Answer to reviewers

We thank both reviewers for their comments, which we address point by point below. We also thank the reviewer and have addressed his last comment just below. We have highlighted the corresponding changes in the manuscript in red.

Editor

Thanks for providing a revised version of your manuscript. I found your new version addresses most comments raised by the reviewers. Particularly, I appreciate the change in the title of the manuscript; the previous title was misleading and the new one better describes the content of the manuscript and the model description.

I have only one minor comment that I would like the authors to address in a final revised version. In section 2.2.3 you should also include the equation for Δ^{14}C that pertains to the values you report. In the 14C terminology there's usually a lot of confusion about the corrections for fractionation and radioactive decay. Please use any of the standard terms described in Stuiver & Polack (1977) or in Mook & van der Plicht (1999).

As suggested, we have added the equation for Δ^{14}C from Stuiver & Polack (1977) in the corresponding section.

Reviewer #1

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 development 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.

The focus of this paper is indeed on the ocean since the model for the terrestrial biosphere already existed and has been described in Goose et al, 2010. We have added onexly minor changes to the terrestrial biosphere to include the isotopes in all carbon pools. As suggested we have modified the title to: “Including an ocean carbon cycle model into i LOVECLIM (v1.0)”.

2) How well does the model conserve tracer’s mass? If there is no sediment, do you assume all expor t, 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?
As first guessed by the reviewer, everything is remineralized/dissolved back in the water column. There is no riverine flux and no drift, all mass is conserved. We have made this clearer in section 2.2.1.

3) Biological production. I feel that the discussion around primary/expor t production can be improved. Many of the tracers (nutrients, O2, pCO2, ALK, DIC) shown here depend on the spatial distribution of surface primary and expor t 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 expor t? What is the PIC/POC ratio in the model? Are the vertical remineralization profile is the same for PIC as for POC?
Unfortunately we haven’t been able to plot the primary production due to issues with the output of the model, but it will be done in further work. The dissolution/remineralisation profile for POC and CaCO3 is similar but with different coefficients.

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

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.
It was a mistake on page 3948 l18, the oxygen is indeed prescribed to the saturation value at the surface as stated before in the manuscript. This has been corrected in the text.
Preliminary results to implement exchange of O2 have shown that it is not completely straightforward and will require more work, therefore it will be done in further work.

5) Air-sea CO2 flux. Which formulation is used? How is the spatial distribution compare to obser ved estimates? What is the annual CO2 flux globally (also for land)?
The air-sea carbon flux depends on the difference between the partial pressure of CO2 in the air and in the surface ocean with the gas exchange coefficient of 0.06 mol m-2 yr-1 (Brovkin et al., 2002). We have made this clearer in the text. We have compared pCO2 with data (the air sea CO2 flux data is computed from ocean pCO2 measurements).

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?

As suggested by the reviewer we have compared the saturated O2 using WOA SST/SSS and surface O2 data from WOA (see figure below). In the North Atlantic, the saturated O2 computed from data SSS and SST is in better agreement with data compared to the O2 from iLOVECLIM, showing that the low O2 values in the North Atlantic in iLOVECLIM are due to the errors from the ocean model as discussed in section 3.2. The low values are then transported to the deep North Atlantic where it explains the disagreement with data, which we have added in the text in this section.

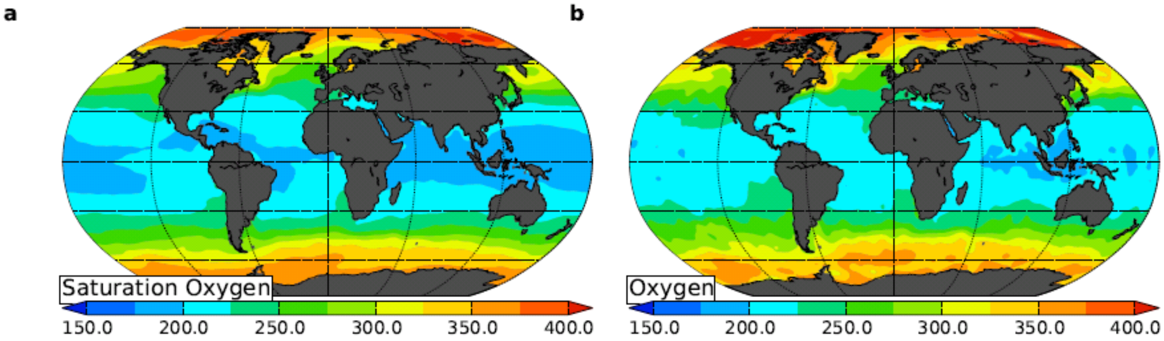


Figure: oxygen distribution at the surface (a) computed at saturation using data SST and SSS and (b) from data.

