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Climate of the Past Discussions

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

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: âĂć -can the authors assess the effect the lack of interactive clouds has on the sensitivity of surface temperatures to CO2? âĂć – 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? âĂć GCM input generated with CO2 @ 400 ppmv âĂć âĂć 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 âĂć 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: âĂć 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 âĂć 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 âĂć 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 âĂć 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 âĂć 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)?? âĂć It's hard to follow this logic: "These species were selected so that temperature could be calculated from δ18O using 105 Marchitto et al. (2014). Final benthic temperatures calculated from δ18O take account of the effect of benthic water salinity on δ18O 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

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). âĂć In line with the latter point, how large do the authors think the deep ocean temperature uncertainties are, and how do they evaluate this? âĂć Clarify this: "Finally, local salinity has a strong control on δ18O; an increase in measured benthic δ18O 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 δ18O 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 δ18O, 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 d18O value of the deep ocean water. Therefore, I think the language above should be clarified so the "the d18O signal recorded" is rewritten as "the d18O 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 âĂć d13C data: don't capture changes in terrestrial carbon storage. I was curious if it could affect the predicted atmospheric d13C (Figure 13, lines 387-392). The authors might also note that a change in atmospheric d13C would affect the average d13C of photosynthate. I'd like to see more discussion of how the atmospheric d13C was derived- the presentation here is too short to be useful to someone not intricately familiar with cGENIE. âĂć Although it's an older paper, this one was a nice one that I didn't see cited: Woodruff, F. and S. M. Savin (1989). "Miocene deepwater oceanography." Paleoceanography 4(1): 87-140.

âĂć 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'm 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 90 to carbon-12, designated as "δ13C" – the divergence from a standard in units of parts-per-thousand (‰, can be estimated for paleo ocean waters by measuring the δ13C 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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