

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 #3

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I appreciated this paper in the spirit of “all models are wrong, but some are useful”. I don’t mean this to trivialize the work here, but to note that a number of very important constraints to the model, such as deep ocean temperature evolution, are poorly known. That said, a real benefit to the approach in comparison to other efforts to model the middle Miocene is that CO₂ is not treated as a known, but as a primary variable to be estimated from the model. Also, the model calculates a salinity flux adjustment to try to capture basin scale patterns- this should make an interesting target for data generators. Overall, what I appreciated about the approach here is that it takes multiple paleo targets into consideration, rather than simply trying to capture, for example, average global temperatures or global surface temperature gradients. The interplay the model

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allows between different types of paleo observations is very useful to a data generator like me.

The authors readily acknowledge that their CO₂ estimates are better understood as “CO₂-equivalent” rather than as direct CO₂ levels. Some in the community may want to hammer the high CO₂ estimates as completely unrealistic. However, I align with those who don’t think the available CO₂ estimates are very reliable and I urge the authors not to back off from their estimates as CO₂ too readily (admitting that vegetation and methane likely do contribute significant warming to the real Miocene world).

Along the way, the authors try to point out which kinds of inferences are strongly constrained by different data types and which are not. This is very useful to someone not using cGENIE- it helps them interpret what’s robust and what’s not. But the presentation here often just says “X constrains Y much more than Z does” without taking the step of diagnosing how the authors know that. A little more explanation would let the non-modeler get much more out of this analysis.

Because of the breadth of this study, I necessarily have questions that I’d like to see clarified:

Model set up: – can the authors assess the effect the lack of interactive clouds has on the sensitivity of surface temperatures to CO₂? – Does the model allow for Albedo- vegetation feedbacks (I assume not)? As above, does this mean it will tend to underestimate Earth System Sensitivity to CO₂? – GCM input generated with CO₂ @ 400 ppmv – Lines 175-182 (geographic/bathymetric choices such as not including Med, Greenland-NA connection, Bering Sea open/closed): can the authors provide the reader with a sense of whether these affect model results significantly – Specify model sensitivity to CO₂? The text specifies “which is as the present day” without giving a value?

Emphasis on bathymetry and closing/opening of seaways on deep ocean properties: – is it justified in comparison to more subtle modulators such as sea ice, ridge

bathymetry, small changes in T and S? I'm thinking of ideas such as put forth for Ferrari et al. to account for glacial storage of CO₂ that rely on mixing or lack thereof of the North Atlantic and Southern Ocean deep circulation loops – I'm not sure why the authors have an open CAS for all except the Holocene time slice, since they note how circulation between the deep North Atlantic and Pacific affect model fits of carbon isotope gradients and North Atlantic salinity – Circulation and deep ocean CO₂ inventory- is this examined explicitly? We know from Pleistocene CO₂ cycles that the storage of carbon in the Southern Ocean can give a 80-90 ppm CO₂ effect. Some theories (e.g. R. Ferrari et al. 2014) hypothesize that the connection between the North Atlantic and Southern ocean loops is critical to whether CO₂ is stored in the deep ocean or vented to the atmosphere. Can the authors comment on how the Southern Ocean CO₂ pump works in cGENIE and if the model is capable of capturing changes such as proposed by Ferrari on carbon storage?

Model-data Methodology – Line 70 : We employ foraminifera proxy data for: surface ocean temperature, Vs Lines 100-101 Published surface temperature data selected are those using either alkenones or TEX86 for all seven slices (Figure 1 for surface 100 temperature data locations and Supplementary material – A further question: what is the balance of alkenone vs TEX86 data? Does the balance change between time slices? Which TEX86 index was used?