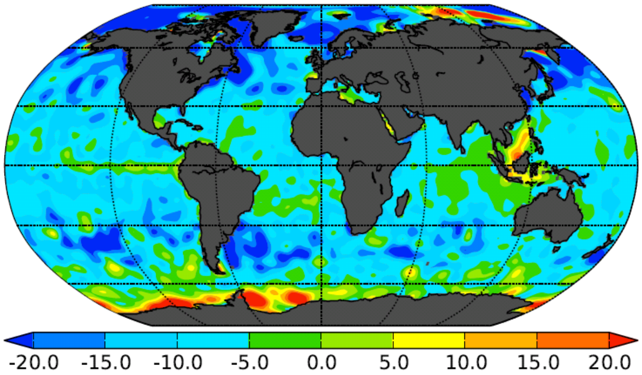


Figure: difference between the oxygen at saturation and the oxygen from data.

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.
The temperature is slightly overestimated by the model in parts of the Southern Ocean (Figure 4), leading to the underestimation of oxygen in these regions. This has been added to the text.

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).
As suggested by the reviewer we have plotted preformed phosphate as in Duteil et al. (2012) (figure below). At depth the concentration of preformed phosphate is lower in the model compared to data, implying that there is not enough preformed phosphate and too much regenerated phosphate. This indicates either that there is too much production and remineralisation or that the ocean circulation is too sluggish. This is a very similar indication to carbon isotopes, hence we haven't included it in the manuscript.

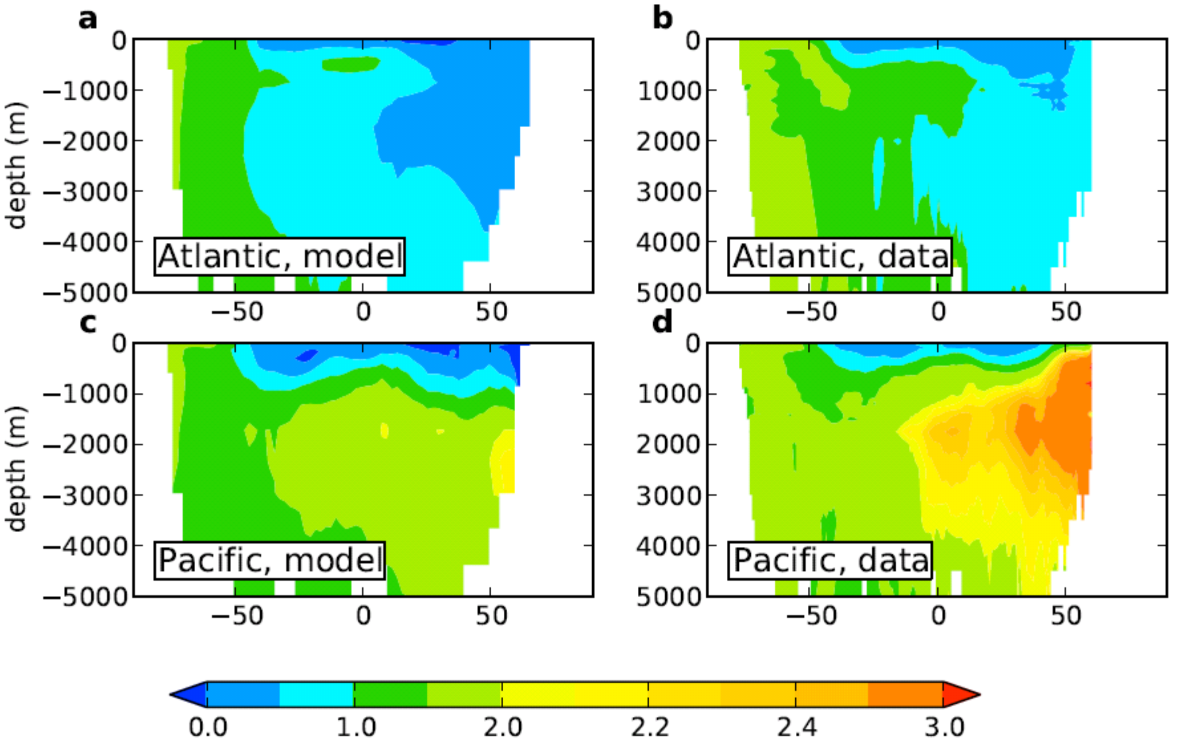


Figure: preformed phosphate computed as in Duteil et al. (2012): (a and c) for the model and (b and d) for the data.

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.

As noted by the reviewer iron cycle is unlikely to explain the mismatch. The latter could be due to not enough convection, but more work will be needed to decipher the cause of the disagreement and to improve the model results. This has been modified in the manuscript.

