubthouse" and is confusing to me. I am not entirely sure what is meant by this phrase. Is the system "doubting" to be unipolarly glaciated? Or "doubting" to go back to a "Greenhouse" state, or to a "Bipolar Icehouse" state?

Reply: We will change the title of the paper to: "We will change the title to: "Climate variability, heat distribution and hydrology in the unipolar icehouse of the Oligocene"; a review and data-model comparison" thereby also incorporating a comment by Reviewer #1.

3. Figure 1: This figure has many inaccuracies. Almost all boundaries or events (apart from those around the EOT, which seem to be OK) are indicted in the wrong place. Furthermore, the MOG is not a previously defined event, the MOGI is. The MOGI as drawn does not cover the MOGI as defined in the literature. The OMT does not cover the largest glaciation across the OMB. The Mi-1 points to an d18O low, not a d18O high, and moreover it is best to abandon Mi and Oi nomenclature completely (more about this below).

Reply: We thank the reviewer for spotting this; most likely a last-minute mishit in illustrator. All boundaries will be checked and moved where necessary, and we agree to abandon Mi and Oi points terminology.

4. Line 108/109, the Oligocene time scale: For an Oligocene review paper, it is important to get the age scale right, and to describe it explicitly (not just mentioning it in a figure caption). The absolute ages of the polarity time scale in the GTS2012 (i.e., the GPTS2012) are of greater quality than those incorporated in the GTS2020/GPTS2020, especially for the latest Oligocene where the GTS2020 incorporated erroneous ages. Admittedly, large efforts have been put toward improving biostratigraphic markers and zonations for the GTS2020, but also their absolute ages are unfortunately too often based on an inferior GPTS2020 scale (compared to the GPTS2012). I recommend using GPTS2012 to any Oligocene (age model) worker, and an Oligocene review paper needs to address the errors in the G(P)TS2020, if the authors decide to stick with this age scale as their preferred option.

Reply: For our contribution regarding large-scale Oligocene trends and data-model comparisons of larger time intervals, the choice of time scale is relatively unimportant. We have therefore chosen to retain the 2020 time scale. We will include this information in the methods section and in the data file so that this can always be updated when an improved time scale is available.

5. Line 353 to 380: This CO2 discussion needs more work. What are the implications for radiative forcing? For example, how to explain the Late Oligocene Warming whilst CO2 is decreasing? This is a major topic in Oligocene research, and it is not mentioned or discussed. For this manuscript to become an authoritative review, such a major climatic enigma needs to be addressed, because it will help with formulating new and important research questions.

Reply: This is a good point. We will include a section 6.3 on outstanding issues, including the presumed late Oligocene warming and its potential discrepancy with the CO2 reconstructions. Additionally, we will discuss it in the light of the recently published CO2 record by the Cenozoic CO2 Proxy Integration Project (CenCO2PIP) Consortium.

6. Discussion and Conclusions, both seem quite superficial to me. Can we take a step back, and distil major research questions for the Oligocene?

Reply: The discussion will be reworked to be more pinpointed at the questions posed in the introduction. The conclusion will be reformatted and focused accordingly.

Minor comments

- Line 13: Perhaps start the abstract with why the Oligocene is of any interest to anybody? It may help draw some more readers in.

Reply: We will change the first sentences of the abstract so it becomes more clear what the interest of the Oligocene is.

- Line 14: Not entirely sure why this would lead from the first line of the abstract. To me the gradients could be a good analogue. Ice sheet geometry, palaeogeography, etc, seem to me the limiting factors in the Oligocene being a palaeo analogy for long term future climate states.

Reply: We will rephrase these sentences, in tandem with the revisions made following the previous comment

- Line 23: proxy data instead of proxy based data?

Reply: This will be changed to proxy data

- Line 25: "In line with previous…" Does this refer to Oligocene modelling efforts? Or modelling of warmer climates in the general. I thought that this was a feature of Eocene modelling output but was unaware of this being a problem in general, also for the Oligocene unipolar icehouse. Perhaps clarify?

Reply: We will clarify this sentence that we indeed mean previous proxy-model comparisons.

