Clim. Past Discuss., 7, C2550–C2573, 2012
www.clim-past-discuss.net/7/C2550/2012/

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Discussions

Interactive comment on "Persistent influence of ice sheet melting on high northern latitude climate during the early Last Interglacial" *by* A. Govin et al.

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Received and published: 6 February 2012

Editor Comments

Editor Both sets of referee's comments are now in and both are in favour of publication of this paper following minor changes. This is a provisional decision which should be regarded as final in the absence of further comments being added in this period of open discussion. Both reviewers require more discussion of your methods, in particular concerning age models and your model-data comparison. There are also important technical reservations about your δ18O calculations. Once these and the other points raised by the reviewers are addressed in full I anticipate that the paper will be acceptable for publication.

Authors We thank the editor and the referees for their constructive comments on our manuscript. Below we address successively the points raised by the three referees. In particular, more details are given in the revised manuscript on our methods (age models, δ18O calculations, temperature reconstructions). We also significantly increased the discussion on the limitations of the model-data comparison. In particular, we added a substantial (2-page long) new section 4.3.4 considering alternative mechanisms to the early LIG climatic pattern, in the context of other modelling studies (without freshwater input). In this new section, we discuss (1) the mechanisms leading to a cold North Atlantic at the beginning of the LIG (Felis et al., 2004; Kaspar and Cubasch, 2007), (2) the mechanisms responsible for a weakened AMOC simulated during the early LIG (Khodri et al., 2003; Gröger et al., 2007), and (3) the influence of changes in the Arctic freshwater budget on the AMOC and North Atlantic climate. We conclude that "further investigation with different models is clearly required to disentangle the climatic forcing and mechanisms regulating the LIG climate." Please see our reply below and the revised manuscript for further details on the modifications.

Modifications in the revised manuscript are highlighted in blue.

Anonymous Referee 1

Referee 1 This paper compiles existing records from the North Atlantic and Southern Ocean for the last interglaciation (LIG). The authors compare this compilation with existing climate model simulations for 126, 122 and 115 ka to test if insolation alone can explain a delay in LIG warmth in the North Atlantic. They conclude that persistent ice-sheet melting likely slowed ocean circulation until 126 ka, resulting in delayed LIG warmth relative to boreal summer insolation. In general, I think this paper represents a nice review of previous work, placing the observations of delayed LIG warmth in context of one set of model simulations. The authors do gloss over certain problems, however, with their data-model comparison and do not compare their results with other model simulations that could lead to different conclusions. I think the paper is fine for publication once the discussion is increased and justification for several of the approaches is further provided.

Authors Thank you.

Referee 1 The authors should consider the climate simulations of Felis et al. (2004, Nature), who showed a spatially variable North Atlantic response to peak LIG insolation. Their simulation would explain much of the climate pattern observed by the authors without the meltwater forcing. Kaspar and Cusbasch (2007) also simulated a somewhat similar pattern, again without the need of freshwater forcing. Thus, I think the authors need to weaken their conclusions that the delayed warmth is caused by remnant ice-sheet melting as they have only used one model and other models show a similar climate map without needing meltwater.

Authors We followed the referee's comment and added a new section 4.3.4 in the discussion on the limitations of model-data comparison to discuss alternative mechanisms to the early LIG climatic pattern in the view of other existing modelling studies. We agree that Felis et al. (2004) and Kaspar et al. (2007) simulated a similar North Atlantic cooling at the beginning of the LIG without freshwater input. It is attributed in their study to a high positive NAO index in response to insolation changes. We discuss this alternative explanation in the new section 4.3.4. Although the presence of IRD in North Atlantic and Norwegian Sea cores argues in favour of our hypothesis (ice sheet melting during the early LIG), we weakened our conclusions (modified at the end of the discussion, of the conclusions and the abstract). We finally highlight the need of further investigations with a variety of models to disentangle the climatic forcing and mechanisms regulating the LIG climate (see the last paragraph of the discussion and the conclusions).

