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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.

Anonymous Referee #2

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This paper describes the introduction of an ocean carbon cycle model, including the carbon isotopes 13C and 14C, into the iLOVECLIM model (which already contained a terrestrial carbon cycle). I think this is a good paper that is acceptable with minor revisions.

The English is fairly good but could use a thorough editing by someone entirely fluent in English (the running title contains an unnecessary 'the' and there are lots of these in the text). The writing is adequate but could have benefited from a bit more thorough editing before submission. There are numerous references to processes not included in the model, with comments like "this could be included in future versions" or "this could be improved to be more realistic". I would prefer to see these statements deleted, and the paper focused on what was done, what was demonstrated, and what the strengths and weaknesses of the current model are. If they want they can include a paragraph in the discussion that speculates about what missing processes might account for some of the discrepancies between model and observations, but I would prefer that these not be sprinkled throughout the text.

Main points:

(1) In general a lot of the claims about model skill in this paper are qualitative and subjective, and have a bit of an arm-waving quality about them. It's hard to tell exactly what is meant by "The thermocline is well represented" (3947/14), for example (see also 3948/16, 3948/25). The discussion of alkalinity on 3949/11-15 is not very convincing. Overall the model does not look very much like the observations, especially in the Atlantic, other than that it reproduces the global spatial pattern to first order (as does almost any model). The maxima associated with the subtropical gyres result from excess evapouration, and before one can even begin to speculate about the role of biological processes in generating the model errors one should look at the salinity-normalized alkalinity, because surface alkalinity to first order follows salinity. The salinity map in Figure 5 shows a pattern similar to the alkalinity but generally with a low bias in the Atlantic whereas alkalinity has a high bias. But I think it is worth examining further the role of the freshwater balance in generating the errors in modelled alkalinity in the Atlantic (contrary to what is claimed in the text, surface alkalinity is too high almost everywhere in the Atlantic and in some places these errors are quite large). I don't in general find subjective comparisons of maps very useful, and Figure 21 shows that when quantitative metrics of model skill are invoked, skill for alkalinity in the Atlantic is indistinguishable from zero.

In the Taylor diagrams (Figures 20-22), it appears that what is being compared here are the basin zonal means from Figures 12, 15 and 16. This is (obliquely) stated in the text (3952/5-7) but needs to be stated in the figure captions. These are the only quantitative assessments of model skill in this paper and the captions do not state whether the Taylor diagrams are comparing vertical or horizontal distributions or both,

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or over what latitude and longitude ranges. (Note that Figures 18-19 also appear to show basin zonal means but again this should be stated in the captions.)

I would also like to know why there are no Taylor diagrams or other quantitative skill assessments for 13C and 14C. Again all of the assessment is qualitative and subjective. From looking at Figures 18 and 19 it looks like the model is doing a reasonably good job of simulating the distributions of 13C and 14C, but I would like to see a bit more discussion of what one expects, i.e., what sort of model performance would we consider to be good enough, or what would we consider poor? Is there any literature to draw upon where similar experiments have been conducted with other models? Some of the statements regarding 13C strike me as questionable. For example, “13C also depends on circulation, so that NADW is characterized by relatively high values and AABW by lower values, in agreement with data.” (3950/10-12). Surface water is enriched in 13C because that’s where photosynthesis occurs, and deep water is depleted where there is a lot of cumulative remineralization (as in the deep Pacific), as described in the text at the beginning of this paragraph. So anywhere surface water is advected downward 13C will be high relative to surrounding waters at that depth. I don’t see any reason for NADW and AABW to differ in this respect, and I don’t see any evidence in the graphics to support this statement. The ’column’ of low 13C rising from the bottom at around 50S in the observations is probably upwelling (see e.g. Karsten and Marshall 2002 JPO 32: 3315) of water transported from the Pacific by the ACC.

(2) The purpose of the whole exercise is presented as developing a model that can be used to run longer simulations to examine past (e.g. glacial) climates, but the description of the exact time scale intended is ambiguous. There are frequent references to a time scale of 100s to 1000s of years. This is long enough to simulate a climate with glacial boundary conditions, but not to simulate glacial-interglacial cycles (e.g., 3939/29, 3940/21), and is not outside the capabilities of more complex models like IPSL-CM5A-LR. It would be possible, for example, for such a model run with glacial boundary conditions to be run to something fairly close to equilibrium (say 3000 years).