- Line 23 to 27: should we learn a lesson from these observations? Is it worth making that lesson explicit in the abstract (e.g., much more effort needs to be invested in improving Oligocene climate simulations?)

Reply: We will add a final conclusional sentence to the abstract about the model simulations.

- 1: Position Mi-1 is wrong. In general, it is best to refrain from using Oi and Mi numbering. Oi and Mi numbers were originally defined (in very low-resolution records from the 1980s and early 1990s) as oxygen isotope zones, like biozones, that lasted several hundreds of thousands to millions of years (see original Miller papers and Wright papers). Over the years, these zone numbers were used to refer to the oxygen isotope "highs" (often called "events") at the base of the original oxygen isotope zones, yet this is not how they were originally defined. The arrows in Fig 1, suggest that the authors also use the Oi and Mi numbers to indicate events. If this usage is adopted, which I do not recommend, then please refer to high oxygen isotope values. E.g., the position of the Mi-1 is not correct in this figure (you refer to an oxygen isotope low). Furthermore, even when the Mi-1 (short for "Miocene oxygen isotope zone 1") is indicated correctly in Fig. 1, it would still fall in the latest Oligocene (yet another reason to abandon Oi and Mi numbers). Lastly, nobody has even been able to explain to me what makes the Oi1b, Oi2a, and Oi2b stand out as particularly interesting, or exceptional "events", probably because these numbers were devised as zones originally.

Reply: We will remove the Mi and Oi arrows/ events from the figures and not use those in the figure in general.

- 1: MOGI instead of MOG? Also, this band is not drawn according to the original definition. You may choose to redefine the main intervals of interest, but then they need to be defined in the text.

Reply: We do define the MOGI in section 2 but only with the Chrons, hence we will add the actual ages of those Chrons to the text. We will also rename the MOG in the figure one to MOGI and make sure that in the text it will always be called MOGI.

- Line 90: Oligoene (missing c)

Reply: The typo will be corrected.

- Line 90: MOG, where G stands for "glacial". In MOGI, this stands for Glacial Interval, and refers to a longer period of mean elevated d18O values (punctuated by shorter lasting d18O decreases of about 1 per mil amplitude, interpreted to be Antarctic "interglacials" and/or deep-water warming events). The use of glacial in MOG is a bit strange and makes me think of something more similar to the last Pleistocene glacial cycle.

Reply: We agree and as mentioned with the previous comment, will use MOGI from now on in the paper.

- Line 99: See Major Comment #1

Reply: We will add subheaders within section 2, to make the section more structured for the reader. Also figure 2 was removed together with sections 2.1-2.4

- Line 109: I strongly recommend using G(P)TS2012 over G(P)TS2020. See Major Comment #3.

Reply: We replied to this comment at the above Major Comment #3.

- Line 115: Beddow et al., 2018 did not work on the OM Boundary type section (Lemme Carrosio) but derived an astronomically calibrated age for the base of C6Cn.2n from Pacific sediments (perhaps make that clearer, because the current sentence can be interpreted as if Beddow et al did work on that section). Also, the 100 kyr error is not mentioned by Beddow et al. and seems too large.

Reply: This will be corrected.

- Line 110 to 125: This section would be much strengthened with the addition of a description of astrochronozones and potentially astro-unit-stratotypes. GSSPs are useful to an extent, but have their limitations (see e.g., Hilgen et al., 2020, Newsl. On stratigraphy). Potentially add a magnetostratigraphic description as well

Reply: Based on above Major Comment 0 and the advice of the reviewer, this section will be removed

- Lines 126 to 145: Overall a good summary, however, as stated above, I would shy away from reiterating Oi and Mi numbers. EOT, MOGI, and OMT are more useful terms in my opinion. Potentially in addition with the Late Oligocene Warming (LOW). MOGI definition used here, instead of MOG in figure caption. Please note that the MOGI is defined as “a generally cold but highly unstable mid-Oligocene time interval (~0 My to 26.3 My ago), which we refer to as the Mid-Oligocene Glacial Interval (MOGI)”, not as “a ~1 Ma year phase of profound cooling/glacial expansion”. Usage of Ma is erroneous in this context too (Ma refers to an age not a duration). In general, this section needs some work/detailed checking, as I have not checked all statements, and I may have overlooked other potential errors.