Referee 1 The authors need to justify their approach for calculating $\delta^{18}\mathrm{O}_{sw}$ using SST effects on a deeper dwelling foraminifera test. The common means of $\delta^{18}\mathrm{O}_{sw}$ calculation is to use the calcification temperature determined from Mg/Ca to remove temp effects on test d18O. The authors here are using transfer function SST, which does not have to reflect the calcification temperature of the test. Indeed, why is the $\delta^{18}\mathrm{O}_{sw}$ of site 980 increasing while cores to the south are depleted? If this is to be from remnant ice melting, I would expect the $\delta^{18}\mathrm{O}_{sw}$ signal to be more depleted further north closer to the remnant ice sheets. Also, where does the CH69-K09 $\delta^{18}\mathrm{O}_{sw}$ depletion come from at ~127 ka? IRD is ~0 in the core at that point and the core is in the middle of the North Atlantic making me wonder about such a large $\delta^{18}\mathrm{O}_{sw}$ change not seen elsewhere and the applicability of the faunal SST to calculating $\delta^{18}\mathrm{O}_{sw}$. The authors should show the raw δ18O from these cores so the reader can see what is a $\delta^{18}\mathrm{O}_{sw}$ change that is in the raw δ18O record versus one that is based on the assumption that the SST corresponds to the calcification temp.

Authors We agree with the referee that calcification temperatures derived from Mg/Ca measurements are now commonly used to reconstruct $\delta^{18}\mathrm{O}_{sw}$ variations. However, transfer function SST (which were originally and are still used for $\delta^{18}\mathrm{O}_{sw}$ calculations) also allow accounting for the calcification depth of the planktic species (Duplessy et al., 1991; Chapman et al., 2000). In the revised manuscript, we now fully explain the $\delta^{18}\mathrm{O}_{sw}$ calculations that we had already performed but described too briefly in the initial

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submission. The new Table 2 provides details on the calculation performed. Using the averaged coretop (0-3.5 ka) planktic δ18O values and modern in situ δ^{18}O$_{sw}$ value at the sites, we determined the modern calcification temperature of *G. bulloides* and *N. pachyderma* dextral. We hence calculated the deviation between this calcification temperature and the summer SST estimate (Tiso = SST – 2 for *G. bulloides* and Tiso = SST – 1.5 for *N. pachyderma* dextral). The deviation is identical to the most recent calibration for *G. bulloides* (Chapman et al., 2000). To our knowledge, no such study exists for *N. pachyderma* dextral. Using these equations, we corrected the available summer SST estimates to reconstruct past δ^{18}O$_{sw}$ variations in cores CH69-K09 and ODP 980. Our δ^{18}O$_{sw}$ calculations from *G. bulloides* and *N. pachyderma* dextral have hence not been changed. They are now fully described in the revised manuscript. Given the lack of suitable in situ δ^{18}O$_{sw}$ values at CH69-K09's site and at the depth range where *N. pachyderma* sinistral lives (50-200 m), we removed from Figure 5 (new Figure 7) the CH69-K09 δ^{18}O$_{sw}$ curve obtained from *N. pachyderma* sinistral. This action does not affect our results and interpretation.

In the initial submission, the δ^{18}O$_{sw}$ records were plotted in Figure 5 with respect to the modern δ^{18}O$_{sw}$ values (0.38 ‰ for core ODP 980 and 0.75 ‰ for core CH69-K09). We realized that this approach was misleading because it could imply that δ^{18}O$_{sw}$ values were lower at site CH69-K09 than ODP 980 (which was not necessarily true). This is now corrected in Figure 7 (former Figure 5) of the revised manuscript where we do not present anomalies anymore. Therefore the δ^{18}O$_{sw}$ depletion highlighted by the referee at 127 ka in core CH69-K09 has a much smaller amplitude (0.6 ‰ than what the former figure could imply (>1.1 ‰. First, this amplitude is close to the δ^{18}O$_{sw}$ uncertainty (± 0.5 ‰ now mentioned in the methods section). Second, the SST and δ^{18}O$_{sw}$ variability is much higher (see new figure 2) in core CH69-K09, which is located at the boundary between the North Atlantic and Labrador currents (Labeyrie et al., 1999) than in core ODP 980 located along the pathway of the North Atlantic Current (Oppo et al., 2006). Stronger input of fresher Labrador Sea water (versus a reduced contribution of North Atlantic saline waters) in response to the high northern latitude cooling could contribute

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to the $\delta^{18}O_{sw}$ depletion recorded in core CH69-K09 at 127 ka.

