Anonymous Referee #2

We thank the reviewer for his/her comments that help to increase the quality of the paper. We provide hereafter answers to the GENERAL and SPECIFIC comments of the reviewer. We tried to implement them faithfully in the revised manuscript in preparation. The referee's original comments are in italic, reply by Loutre et al is in plain text, and text included in the revised manuscript is in bold.

1) Why is there increased AMOC variability towards inception?

Clearly, the model evolves towards a different mean state at the end of the simulation. The model becomes close to a bistable regime. Actually this behaviour has already been discussed previously (Jongma et al., 2007). For example, it has been shown that, under certain conditions, a small amount of freshwater will lead the model to oscillate between two modes. However, adding freshwater is not required. Rather, internal variability can provide changes large enough. The glacial inception and the discussion of bistable mode in LOVECLIM are clearly out of the scope of the paper.
One sentence is added: "**We hypothesize that the model becomes close to a bistable regime, which makes it oscillating between two modes**."

2) Why is reconstructed EPCA temperature change much larger than what is simulated?
3) Why is amplitude of summer temperature change during LIG much smaller in model than in the proxies?

Several authors already pointed out that the amplitude in the modelled temperature change is smaller than in the proxy during the LIG (see for example Lunt et al., 2012). These authors suggested that some proxies might be biased towards warm growth-season changes. Recently, Bakker and Renssen (2014) confirmed that this effect could at least partly explain the difference in magnitude between simulated and reconstructed temperatures. On the other hand, several processes taking place in the climate system might not be fully represented in the model and might be responsible for at least part of the discrepancies. We can mention, amongst others, the representation of changes of the ocean circulation in the southern ocean, changes in the stratification, changes in the meridional heat exchange in the ocean, as well as the representation of clouds and radiative budget in the atmosphere. Unfortunately, it is not possible to test these hypotheses in the present framework.

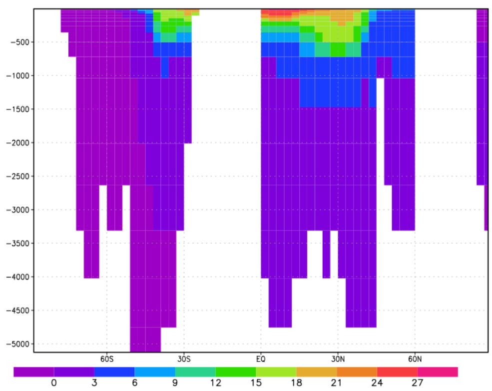
4) What is difference between figs 6a,b and fig. 4 in Bakker et al. (CP, 2013)? If they show significant differences - why?

The purpose of the figure is the same. However, the model versions and the methods for computing the MWT are different. Nevertheless, the major patterns are similar in both simulations. MWT occurs late in the LIG in January and early in the LIG in July over most of the Earth. Both also identify an early MWT in the high Northern latitude although it is not considered as highly significant according to our methodology. MWT at ~ 122kyr BP over Europe in January is also a similar pattern for both papers. The major differences between them are an early MWT over the Southern Ocean and the West Antarctic ice sheet in January in our paper as well as a MWT occurring between 125 and 120 kyr BP in the equatorial region in July. It must be kept in mind that the amplitude of the temperature change in Antarctic is small, which might explain the discrepancy between the papers. Moreover, the Southern Ocean is subject to overshoot, which may not have occurred in Bakker et al (2013) due to slightly different experimental conditions.

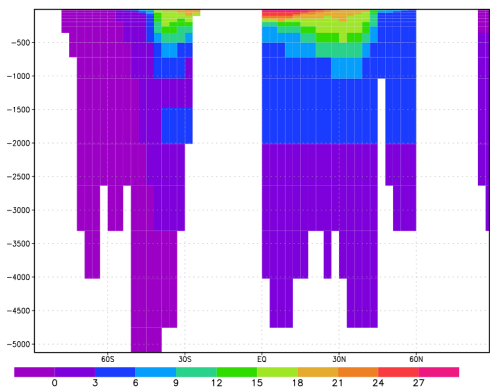
This has been added in the manuscript:
These results are in general good agreement with a similar study (Bakker et al., 2013). The use of a slightly different methodology and of a different model version may explain the major differences between both studies.