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 expor t production, which lead to too much remineralization, also seen in the O2 and PO4 signals. So the expor t/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'.
As noted by the reviewer, it is difficult to separate the different effects, which should be done in future work.

Technical comments:
P3939, L28 is the same as L26. Remove or rephrase one.
This has been changed.

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

P3941, L3: developed
Done

P3942 L26: some description on the carbon chemistry would be useful (e.g., OCMIP protocol?).
We have added more details in section 2.2.1 on the carbon chemistry, which is computed exactly as in Brovkin et al. (2002).

P3945, L25: clarify what is meant by "adjusted" here.
The total amount of carbon is set to obtain the correct atmospheric CO2 using iterative runs. This has been modified in the text.

P3950,L13: north
Done

P3950, L14: do you mean the 'high' delta13C values?
Yes, this has been added.

P3951, L12: too much nor thward
Done

P3952, L12: Additionally

Done

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

P3954, L6: reproduces
Done

P3959: add space in oceanmodels
Done

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

Units missing from Figs 9-19. Adding latitude labels on the surface maps would be useful.
We have added the units and the latitude labels.

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

P3982, Figs. 20/21/22 captions: clarify if this is surface/3D fields, area-weighted or not?
We have modified the captions to make it clearer (it is for the zonally averaged distribution).

Reviewer #2

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.
There is a paragraph at the end summarizing future development, but it seems useful to us to explain the possible cause of a problem when it is identified, and what could be done in the future to improve it, especially in a paper on model development.

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, 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.

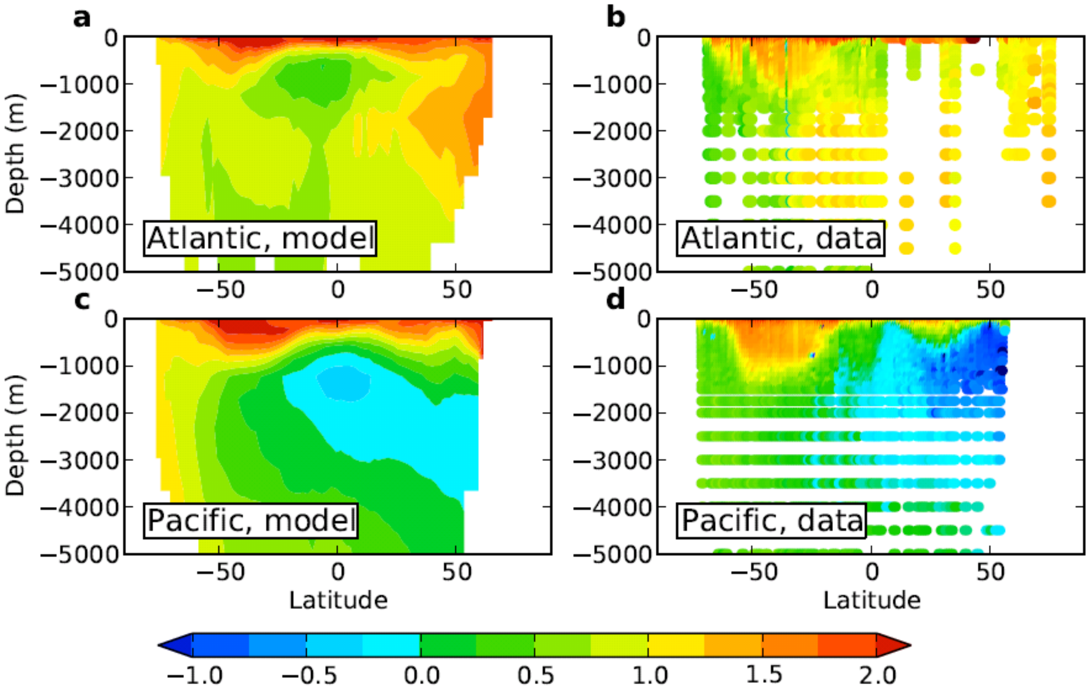
We have modified the captions of figures 18 to 22 to include the information about what is compared in the Taylor diagrams.

There is no Taylor diagram for 13C and 14C because there is no 13C data and no published 14C data available from OCMIP.

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 *suppor t 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.*

The figure was probably misleading because of the interpolation and the small number of data. We have modified the d13C figure to use an improved dataset that contains the data that we were using before (GLODAP) in addition to other data (Schmittner et al., 2013). We show the observed values directly and don't do an interpolation anymore. The distinctive values of d13C for NADW and AABW are more easily discernible with the new figure. The difference between the d13C values is due to the fact that NADW is more ventilated than AABW and therefore has higher d13C values.

A discussion of the performance of the simulated C14 compared to other C14 models has been added in the paper.