Finally, as suggested by the referee, we added a new figure (figure 2 in the revised manuscript) that presents the raw planktic δ18O records together with the SST and seawater δ18O records for both cores ODP 980 and CH69-K09.

Referee 1 Where do the uncertainties on the core chronology come from? More justification is needed to explain the core chronologies if they are to be really +/-2.2 ka for a period with only two tie points used to make the age model. The authors subsequently rarely discuss the uncertainty in their interpretations, even using dates at 100's of years accuracy. This should be dampened given the uncertainties (and what I think are overly optimistic based on the lack of justification) in the age model.

Authors Table 3 details how we calculated the age uncertainty for each tie-point of the six sediment cores. For higher clarity for the readers, we now mention at the end of sections 3.1.1 and 3.1.2 (definition of age models) the individual age uncertainty of Southern Ocean ($\pm$ 1.3 ka, 1σ), North Atlantic ($\pm$ 2.2 ka, 1σ) and Labrador/Norwegian Sea ($\pm$ 2.3/2.5 ka, 1σ) records on EDC3 time scales. We also specify that these uncertainties integrate (1) the resolution of correlated records in marine sediment cores, (2) the resolution of ice core reference records, (3) a matching uncertainty graphically estimated when defining the tie-points and (4) for northern hemisphere records only, the relative uncertainty on ice core chronologies (i.e. on the transfer of NGRIP record on EDC3 time scale or on the gas-ice age difference in EDC ice core). We explain that combined age uncertainty needs to be considered when comparing Southern Ocean to North Atlantic records. It reaches at the most 2.6 ka (1σ) over the period 130-115 ka. The same is explained at the end of section 3.1.2 for the comparison of Norwegian/Labrador Sea records to North Atlantic ones (at the most 3.1 ka, 1σ). This detailed approach allows us to provide well-justified and realistic age uncertainties.

Please note that, following the reorganisation of the definition of the chronology of the Labrador/Norwegian Sea cores (see reply to the second referee), we reconsidered in

Table 3 the errors of the tie-points defined in these cores. They remain very similar and do not affect our results. Finally, we rounded up throughout the revised text the dates given at 100's of years accuracy, as highlighted by the referee. We added the age uncertainties in our interpretation of the data (section 3.2, when relevant).

Referee 1 On the origin of the freshwater, the authors need to discuss their options beyond arm waving at some remnant ice melting somewhere. With ∼20 m of sea-level rise to go between 130-126 ka according to their line 26 on 3459, that's ∼0.06 Sv, much less than the 0.17 Sv they have in the 126 ka simulation. Has their model been run using just 0.06 Sv? Does it match the data? Greenland ice retreat would only be a smaller forcing, <0.01 Sv based on the Colville et al. (2011, Science) results that show that ice persisted on southern Greenland through the LIG, consistent with the lower end of of the Otto-Bliesner et al. simulations, or only ∼2.2 m of sea level rise through the LIG from Greenland. The authors should also include reference to "small" Greenland retreat suggested by NGRIP (2004) and Willerslev et al. (2007, Science) on line 20 of page 3260.

Authors First, it is not possible at this stage of the study to run the model with a 0.06 Sv freshwater flux on such a long time frame (130-126 ka) and compare the climatic response to proxy data. The value of 0.06 Sv is obtained by assuming a regular ice sheet melting. In contrast, our 0.17 Sv can be interpreted as a melting pulse over a small time frame, although there is no observational evidence of such a pulse. Please find further details on realistic estimates of the meltwater flux at the beginning of the LIG in our reply to the second referee (last specific comment).

Second, we thank the referee for drawing our attention of the recent paper by Colville et al. (2011). We followed the referee's comment to strengthen our section 4.3.1 on the origin of the meltwater. We now indicate that "the contribution of Greenland melting to the LIG sea level highstand has been reevaluated to 1.6 m to 2.2 m (compared to modern times) (Colville et al., 2011)". This estimate suggests "a limited contribution of the Greenland retreat to the meltwater input at the beginning of the LIG". "A significant

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proportion of icebergs melting in the North Atlantic hence originate from remnants of glacial northern ice sheets. The Eurasian Saalian ice sheet constitutes a very likely source for the icebergs. The influence of the melting Laurentide ice sheet can not be excluded". We finally conclude that "evaluating the respective contribution of northern ice sheets to the early LIG meltwater input would constitute an interesting challenge for future climate modeling studies". Please see the revised manuscript for specific modifications in the text.