5) Why does including NH ice sheets stabilize AMOC and reduce impact of freshwater forcing (e.g. page 251, line1).

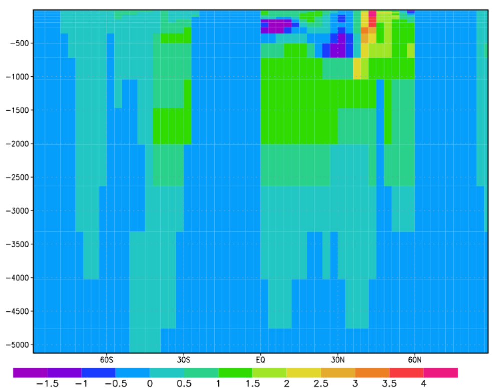
allGR



fwfGR



fwfGR-allGR



Annual mean potential temperature at 45W at 134 kyr BP (averaged over 100 years) according to allGR (left) and fwfGR (right). The difference between the figures on the top row is presented on the second row.

When the NH ice sheets are included (as in allGR), the sea surface temperature in their vicinity decreases. This favours the deepwater formation and therefore stabilizes the overall Atlantic overturning circulation (Renssen et al., 2005). This explanation is added in the manuscript.

6) Why is variability in experiment fwfGR high towards end of LIG (section 4.2)? If there is FWF forcing included in this time period it should be shown clearly in fig. 3.

Clearly, the model evolves towards a different mean state at the end of the simulation. The model becomes close to a bistable regime. Actually, this behaviour has already been discussed previously (Jongma et al., 2007). For example, it has been shown that, under certain conditions, a small amount of freshwater will lead the model to oscillate between

two modes. Here it is only related to internal variability. The glacial inception and the discussion of bistable mode in LOVECLIM are clearly out of the scope of the paper.
One sentence is added: "**We hypothesize that the model becomes close to a bistable regime, which makes it oscillating between two modes as previously discussed in Jongma et al. (2007).**"

7) What causes drop in AMOC in experiment IGonly in late LIG (section 4.3)?

The abrupt change at ~120.5 kyr BP is discussed in the manuscript at section 7.1. It is related to a change of convection site in the Labrador Sea, a decrease in surface temperature in the Labrador Sea and in the Barents Sea, a decrease in sea surface salinity in the Hudson Bay, Baffin Bay and Davies Strait, a change in the pattern of convection in the Labrador Sea, and an increase in winter sea ice area in the NH. It has been suggested (Friedrich et al.; 2009) that such a feature may be due to a flush of freshwater from the Hudson Bay to the Labrador Sea due to changes in wind and pressure. Another hypothesis is that this event is associated with changes in the Nordic Seas Overflow (Galaasen et al., 2014). This latter reference is added in the manuscript. However, it is out of the scope of this paper to study the origin of this event and the processes that induced it.

8) The importance of changing model configuration is not clear (section 9). Need to give details of main differences between the two model configurations used. Do these span a large parameter space. I.e. is the statement that "external" forcing dominates over internal model uncertainty merited? If so, this needs more documentation.

The following table provides the differences between the parameter sets used in the two model configurations.

