

# ***Interactive comment on “Data-constrained assessment of ocean circulation changes since the middle Miocene in an Earth system model” by Katherine A. Crichton et al.***

## **Anonymous Referee #1**

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General comments - overall quality of the discussion paper

In this paper the authors provides simulations for 7 time-slices from 15Ma to the Holocene using the intermediate complexity model cGENIE, which includes carbon and oxygen cycling. For each time-slice they run several simulations varying 2 parameters they consider to be uncertain/unknown – ie. equivalent CO<sub>2</sub> forcing and the magnitude of N. Pacific to N. Atlantic salinity flux adjustment. They then select the combination of parameters that enable the model results to best-fit the data (sea surface temperature from Alkenones and TEX86, benthic  $\delta$  18O and benthic  $\delta$  13C) for each time-slice. In doing so they try to explain the changes seen in the proxy record throughout the

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Miocene in term of greenhouse gases forcing and AMOC intensity changes.

This paper provide interesting methodology that can be use to track which process may be lacking in some models to be able to better simulate the climate of past warm periods. Also, we definitely need more 'systematic' methods to test model against data and I do think ensemble set of simulations is a good point to start with.

However I think that the authors should re-organize and re-write the results and discussion part so the main results from this paper are better highlighted. This would be very helpful to help the readers to better understand the purpose of the paper. I list some points below that in my opinion should be taken into account before publication.

Specific comments - individual scientific questions/issues

First, I am a bit concerned regarding some CAS-related statement in the text :

- Wide part of the Discussion focuses on the potential effect of the CAS configuration on circulation proxy, which thus seems to be (one of ?) the main point of the paper. As most of the discussion is CAS-oriented, authors should probably provide a few lines about CAS configuration changes during the Miocene and effect on the global circulation in the introduction using existing literature (e.g. Schneider and Schmittner, 2006 ; Butzin et al. 2011 ; Sepulchre et al. 2014 ...). From the actual content of the discussion and conclusion it seems that it should also appears somewhere in the abstract.

As authors refers a lot to CAS closure, they should also probably provide as supplementary information a table that contains at least the mean depth of CAS in the different settings they used.

- P17. L.525 “ We find that an early CAS restriction probably fits  $\delta^{13}\text{C}$  data better “ – It does not seems obvious, while reading the paper that this finding arise from analyzing author's model-data comparison. I also have the feeling that the CAS configurations used in the simulations does not enable to conclude such a thing, as CAS remain very

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deep in all the time slices (except for the Holocene). For the most recent cases this is much deeper than what we can find in the literature.

The paper would obviously benefit from additional sensitivity tests on CAS closure, especially for younger time slices (e.g. 2.5 and 4.5 Ma), as it seems, from what the authors states, that this is the missing point to be able to improve model-data fit. It would enable then to conclude whether or not CAS restriction better fits  $\delta^{13}\text{C}$  data for the youngest time-slices. The author should also in any case better discuss the relation between CAS configuration and  $\delta^{13}\text{C}$  in the light of results from previous studies.

- P16, l. 493 “The flux correction in the N. Atlantic that we apply may be seen as a combination of compensating for a more restricted (or closed) CAS” – The reason why is not very clear at that point of the text. I understand that stating that the authors refers to the fact that closing CAS increase AMOC intensity – or at least change the NADW properties in most models but then authors should refer to those papers and provide a little bit more explanation.

My second issue is with the Results and Discussion parts that are sometimes a bit messy and do not always reflect the purpose of the paper and the main conclusions. I thus suggest that they should be re-written to better emphasize the main results related to change in oceanic circulation, and discussing uncertainties/caveat of the study framework/applied methodology.

- As the title of the paper relates to circulation, I do think that it would be better to start the discussion with circulation-related findings. I feel like the CO<sub>2</sub>-related issues should come later, introducing caveats of the methodology and model or/and data related uncertainties.

- As the section 4.1 is written now, it looks more like a list of previous Miocene studies than a real discussion about which may be the reasons why the CO<sub>2</sub> forcing required in this study to improve model-data fit does not fit with proxy-based CO<sub>2</sub> estimates.

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- Also, methane (as well as gas hydrates) probably have a role to play but given all the issues associated with the absence of real atmospheric dynamics and vegetation feedbacks in the model, I am not sure it can by itself explain the discrepancy between the model and proxy-derived SST, while being forced with reasonable amount of CO<sub>2</sub> in the oldest-slices. So widely discussing it do not really help to understand the focus of the paper and the main results. Moreover, as suggested by the title, the discussion should probably focuses on oceanic circulation; however in the present configuration, about half of this section is dedicated to GhGs forcing. Slightly reducing the methane/gas hydrates part, that I think is not very useful in this paper should thus help making the content of the paper reflecting the title

- P15 – I would be helpful to change the title of section 4.2 to something like “Control on benthic temperature”.

- P15 – I do not think the title of paragraph 4.3 is meaningful and really relate to its content. From what I am reading it seems that the paragraph mostly relates to caveats of the model framework and uncertainties arising from modeling or unperfect interpretation of data.

- P26 – Paragraph 4.4 is entitled “North Atlantic Salinity and CAS” which is apparently not appropriate as the authors start by discussing orography changes during the Miocene.