Reply: The figure text will be adjusted as mentioned above. In this section the definition of the Mi/Oi will be left in as they have often been used in the literature and thus need mentioning in the text, but we will also explain why the community is moving away from using them. We’ll include the description of MOGI as indicated by the reviewer and use ages from Liebrand et al. and of course correct Ma to Myr.

- Section 2.1. The GTS2020 is abandoned here?

Reply: This section will be removed.

- Section 2.2. Similar comment. Agnini et al., 2014, cannot have used GTS2020. Please mention on which age scale these bioevents are given.

Reply: This section will be removed.

- Section 2.3. Same here.

Reply: This section will be removed.

- Section 2.4. and here. For Chapter 2, in general, best to refer to an accepted general age scale. I recommend G(P)TS2012. Where biostratigraphic workers have deviated from these scales, it is important to mention so, and to estimate how large uncertainties/discrepancies are between scales.

Reply: This section will be removed.

- Line 329 to 352: This discussion is only concerned with the EOT interval. What about the 400 kyr, and 2.4 Myr cycle that dominate the Oligocene benthic d13C records?

Reply: This section will be removed.

- Line 353 to 370. What about the drop in $CO_2$ at around 24 Ma, during a 1.2 Myr Obliquity cycle “node”. May this drop have preconditioned the system for the (large amplitude of the) OMT.

Reply: this section will be thoroughly reconsidered given the proposed changes by the reviewer.

- Line 377: 16esults. Typo?

Reply: Yes, this will be corrected.

- Figure 5: OMT, MOGI, EOGM: please check ages.

Reply: All ages will be checked.

- Line 647 to 657: Very qualitative language. Warm, warmer, etc. Can we not put some numbers to how warm the Oligocene was?

Reply: This is difficult to do with the current availability of data. But we will replace qualitative with quantitative estimates where possible.

- Line 658 to 672: this section is mixing things up. It starts with a description of high resolution benthic d18O records and compilations. Then merges into a discussion of low-resolution SST data, with a reference to Fig. 5 as the only give-away that the topic has changed (admittedly still temperature, but now surface instead of deep ocean). It then comes back to comparing low-res SST (without mentioning that that is what is being compared) to benthic d18O.

Reply: We agree that this section needs some re-shaping. We will re-asses the section and focus it more clearly about the discrepancy of the late Oligocene warming recorded in the benthic foraminifera oxygen isotopes, which is a missing signal in the temperature records.

- Line 660: "most likely affected by a shift of geographical". As far as I am aware there is no doubt about it. Not sure if it is still meaningful to dig out this splicing issue in Zachos 2001. We are two good compilations further (Zachos 2008 and Westerhold 2020). Also confusing this with a true signal in both the Atlantic and Pacific benthic d18O of the Late Oligocene Warming is unhelpful. Is it not smarter to have a section in the paper that discusses benthic d18O. To me, the striking absence of a LOW signal in SST is something more interesting, but that is not discussed.

Reply: The Reference will be updated for this sentence. Additionally the sentence will be re-written in the reshaping of the section. The sentence about the geographical influence will be removed as it is not fitting with the purpose of the section and thus adds to the mix-up in the section as mentioned by the previous comment.

- Line 667: "obliquity". Not true. Both the 1218 and 1264 (both incorporated in Westerhold et al, 2020) are dominated by eccentricity in their spectral power. Low latitude forcing appears to be dominating global climate (in agreement with theory I would argue). How, power from precession is transferred to eccentricity is a topic of discussion, not included in this review.

Reply: We agree with this correction and will re-edit the variability section of the paragraph and also split it off from the long term trends so the paragraph becomes clearer.

- Line 668 -669: "Additionally, stratigraphic constraints on these records are insufficient to assess if this variability is consistent between sites." This is not true either. In the Westerhold et al., 2020 files, Site 1218 and 1264 are correct on the ~110 kyr time scale of tuning. Testing of orbital variability between sites is thus possible, at least on eccentricity time scales.

Reply: Same as above. This is a mistake in the text and will thus be adjusted in the new version.

- Line 669: are we still talking about benthic d18O?

Reply: This line will be clarified.