Parameter set	λ2 (m)	λ4 (m)	amplw	explw	albocef	albice	avkb	CorA
71	0.131	0.071	1.00	0.4	0.950	0.44	1.5	-0.0850
22	0.125	0.070	1.00	0.4	0.900	0.42	1.5	-0.0425

Table: Value of the major parameters involved in the parameter sets (column 1) used in this study. Parameters λ2 and λ4 (columns 2 and 3) are applied in the Rayleigh damping term of the equation of the quasi-geostrophic potential vorticity. The coefficients amplw and explw (columns 4 and 5) are used in the longwave radiative scheme to compute anomaly in humidity. The uncertainties in the albedo of the ocean and sea ice are accounted for through albcoef (column 6) and albice (column 7). The minimum vertical diffusion coefficient in the ocean is scaled according to avkb (column 8). CorA is a correction factor for the distribution of precipitation over the ocean (column 9). More details about these parameters are available in Goosse et al. (2007).
This material is included as supplementary. Indeed, it was already published in Goosse et al. (2007).
We also aim at keeping a balance between a model configuration that covers a large parameter space and a model that provides realistic simulation of present, pre-industrial and Holocene climates. The impact of the choice of the model configuration on its reponse to atmospheric CO_2 concentration increase and freshwater increase (see paper) is used as an indicator of the parameter space. Additional model configurations were tested in previous papers (Goosse et al., 2011; Loutre et al., 2012 ; Goelzer et al., 2012). The reviewer can see that indeed the model configuration used here is in the lower end of the full range of the tested model configurations.

However, we must take care here because sensitivity of the model configurations used here are in the lower end of the full range of the tested model configurations (Goosse et al., 2011; Loutre et al., 2012 ; Goelzer et al., 2012).

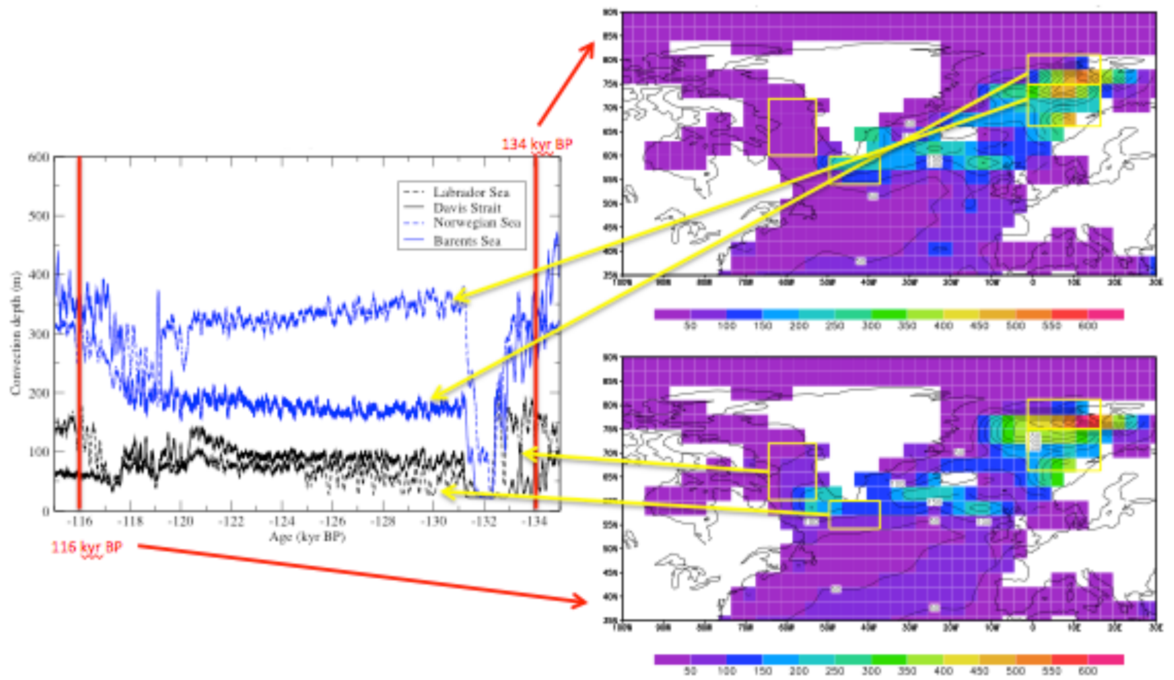
9) What is the impact of model simplifications used in LOVECLIM on the results (in particular the AMOC)? E.g. what is the impact of simplified low resolution atmosphere + ocean.