New figure for d13C (Figure 18) using data from Schmittner et al. (2013) and no interpolation.

(2) The pur pose 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 boundar y 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 transi-

tions, 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.)
The objective is to use the model for timescales of at least a few thousand years, this has been made clearer in the text.

(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.
We have replaced "remineralization" by "dissolution" when it refers to CacO3.

(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.
As discussed in question 1 of reviewer #1, we agree that the title might be misleading. The terrestrial carbon was already modelled and has been described previously, the only modification that has been made here concerns the carbon isotopes. This has been made clearer in the text and the title has been modified, see response to reviewer #1, question #1.

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.
The desert fraction is the term used by Brovkin et al., 2002 (GBC) in the description of the terrestrial biosphere module.

(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.

Regarding CO2, we have modified the text to make it clearer when we refer to the pre-industrial value of 280 ppm.

Regarding carbon 14, in the iLOVECLIM model the atmosphere component consists in just one box. The rate of cosmogenic production of 14C is stated in atom 14C cm-2 s-1 in the paper to be comparable with the value measured by Masarik and Beer (2009). In the model, this number is then integrated over the Earth surface. This integrated flux is thus applied once to the atmosphere box. This has been made clearer in the text.

(6) The simulation (which uses preindustrial boundary conditions) was compared with a contemporar y (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.

We agree with the reviewer that it would be better to compare our results with a closer period from the CMIP5 model simulations. Therefore we are now comparing with the model results for years 1890-1899 instead of 1990-1999. Because we're comparing the vertical distribution, which takes a few hundred years to be modified, it results only in very minor changes in the comparison.

As suggested, we have added a section on the data (new section 2.4).

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

The reviewer is right: biological fractionation of 14C is considered to be twice the biological fractionation of 13C because 14C isotope is heavier than 13C. Thus, compared to 13C biological fractionation, 14C biological fractionation is clearly not negligible. The sentence in the text was misleading as it was written and it has been made clearer.

The reviewer is still right saying that fractionation should not be *a priori* assumed negligible when establishing a 14C representation in an Earth System Model. We made the choice of neglecting fractionation processes so that they can be directly compared to Δ14C values provided in observations (e.g. Key et al. 2004) and 'paleo' reconstructions (e. g. Burke and Robinson 2012). Δ14C are indeed equal to δ14C corrected for fractionation. Basically, the 14C concentrations are corrected in order to be directly convertible in time through the radioactive decay relation.

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.
The sentence was indeed not clear. What we meant is that the age of a reservoir of 14C reflects the time since when this reservoir has been in direct contact with the atmosphere. Thus, for water masses, this implies that the longer they have been separated from a direct contact with the atmosphere, the older they are, which is compatible with some intermediate ocean waters being older than deep waters due to certain ocean circulation pathways, like in the Pacific Ocean for example. This has been made clearer in the text.

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?
For the range and precision of the model preindustrial value, it has been improved in the text. The reference for 14C has been also added.

(8) The boundar y 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.
The O2 is set to the saturation level at the surface, this has been made clearer, see response to reviewer #1 question #4 and 6.

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.
This has been changed to: Are likely to continue

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.
This has been removed.

3939/4 “influences the development of terrestrial biosphere and its decomposition” influences the development of the terrestrial biosphere and decomposition of terrestrial organic matter
This modification has been done.

3939/6 “climate-carbon retroactions” interactions
Done

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.
The sentence with box models in it was not referring to C4MIP. To make it clearer we have created a new paragraph there.

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

3940/13 “the measure of 13C and 14C in sediment cores” measurement
Done

3940/26 “avoid increasing the computing time exceedingly” excessively
Done

3940/28 “Processes linked to sediments” Sedimentary processes
Done

3941/8 “a code fork” a jargony term that could be avoided
It has been made easier to understand by using “new development branch”. We have nonetheless also kept “code fork” as it is the correct description.

3941/11 “oxygen water isotopes” water oxygen isotopes
Done

3942/2 “a parameterisation of downsloping currents” downslope
Done

3943/13 "par t of it is exuded to DOC (and DOCs) and par t 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.

This has been modified.

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

Changed to tracers

3947/1 "The ocean dynamics, which depend on temperature and salinity gradients" is this really necessar y?

This is just a reminder.

3947/8 delete "perfectly"

Done

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.

We have added NADW and AABW on figure 6a.

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.

This refers to CMIP5 models studied in Weaver et al., 2012., This has been made clearer in the text by adding: "for CMIP5 models".

3948/9 "their use by marine biology" biota

Done

3950/17 "deconvoluate"

Changed to "separate".

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

Done

Fig. 17 caption "distribution" misspelled

Done

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

Done