It is not clear if the biological pump is represented in the model configuration used for this study (ie. Is the biological pump module activated ?). I think this should be written more clearly, probably in section 2.2, especially because as the authors stated, “the  $\delta^{13}\text{C}$  [...] represents the combined effects of ocean circulation [...] and the ocean biological carbon pump”. Anyway, I think that the authors should include a least a short discussion on the eventual changes in the biological pump during the Miocene and how taking it into account could help improving the  $\delta^{13}\text{C}$  model-data fit.

In this study, the orbital parameters are kept as modern. However, at the time the

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proxies were generated it is likely that the orbital configuration was different, which can explain also part of discrepancy between proxy and model. I would have liked to see this point being discussed or at least mentioned in the discussion.

Other comments :

- P2 ; l. 43– The rise of mountains belt can also modified the oceanic circulation via changes in atmospheric dynamics (e.g. wind stress) and also via redistribution of atmospheric water fluxes from one basin to the other (e.g. Maffre et al. 2017). It should probably be introduced there as authors refer to such a thing in paragraph 4.4.

- P3 ; l70 – “We employ foraminifera proxy data for: surface ocean temperature, benthic ocean temperature, and benthic ocean  $\delta^{13}\text{C}$ , and compile this for seven time-slices. This is redundant with l. 85 of the same paragraph “Three different paleo/proxy datasets were compiled: surface temperature, benthic  $\delta^{18}\text{O}$  and benthic  $\delta^{13}\text{C}$ .”

- P3 ; l. 100 “noting that these proxies, like any proxy, are subject to uncertainties and limitations” is a pretty vague statement and do not help the reader to identify potential weakness of the model-data comparison. Short sentence listing main limitations of that proxies, especially the saturation effect and the bias at high latitudes toward the warm season (e.g. Sluijs et al. 2006 , Bijl et al. 2009 , Richey and Tierney, 2016) would be more useful.

- P7 ; l. 209 – [...] as an additional and independent control on  $\delta^{13}\text{C}$  (but not temperature). Written like that, what you mean is not very clear. Does it mean that you take into account temperature effect on  $\delta^{13}\text{C}$  ?

- P9 ; sub-section 3.1 – The first paragraph on  $\delta^{13}\text{C}$  sounds out of the scope of the section as it is presented from data perspective only, while other paragraphs in this section are exposing data-model comparison. I would therefore suggest that the authors re-organize the section and move this paragraph at the place they discussed the  $\delta^{13}\text{C}$  model-data fit.

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- Authors should also be careful to separate CO<sub>2</sub> forcing as it is in cGENIE (the one the authors refer to as equivalent CO<sub>2</sub> forcing, that take into account other GhGs forcing - for example l. 265 ; P9 and following paragraph) and past CO<sub>2</sub> values estimated from proxies (e.g. Sossian et al. 2018). I found it really confusing because the text switch from one to the other. My advice would be to use “eq. CO<sub>2</sub> forcing” when the authors refer to the parameter used as a forcing in cGENIE.

Technical correction

P2 ; l. 34 – Wrong year for the reference – Sepulchre et al. 2014

P2 ; l.55 – Lynch-Stieglitz, 2003

P5 ; l.134 – “potentially limiting the biological productivity” instead of “potentially limiting to biological productivity”

P10 ; l.292 – “benthic temperature” instead of “benthic T”

P11 ; l. 357 and following, please cite the figure you refer to (Fig. 9) when you state that the model has too cold N. Atlantic compare to data.

l.427 – “greenhouse gases” instead of “green gases”

P.20 ; l.613 – Remove “Bell et al2015”

Fig 2. – Please set the same boundaries for the vertical axis of each plot, otherwise it is very difficult for the reader to read the plot and to understand the differences in data from different time-slices.

Fig 3. – Horizontal scale with the label oriented as they are is difficult to read. A vertical scale, with horizontal labels would probably be more readable.

Fig. 7; Fig. 11 Top - Please keep homogeneous min/max boundaries for the shaded color-scale within the different panels in the same figure. It make it easier to visually compare one time-slice to another. It also highlight changes in the simulated  $\delta^{13}C$

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pattern, a thing that is not possible when color scale is different within each panel. Fig 7. Add the legend near the shaded color bar (something like “ $\delta^{13}\text{C}$  m-score”), even if it is stated in the caption it helpful for the reader, especially in that case where a lot of information are provided on the same plot.

Fig 7. In the caption, add (from Fig. 5 ....) (from Fig 6...) when components are also shown on another figure. This make is more readable !

Fig. 12 – Please label each panel with alphabet letters, so you can directly refer to the right panel in the text.

### References

Bijl et al. 2009, Early Palaeogene temperature evolution of the southwest Pacific Ocean, Nature Letters

Richey and Tierney, 2016, GDGT and alkenone flux in the northern Gulf of Mexico: Implications for the TEX86 and UK'37 paleothermometers. Paleoceanography

Butzin et al. 2011, Miocene ocean circulation inferred from marine carbon cycle modeling combined with benthic isotope records. Paleoceanography

Maffre et al. 2017, The influence of orography on modern ocean circulation. Climate Dynamics

Schneider and Schmittner, 2006, Simulating the impact of the Panamanian seaway closure on ocean circulation, marine productivity and nutrient cycling. Earth and Planetary Science Letters

Sepulchre et al. 2014, Consequences of shoaling of the Central American Seaway determined from modeling Nd isotopes. Paleoceanography

Sluijs et al. 2006, Subtropical Arctic Ocean temperatures during the Palaeocene/Eocene thermal maximum. Nature Letters

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