The LOVECLIM model has been part of several model intercomparison exercises that allow assessing its performances against more sophisticated models. LOVECLIM behaves like many general circulation models. Compared to other models (OAGCMs and EMICs), LOVECLIM is in the lower part of the climate sensitivity range. Gregory et al. (2005) did not see any systematic differences in the EMIC and OAGCM simulations of THC behaviour on decadal timescales. From an in depth study of the model, Opsteegh et al (1998) conluded that LOVECLIM can be used to study the fundamental nature of air/sea interaction in the extratropics although it cannot deal adequately with extratropical climate variability resulting from interaction with the tropics.
One sentence is added : «**LOVECLIM remains well within the range of other models. However, its climate sensitivity is in the lower part of the range of the other models and its dynamical response is weak.** »

10) What is the potential role of Antarctic ice sheets? Would including these change results significantly? Section 7.2 should be expanded.

The study of the potential role of Antarctic ice sheets on the climate during the LIG is clearly out of the scope of this paper. However, in a further study, we intend to present the LIG climate simulated with LOVECLIM coupled to an ice sheet model, including the Antarctic ice sheet. We will then be in a position to answer the referee's question. On the other hand the uncertainty about the evolution of the Antarctic ice sheet during the LIG (AR5, IPCC), both in time and space, remains large. So, it was instructive to focus first on the influence of Greenland ice sheet.

11) In the freshwater experiments the AMOC is reduced, however convection in the Labrador Sea is maintained. Is convection reduced other places, where? Add plot of change in convection for the relevant experiment.



Winter (DJF) convection depth (m) simulated with allGR. (left) time evolution for four sites of the North Atlantic: Labrador Sea (black dashed line), Davis Strait (black full line), Norwegian Sea (blue dashed line) and Barents Sea (blue full line). These sites are identified with boxes on the right hand side of the figure. (right) convection depth (m) for the North Atlantic at 134 kyr BP (top) and 116 kyr BP (bottom).

12) Include figure showing core locations (or add to one of the existing figures). A map is added in the revised manuscript.

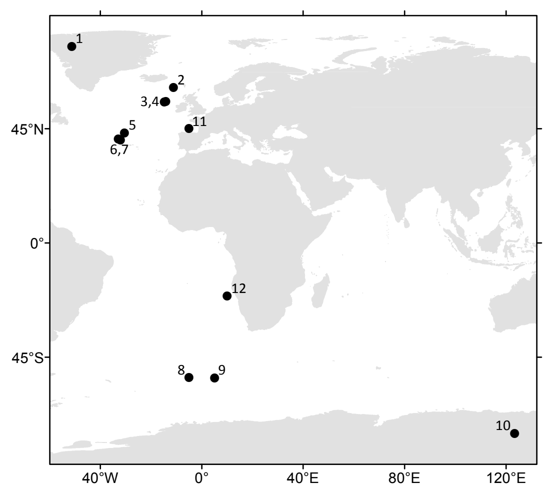
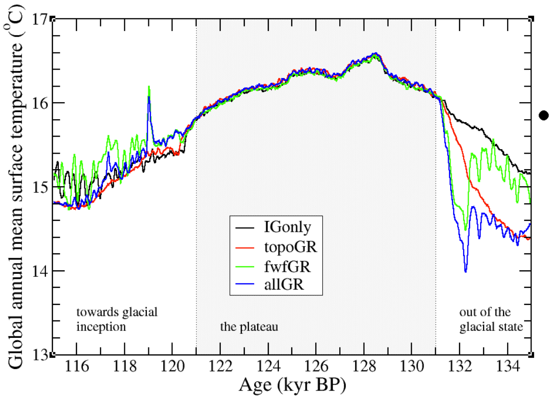


Figure : location of the study sites, including marine and ice core sites. (1) NEEM, (2) ENAM33, (3) ODP980, (4) NA87-25, (5) SU90-08, (6) V30-97, (7) SU90-03, (8) PS2102-2, (9) OPD1094, (10) EDC, (11) MD04-2845. Site (12) corresponds to an unpublished

marine core for which only the simulated temperature is available. Details about the cores and related proxy data are provided in the text as well as in Capron et al. (2014).