Benthic isotopes & temperature (105-107)?? – It's hard to follow this logic: “These species were selected so that temperature could be calculated from $\delta^{18}\text{O}$ using 105 Marchitto et al. (2014). Final benthic temperatures calculated from $\delta^{18}\text{O}$ take account of the effect of benthic water salinity on $\delta^{18}\text{O}$ which is affected by ocean circulation (the temperatures in Table S2 are uncorrected for salinity).” As the deep ocean has extremely small salinity variations, it's not clear why a salinity correction is needed. The deep ocean salinity will tend to be pretty homogeneous, because it is filled from only a handful of sources. Furthermore, the deep ocean salinity is constrained to be very close to the average salinity of the entire ocean, since only the thin skin layer of the

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ocean exchanges water with the atmosphere. The only way to make large changes in average salinity on the timescale considered here is to remove/add water by forming/melting ice. The correction for the ice volume effect on isotopic composition, is potentially large and not very well constrained from the Pliocene and earlier, as the volume and the isotopic composition of polar ice aren't well known. (see e.g. Bohaty et al 2012, EPSL for an example taken from the Eocene/Oligocene transition). \hat{A} In line with the latter point, how large do the authors think the deep ocean temperature uncertainties are, and how do they evaluate this? \hat{A} Clarify this: "Finally, local salinity has a strong control on $\delta^{18}\text{O}$; an increase in measured benthic $\delta^{18}\text{O}$ in the N. Atlantic during the late Miocene that may be interpreted as evidence of a strong cooling, may actually be attributable to the increased salinity of the deep sea water, where salinity (rather than temperature) dominates the $\delta^{18}\text{O}$ signal recorded in the benthic foraminifera. With the onset of Atlantic overturning circulation during the Miocene, the salinity of deep N. Atlantic waters has a strong control on $\delta^{18}\text{O}$, and when 470 included in the temperature calculation results in increases of up to 3°C in some locations (compared to the temperature uncorrected for salinity) (Fig. 10)." The salinity of the global deep ocean is constrained to be the average salinity of the ocean (because deep water masses have only one or two sources and must have nearly the same densities throughout the global deep ocean); likewise for the $\delta^{18}\text{O}$ value of the deep ocean water. Therefore, I think the language above should be clarified so the "the $\delta^{18}\text{O}$ signal recorded" is rewritten as "the $\delta^{18}\text{O}$ signal recorded in the North Atlantic. . ." or similar. I take it from the topic sentence of the paragraph that the authors are looking at local patterns but the reader may mistake the intent of the later sentences.

Carbon isotopes \hat{A} $\delta^{13}\text{C}$ data: don't capture changes in terrestrial carbon storage. I was curious if it could affect the predicted atmospheric $\delta^{13}\text{C}$ (Figure 13, lines 387-392). The authors might also note that a change in atmospheric $\delta^{13}\text{C}$ would affect the average $\delta^{13}\text{C}$ of photosynthate. I'd like to see more discussion of how the atmospheric $\delta^{13}\text{C}$ was derived- the presentation here is too short to be useful to someone not intricately familiar with cGENIE. \hat{A} Although it's an older paper, this one was a nice

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one that I didn't see cited: Woodruff, F. and S. M. Savin (1989). "Miocene deepwater oceanography." *Paleoceanography* 4(1): 87-140.

• D13C as a tracer of circulation: I wondered if the d13C data base has a change in the proportion of *Uvigerina* vs *Cibicidoides* values over time? While we "correct" for isotope offsets between species, this may not always work.

Finally, the style of the paper wavers a bit between assuming no previous background and a lot of background. I think this level of explanation is not needed for the target reader- it's too low-level (lines 89-92): "The isotope of carbon, carbon-13, has been used as a tracer for paleo ocean circulation for many years (Lynch-Stieglitz 2003). It is a stable isotope, heavier than carbon-12 and accounts for about 1% of all carbon on Earth. The ratio of carbon-13 to carbon-12, designated as " $\delta^{13}\text{C}$ " – the divergence from a standard in units of parts-per-thousand (‰, can be estimated for paleo ocean waters by measuring the $\delta^{13}\text{C}$ in shells of foraminifera formed in those paleo water masses. . . . Shells of dead foraminifera gradually accumulate on the ocean floor, thus providing a record of changes in water column chemistry over time at that location in the form of ocean sediments"

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