Third, we added the reference to North Greenland Ice Core Project members (2004) and Willerslev et al. (2007) in section 4.3.2 (Magnitude of the computed freshwater flux and North Atlantic response), as suggested by the referee.

Referee 1 What about an elevated hydrologic cycle with warmer wetter Arctic supplying more freshwater?

Authors We now discuss this point as part of the new discussion section 4.3.4 ("Alternative mechanisms to the early LIG climatic pattern?"). We first highlight that "past changes in precipitation are difficult to estimate from proxy data. Qualitative estimates suggest wetter conditions in most Arctic sectors during the LIG compared to the Holocene". Second, we now indicate that "the response of the Arctic freshwater budget to the sole insolation forcing differs in modelling studies". Depending on the studies and the models, the Arctic freshwater budget can be increased or reduced during the early LIG. Finally, the influence of changes in the Arctic freshwater budget on the AMOC differs in modelling studies (direct impact, overprint by surface-water warming or sea ice-related processes). Please see the revised manuscript for further details and associated references. We conclude that "the impact of insolation changes on the AMOC remains under debate. Mechanisms with opposite effect on the AMOC come into play. Depending on their relative magnitude in the models, these mechanisms can lead to different AMOC responses, as already illustrated in future climate projections (Gregory et al., 2005)."

Referee 1 Can the authors add in a comparison of when the peak Holocene temps were reached in these cores when radiocarbon chronologies can be applied? If their mechanism of melting lingering ice sheets is correct for the LIG, wouldn't then the peak of the Holocene be reached at the end of the last deglaciation ~7 ka in these some cores? I think such a comparison could supply further support for their hypothesis.

Authors Among the six marine sediment cores selected here to investigate the LIG climate evolution: (1) two of them (Southern Ocean core MD02-2488 and North Atlantic core MD95-2042) do not present any 14C dates; (2) two of them (Labrador Sea core JPC2 and Norwegian Sea core MD95-2010) only cover the very early Holocene and do not provide any climate constraints after 12 ka; (3) one of them (North Atlantic core ODP 980) do not present 14C dates before 12 ka (i.e. no well-dated Termination I); and (4) only one of them (North Atlantic core CH69-K09) is suitable and sufficiently well-dated for the Holocene-LIG comparison suggested by the referee. Therefore such a comparison would require adding at least 3-4 new marine sediment cores to the study. This would significantly increase the length of the manuscript (already substantial).

In addition, the orbital configurations and insolation values were different at the beginning of the LIG (obliquity maximum preceding the precession minimum, very high boreal summer insolation) and of the Holocene (precession minimum slightly preceding the obliquity maximum, relatively lower boreal summer insolation) (see Masson-Delmotte et al., 2010 for a comparison). These different orbital configurations resulted in different deglacial histories during Termination I and II (e.g. no thermal reversal is observed during Termination II, in contrast to Termination I, Carlson, 2008). Given the different orbital configurations and deglacial histories, we are hence not convinced that (1) similar thermal patterns at the beginning of the LIG and Holocene would provide further support to our assumpation, or that (2) different thermal patterns would dismiss our hypothesis.

Therefore, a comparison of Holocene and LIG thermal evolution would significantly increase the length of the manuscript (already quite long) and dilute the main message of our data-modelling study. We believe that such an interesting but complex topic should rather be the objective of a specific study.

Anonymous Referee 2

General comments Referee 2 This is a well-written paper, and a nicely designed study to evaluate the potential influence of sustained meltwater delaying the ocean thermal maximum in the high North Atlantic during the Last Interglacial period. If correct, the evidence for the concept that increased melting in the early Eemian (relative to the early Holocene) suppressed AMOC and ocean temperatures would be an important contribution our understanding of the evolution of the Last Interglacial. I do have concerns about the sediment core age models, especially for those in the Labrador and Norwegian Seas, which are critical to the thesis of the paper. Providing that the authors can satisfy my concerns, primarily about the LIG age models of the marine sediment cores, I recommend that this paper be published.