13) The three different time periods described in section 3 should be clearly marked on the figures with the model output (figs. 4 & 5). The figures are modified in the revised manuscript to take into account the referee's comments.



Time evolution of global annual mean surface temperature (°C) from model simulations using different surface boundary conditions. The series are smoothed using a moving average over 100 years. The black dot on the right hand side of the figures provides the corresponding simulated pre-industrial value. The three subintervals discussed in section 3 are identified.

SPECIFIC COMMENTS

page 238, line 19: LIG sea level is quoted as up to 6m higher than modern in pe- riod 130-116ka BP with references included. Note, however that Kopp et al. (Nature, 2009) estimates a sea level high to be above 6.6m and likely to have exceeded 8.0m. Statement in manuscript should be rephrased accordingly.

This has been re-phrased: «**From 130±2 kyr BP until the glacial inception at the end of the LIG, ca. 116 kyr BP, the sea level was at least 4 m above modern level (Tarasov and Peltier (2003)) but unlikely higher than + 9 m …** »

page 241, line 12: CO2 in figure 2 is only above 280ppm for a very short period. Should edit statement to match interval described.

We thank the reviewer for pointing out this typo, now corrected.

page 245, line 13: reference to parameter set 22 is not sufficient. Need to add at least a short summary of what this entails.

See response to GENERAL COMMENT #8.

page 247, line 3: original reference to seesaw should include Crowley (Paleoceanog- raphy, 7, 1992). This is now added.

page 247, line 12: should refer to figure 4a (not 5a). This now corrected.

page 247, line 20: the recovery at 24.8kyr BP is not clear from figure 4 or 5. What does this refer to? This refers to the relative minimum at 126.8kyr BP.

page 252, line 18: should remind reader how fwfGR differs from allGR. Same goes for topoGR on page 253, line 15. One sentence explaining fwfGR and allGR is added in the revised manuscript : "**First, we compare the reference simulation (allGR) with a simulation that does not take into account the evolution of the NH ice sheet configuration but only includes the freshwater forcing resulting from changes in ice volume (fwfGR)**" and "**The reference simulation (allGR) is compared here to a simulation that does not take into account the additional freshwater flux from the ice sheets but only includes the evolution of the NH ice sheet configuration (extent, altitude, albedo) (topoGR).**"

Figure 4 & 5: should add point/line showing Pre-industrial model values. (also add modern/late Holocene proxy values to fig. 5). Revised figures are proposed to take into account these comments.

Figure 5: should add caption/heading in each individual figure indicating location temperature record. Also need to add uncertainties to proxy data.

The figures are modified in the revised version of the manuscript in order to include a heading for the core location. Unfortunately, the uncertainties on the proxy data were not provided systematically for all the original studies. Therefore, we are not in a position to add them in the figure. However, we tried to be as precise as possible in the presentation of the proxy data in the manuscript regarding the range of uncertainties that could be reached based on the SST reconstructions method used. « **Uncertainties on each reconstructed SST record estimated from (1) the uncertainty on measurement and (2) the calibration of geochemical and microfossil proxies under modern conditions range between 0.6 and 1.9 °C depending on the SST proxies (e.g. Chapman and Shackleton, 1998; Oppo et al., 2006). It reaches up to 4°C for the NEEM ice core precipitation-weighted temperature reconstruction (NEEM community members, 2013).**»

Figure 9: Definition of sign used in this figure is not consistent with description of results in the text (section 4.4, e.g. page 256). This is now corrected.