Given time and resource constraints most climate modelling labs would probably not do such an experiment, but it is not outside the capabilities of modern supercomputers. If the intent is to simulate glacial-interglacial cycles or even glacial-interglacial transitions, a time scale of 100s to 1000s of years is inadequate. On 3941/12 it is stated that "The general goal of the new developments within iLOVECLIM is to include the suite of processes needed for climate simulations on the Milankovic timescale", which is a lot longer than 100s to 1000s of years. (Note also that the simulation was said to have been run for ∼10000 years or "until it reached an equilibrium for deep ocean variables" (3946/1) but the criterion for convergence is not stated.)

(3) The errors in simulation of DIC and alkalinity are often attributed to the remineralization/dissolution profiles, but the description of what was done here is inadequate. Figure 2 refers only to POC and the text implies but does not actually state that the same profile is used for CaCO3 dissolution (3943/15-18). CaCO3 dissolution is frequently referred to as remineralization (e.g., 3949/14, 20, 23) which is not appropriate as CaCO3 is an inorganic compound.

(4) The text is ambiguous as to what was done regarding the terrestrial carbon cycle. At some points it says that the terrestrial carbon cycle was already included and the purpose of the present research was simply to incorporate the ocean carbon cycle, but at other points it refers to modifications to the terrestrial carbon cycle model but does not state exactly what these are or what their relationship to the research presented is. The title refers to the "full" carbon cycle but could probably be changed to say simply "ocean" carbon cycle.

If nothing has meaningfully changed in the terrestrial carbon cycle model it is not clear to me that the descriptions of terrestrial vegetation at the beginning of section 3, or Figure 3, are necessary, especially given that there is no feedback from the ocean carbon cycle to the climate. "The total vegetation cover simulated by the model (Fig. 3) is in agreement with the one from another version of LOVECLIM" doesn't seem like a very useful analysis as it is essentially the same model. On 3951/2-3 it states that "The terrestrial biosphere has only been slightly modified to include the carbon reservoirs", but I find it hard to believe that these were not already part of the model.

The description is also ambiguous regarding the unvegetated fraction. It is initially referred to as "bare soil" and later as "desert" (3943/24). The latter term is not defined and in my view not appropriate as most desert areas are not entirely devoid of vegetation. It appears to be equivalent to the "bare soil" fraction but this is not stated.

(5) The description of the atmospheric CO2 boundary condition is also ambiguous. It is clear that the atmospheric radiation code does not use the simulated concentration and a constant concentration of 280 ppm is stated (3945/12). There are several subsequent references to the "data" or "measured" values, but no observational data set is referenced. I assume the "data value" means the specified constant value of 280 but this needs to be made clear. The rate of cosmogenic production of 14C is stated (3945/23), but it is not stated if this occurs at all levels of the atmosphere is only in the top one. This probably doesn't matter that much as only an areal rate is stated and the atmospheric circulation will redistribute it among the layers fairly quickly, but I think it is appropriate to specify whether or not it is initially injected into all levels or just the topmost one. Section 2.3 is rather verbose, without being very informative regarding the critical details of the model setup.

(6) The simulation (which uses preindustrial boundary conditions) was compared with a contemporary (1990-1999) climate from the CMIP5 historical experiment (3952/1-12), but it is not stated why this was done. Preindustrial simulations are available for most of the CMIP5 models. Perhaps there should be a section in the Methods that describes the validation data sets (I consider the CMIP5 models to be among these, as comparing to higher-resolution ESMs is standard for evaluating an EMIC). Then such decisions could be explained up front instead of appearing out of nowhere in the Results or Discussion.