Authors Thank you.

Specific Comments Referee 2 My primary concern regards age model development. First, for all of the Northern Hemisphere cores, I think that authors' assumption that sea surface temperatures ought to be synchronous with global methane concentration needs a bit more investigation. Global methane concentration is influenced by the extent and wetness of boreal forests, which clearly are at their maximum during interglacials, but it is also related to tropical hydrology and circulation among other phenomena, so it's not self-evident that North Atlantic SSTs and global CH4 should be synchronous during the LIG. It might be a fair assumption, but I think it needs more justification than the observation that they appear to have been synchronous during the early Holocene.

Authors We agree with the referee that changes in methane concentrations are strongly influenced by the strength of tropical methane sources and sinks related to tropical hydrology and circulation (e.g. Loulergue et al., 2008). We do not mean that past methane variations are only linked to changing CH4 emissions from Northern Hemisphere periglacial wetlands and hence always reflected in North Atlantic SST changes. Air temperature and methane records from the Greenland ice cores indicate that abrupt Greenland warmings during the last glacial period (Dansgaard-Oeschger warmings) and Termination I are in phase with sharp methane increases (e.g. Chappellaz et al., 1993; Severinghaus and Brook, 1999; Flückiger et al., 2004; Huber et al., 2006). In our study, we assume that similarly, the abrupt methane increase at the end of Termination II is in phase with the deglacial warming recorded at high northern latitudes. This hypothesis is supported by the Antarctic methane record, which indicates major methane emissions by boreal wetlands induced by northern ice sheet retreats during the terminations of the last 800 ka (Loulergue et al., 2008). We do not mean that global methane and North Atlantic SST are synchronous throughout and at the end of the LIG, only at the end of Termination II. This point is now clearly explained in section 3.1.1 (definition of age models in the North Atlantic) of the revised manuscript.

Referee 2 Secondly, and most importantly, the authors need to better justify the choice of tie points for the Labrador Sea and Norwegian Sea cores, since the choice of tie points here is directly responsible for when the peak warmth appears to occur. It seems to me that the small, single-point peaks in SST around 128 ka, could easily be interpreted to match the small rises in CH4 that occurred between 133 and 129 ka, as they were in the “North Atlantic” cores (Fig. 3). Likewise, the peak warmth that occurs around 125 ka, could be assigned to the peak CH4 concentrations around 128 ka. I recognize that there is more evidence than just the wiggle-matching between CH4 and inferred temperatures, such as the onset of the diatom-rich sediments which were also observed in the Holocene, but the choice of tie-points, and the alternative chronology I suggested, need to be more fully examined and discussed, because if the alternative points are correct, the entire premise of the paper (late peak SSTs) falls apart.

Authors We agree with the referee that the definition of tie-points in the Labrador Sea and Norwegian Sea cores is delicate and constitutes an important part of our interpretation. Three additional lines of evidence support our choice of tie-points in these cores.

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(1) Percentages of *N. pachyderma* sinistral (NPS) do not record SST variations below 6.5°C (see the calibration of % NPS in Figure 1 and our reply to the next comment of the referee below). Foraminiferal SST reconstructions from the Last Glacial Maximum (LGM) indicate that the temperature of surface waters was below 3°C in the Nordic Seas and western North Atlantic (Meland et al., 2005; MARGO Project Members, 2009). There is so far no such study for Marine Isotope Stage (MIS) 6 preceding the LIG. However, similar sea levels during the LGM and MIS 6 (Waelbroeck et al., 2002) and the presence of a large Fennoscandinavian ice sheet during MIS 6 (Svendsen et al., 2004) suggest that surface waters in the Nordic Seas and the Labrador Sea were at least as cold (below 3°C) during MIS 6 as during the LGM. This result suggests that a surface-water warming (of at least 3°C) may have occurred in the Norwegian Sea and the Labrador Sea before the warming indicated by the main decrease in the percentages of *N. pachyderma*. Therefore, the deglacial warming in these regions probably started earlier than indicated by the large decrease in *N. pachyderma* percentages, i.e. together with the abrupt methane increase (Figure 7). This is consistent with our assumption of synchronous high northern latitude warming and methane increase during Termination II. Our results nevertheless show that peak interglacial conditions were reached during the late LIG in the Labrador Sea and the Nordic Seas.

