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Interactive comment on "Including a full carbon cycle into the <i>i</i>LOVECLIM model (v1.0)" *by* N. Bouttes et al.

J. Tjiputra (Referee)

jerry.tjiputra@bjerknes.uib.no

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General comments:

In this study, Bouttes et al. describe and evaluate the new land and ocean carbon cycle components coupled to the iLOVECLIM model. The manuscript is well written and easy to follow. They showed that the model is capable of broadly reproducing the observed distribution patterns of key ocean biogeochemical tracers. Statistically, the model also performed comparable or better than some more complex CMIP5 models. This is very encouraging and highlight the potential of using the computationally efficient EMIC model to study past climate variability involving the global carbon cycle interactions.

The study fits very well within the scope of GMD, through documenting the new devel-

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opment and analyzing the new model performance. Below I have some suggestions which I thought could clarify improve the manuscript prior to publication. Most of them are quite straight forward to address.

Specific comments:

1) Land/Ocean imbalance. The title give away the impression of land and ocean carbon cycle, but there is an obvious imbalance in the content, e.g., 1 fig for land and 21 figs for ocean. I am not a terrestrial modeler and not qualified to give a fair judgement whether the presented land evaluation is sufficient for the purpose of future studies using this model.

2) How well does the model conserve tracer's mass? If there is no sediment, do you assume all export, PIC/POC, are remineralized back into the water column before reaching the ocean floor? If not, is there any riverine fluxes? Does the model has any drift on the DIC/nutrients/O2/etc budget?

3) Biological production. I feel that the discussion around primary/export production can be improved. Many of the tracers (nutrients, O2, pCO2, ALK, DIC) shown here depend on the spatial distribution of surface primary and export production, as the author correctly noted in Fig. 1. Since the model includes an NPZD type ecosystem model, it would be useful also to show the surface distribution of NPP and export, and compare that with estimates from e.g., remote sensing data. How good is the annual globally integrated NPP and export? What is the PIC/POC ratio in the model? Are the vertical remineralization profile is the same for PIC as for POC?

On P3943, L17-18: what is the motivation for modifying the vertical remineralization rate? Was it to improve model-data fit?

4) Air-sea oxygen fluxes. P3943, L4 states O2 is prescribed to saturation values, but on P3948, L18: you mentioned there is an exchange with the atmosphere. How do you prescribed surface O2 and at the same time prognostically simulate O2 sources (to

photosynthesis) at surface? Please reformulate the sentence. Can the authors elaborate why O2 gas exchange is not implemented in the model? It should be relatively similar to the CO2 fluxes and won't take much computational time.

5) Air-sea CO2 flux. Which formulation is used? How is the spatial distribution compare to observed estimates? What is the annual CO2 flux globally (also for land)?

6) Oxygen is quite low in the deep North Atlantic. Is this because of the prescribed to the saturation state of O2 at surface? If so, it is possible that bias in SST/SSS translates to/enhances this bias. Have you compared the saturated O2 computed using WOA SST/SSS with surface O2 data from WOA?

7) Related to P3948, L20, in much of the high latitude Southern Ocean, O2 is also underestimated despite reasonable SST. Seems to contradict the statement on the NW Atlantic and Benguela upwelling regions. Maybe clarification on the biological coupling to the surface oxygen (point no.3 above) can explain this.

8) P3949,, L2: This is unconvincing to me. How much of this bias is due to the, say too strong remineralization rate at depth or lower surface productivity? It would be useful to plot the preformed vs regenerated PO4 (Duteil et al., 2012, Biogeosciences).

9) P3950, L4: Maybe i misunderstood this, but by including iron cycle (limitation), production should decrease, less CO2 uptake by photosynthesis, thus even higher simulated pCO2.

10) For the disagreement in the delta13C values in the interior N. Atlantic, the authors attribute this to the too much diffusion (P3950, L15). It appears to me that the biology in the equatorial Atlantic also plays a critical role: too much export production, which lead to too much remineralization, also seen in the O2 and PO4 signals. So the export/surface production map would be useful here. What is the role of too weak AMOC, as noted in the manuscript? I understand that it is challenging to isolate the reason for this bias, but it just seems more than simply 'diffusion'.

Technical comments:

P3939, L28 is the same as L26. Remove or rephrase one.

P3940, L19: consider revising the last part of the sentence. E.g., replace "and improve our understanding and model simulations" with "to calibrate model simulations and improve our understanding."

P3941, L3: developed

P3942 L26: some description on the carbon chemistry would be useful (e.g., OCMIP protocol?).

P3945, L25: clarify what is meant by "adjusted" here.

P3950,L13: north

P3950, L14: do you mean the 'high' delta13C values?

P3951, L12: too much northward

P3952, L12: Additionally

P3952, L17: add "statistically" : statistically perform best . . .

P3954, L6: reproduces

P3959: add space in oceanmodels

P3960, L23: replace 'e. a.' with 'et al.'

Units missing from Figs 9-19. Adding latitude labels on the surface maps would be useful.

P3979, Fig 17 caption: 'distribution' and 'Takahashi et al.'

P3982, Figs. 20/21/22 captions: clarify if this is surface/3D fields, area-weighted or not?

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