(7) I don't understand the assumptions made regarding biological fractionation (or not)

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of 14C. Because 14C is the heavier isotope, I would assume that the fractionation effect is stronger than with 13C. In the present-day world the effect of isotope fractionation is negligible because a massive pulse of excess (bomb) 14C was put into the atmosphere over a very short period (effectively instantaneous relative to the time scale of ocean circulation). So biological fractionation plays a very small role in the redistribution of this excess 14C in the ocean. This does not necessarily mean that the biological pump as a whole plays a negligible role, although it is often assumed that it does. But I don't see why this should a priori be assumed to be the case in the preindustrial world where inputs of 14C to the atmosphere are much more gradual. At least, it should be clearly stated that biotic uptake and sedimentation are neglected. The present text simply states that fractionation is neglected (3944/23-25).

I can't make any sense of "14C in a reservoir is mainly driven by the distance between this reservoir and the atmosphere where 14C is formed by cosmogenic production. The more distant this reservoir is from the atmosphere, the more depleted in 14C it will be, because of radioactive decay." (3945/1-4) This seems to imply that the 14C activity should decrease monotonically with depth, which is not the case since intermediate ocean waters are generally older than deep waters.

They might also want to state something about the range and precision of estimates of preindustrial del14C (3946/5-6). I think a model value of 1.5 permil vs an observed (?) value of 0 is very good, but not all readers will know this. Many readers might have no idea what the range or precision are. Also why is there a literature reference given for CO2 concentration and del13C but not for del14C?

(8) The boundary condition for oxygen could also be better explained. From what I can tell, there is no air-sea exchange: O2 is simply set to the saturation value in the surface layer of the ocean. But I find this hard to reconcile with Figure 11, which shows that the observed oxygen concentration is larger than the modelled over large areas of ocean. This in turn implies that the mean concentration over all of these areas is significantly supersaturated, which doesn't make sense to me. Supersaturation can

develop when cold water warms rapidly or through excess photosynthesis, but at the surface gas exchange should limit supersaturation to within a few umol/kg of the saturation concentration. This could be due to errors in model SST but these don't look very large (Figure 4). It would be worth trying to explain this apparent anomaly because this figure looks suspicious to me.

Some details:

3938/7 "anthropogenic carbon emissions are predicted to continue in the future". Probably true but not consistent with the way the word "predict" should be used in climate science.

3938/15 "The model can thus be used for long-term past and future climate–carbon studies." Again, probably true, but I don't see how this follows from what is stated in the previous sentence.

3939/4 "influences the development of terrestrial biosphere and its decomposition" influences the development of the terrestrial biosphere and decomposition of terrestrial organic matter

3939/6 "climate-carbon retroactions" interactions

3939/16 "range from simple box models to Global Climate Models" I don't recall there being any box models in C4MIP. I'm also not sure there were any models in C4MIP with a terrestrial N cycle as seems to be implied by 3939/17-19.

3940/8 and elsewhere change "values" to "concentrations" (e.g. 3948/27-28, 3949/1-3, 13, 28, 3951/16)

3940/13 "the measure of 13C and 14C in sediment cores" measurement

3940/26 "avoid increasing the computing time exceedingly" excessively

3940/28 "Processes linked to sediments" Sedimentary processes

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3941/8 “a code fork” a jargony term that could be avoided

3941/11 “oxygen water isotopes” water oxygen isotopes

3942/2 “a parameterisation of downsloping currents” downslope

3943/13 “part of it is exuded to DOC (and DOCs) and part to POC” The term “exuded” is not appropriate here as it refers specifically to losses to the dissolved phase. The arrow in Figure 1 shows exudation going to DOC only.

3946/24 “the distribution of the variables in the ocean” tracers? chemical species? biogeochemical fields?

3947/1 “The ocean dynamics, which depend on temperature and salinity gradients” is this really necessary?

3947/8 delete “perfectly”

3947/13-26 I’m not sure it is appropriate to refer to all of the water masses named in the text without identifying them on any of the plots. For a reader familiar with oceanography it’s not much of a burden, but in general it’s not good practice.

3947/28 “compared to values between 14 and 31 Sv” This sentence gives no indication of what this range of estimates is for. Is it for CMIP5 models? Please clarify.

3948/9 “their use by marine biology” biota

3950/17 “deconvoluate”

3952/21 “most models simulate badly” poorly (see also 3952/24)

Fig. 17 caption “distribution” misspelled

Should the legends to Figures 20-22 refer to iLOVECLIM instead of LOVECLIM?

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