(2) Rasmussen et al. (2003b) compared the thermal evolution of surface waters in the North Atlantic and the Norwegian Sea using two marine sediment cores. The LIG chronology is strongly constrained by the identification of a same ash layer (at ∼127 ka) in the two cores north and south of Iceland. Thanks to this robust tie-point, the authors document a late LIG warming in the Norwegian that lagged by 3-4 ka the deglacial warming in the North Atlantic. Unfortunately, the very poor resolution of planktic δ18O data and the absence of benthic δ18O data during the LIG in the North Atlantic core (ENAM 33) prevent us from adding this core to our compilation and from gaining further temporal constraints on the timing of the deglacial warming in the Norwegian Sea with respect to the North Atlantic. Nevertheless, the study by Rasmussen et al. (2003b) supports with a robust tephra tie-point the late establishment

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of peak interglacial conditions in the Nordic Seas that we document in our manuscript.

(3) The referee suggests an alternative chronology to the Norwegian Sea and Labrador Sea cores. The alternative tie-points consist in shifting the deglacial warming in the Norwegian Sea and Labrador Sea a few thousand years earlier, which would lead to peak warmth around 128 ka. We dismiss this alternative chronology for the two following reasons. (a) By doing so, the deglacial warming would start very early (132-133 ka) in the Norwegian Sea and Labrador Sea, even earlier than in the North Atlantic. This result is in contradiction with the robust late warming documented by Rasmussen et al. (2003b) in the Norwegian Sea (see point 2 above) and with the occurrence of the diatom mat in the Labrador Sea when interglacial conditions are being established (Rasmussen et al., 2003a). (b) In addition, the LIG benthic δ18O plateau would start very early, at around 132 ka in the Norwegian/ Labrador Sea. This timing disagrees with the evolution of North Atlantic benthic δ18O records whose plateau starts at 130 ka. At 132 ka, North Atlantic benthic δ18O values are 1.2 ‰ higher than during the LIG plateau. Although this δ18O difference integrates a deep-water temperature component, it implies that sea level significantly increased (several dozen meters) between 132 and 130 ka. In the alternative chronology, the beginning of the benthic δ18O plateau at 132 ka in the Norwegian/Labrador Sea would require a very large amount of meltwater to compensate for the remaining sea level increase. IRD data indicate that the freshwater input was already significantly reduced at the beginning of the benthic δ18O plateau in the Norwegian Sea. Sea level reconstructions also indicate a late LIG highstand (e.g. Waelbroeck et al., 2008; Blanchon et al., 2009). Altogether, these arguments are in favour of our Norwegian/Labrador Sea chronologies and make the alternative tie-points proposed by the referee very unlikely.

We reorganized and developed the definition of age models of the Labrador Sea and Norwegian Sea cores by integrating these additional pieces of evidence in the revised manuscript.

Referee 2 The description of how *N. pachyderma* percentages were calibrated to, and

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converted into temperature on page 3245, lines 8-14, is confusing, and needs to be clarified. Furthermore, I'd like to see a more thorough analysis of the calibration; specifically, a full error analysis, so that error bars could be included with the SST reconstructions. Furthermore, it would be good to see how the choice of where to truncate the calibration set (e.g., rather than creating a regression using 10-94%, one could argue that 15-83% would result in a more robust relationship) would affect the inferred SSTs.

Authors In the methods section of the revised manuscript, we clarified and detailed the calibration of *N. pachyderma* percentages in terms of summer SST (used MARGO database from the North Atlantic, averaged July-August-September at 10 m water-depth from World Ocean Atlas 2001). We also specify the total uncertainty on reconstructed SST (1.9°C), which combines the uncertainty on the calibration (1.8°C) and the uncertainty on *N. pachyderma* percentages (< 0.7°C). Finally, modifying the calibration interval (e.g. 10-94 % or 13-84 %) produces similar linear relationships (slope and intercept values within uncertainties). The change in reconstructed SST ranges from 0.1°C to 0.7°C for respectively high and low percentages of *N. pachyderma*. The effect of the calibration interval on reconstructed SST hence remains small. This is now mentioned in the revised text. Despite the uncertainties, the calibration gives a reliable range of temperature change in the Norwegian and Labrador Seas.

Referee 2 Lastly, the authors were clear in stating that a freshwater flux of 0.17 Sv is too high to be considered realistic over for the whole early LIG, and so the freshwater-melting simulation is to be considered an upper limit estimate. That said, it would have been really interesting to see how a more realistic estimate of the flux, even if not included dynamically in the model, would be simulated in the model, and whether it could potentially explain the suppressed warming in the North Atlantic. I realize that it's likely impossible to conduct such an experiment at this point, but it also means that the question of whether a realistic amount of meltwater could, by itself, drive the reduced AMOC and cooler early-LIG temperatures.

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Authors The referee is right. It is not possible at this stage of the study to perform sensitivity experiments to determine the climatic response to more "realistic" freshwater inputs. However, we added at the end of section 4.3.2 (Magnitude of the computed freshwater flux and North Atlantic response) a short discussion on the study by Bakker et al. (2011). These authors "investigated the range of Greenland melting rate at the beginning of the LIG using an ensemble of sensitivity experiments with a model of intermediate complexity. They indicate a possible range between 0.052 and 0.13 Sv of the meltwater flux that resulted in decreased deep convection and reduced air temperatures over the North Atlantic and the Labrador Sea (Bakker et al., 2011). This study confirms the extremely high freshwater flux computed in our study. It also shows with "more realistic" freshwater values the similar impact of northern ice sheet melting on North Atlantic climate that we report at the beginning of the LIG."

We also estimated the range of freshwater flux suggested by the proxy data. We added to the discussion (section 4.3.2): "In addition, relatively stable North Atlantic benthic δ18O data during the early LIG (Figure 8c) suggest sea level fluctuations smaller than 20 m. Assuming a sea level rise of maximum 20 m during the 3-4 ka of persistent IRD deposition at the beginning of the LIG implies a mean freshwater flux of 0.06 to 0.08 Sv (that was probably irregular and higher at 130 ka than 126 ka). Although highly uncertain, these values fall within the range of meltwater flux inferred by Bakker et al. (2011)." Finally, we clarified our last sentence of this section on future work: "Nevertheless, all these elements highlight the critical need in the near future to couple ice sheet and climate models in order to improve the computation of freshwater fluxes and realistically estimate their impact on North Atlantic climate."

Technical Corrections Referee 2 3244, line 15: Sea Surface Temperature and Ice-Rafted Debris should not be capitalized.

Authors It has been modified.

Referee 2 3244, line 20: replace "1,95" with "1.95"

Authors Done.

Referee 2 3245, 15-19: Consider replacing or supplementing this discussion with the explicit equation used to calculate $\delta^{18}\mathrm{O}_{sw}$.

Authors We added the explicit equation used for the $\delta^{18}\mathrm{O}_{sw}$ calculations in the methods section.

Referee 2 3248, 20: replace "at midpoint" with "at the midpoint"

Authors It has been modified.

Referee 2 3256, line 27: the second point begins "A mechanism involves". Please be explicit about what this mechanism is, or otherwise clarify this sentence.

Authors We reformulated the sentence as "The mechanism identified here brings the export of Arctic sea ice towards the Nordic Seas (where sea ice melts) into play."

Referee 2 Figures 5 and 6, in all of the other figures, the figures are labeled with a,b,c. . ., descending from the top, but in these figures they letters begin at the bottom and go up. This should be made consistent between figures, preferably descending.

Authors Figures 5 and 6 (new figures 7 and 8) are now labelled from top to bottom for improved consistency. The cross references have been modified accordingly.

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Anonymous Referee 3

Referee 3 This is a very well written and structured paper which examines the timing of peak Last Interglacial (LIG) climate conditions in the North and Southern Hemispheres, with an emphasis on high northern latitudes. They find from high resolution sediment records there is a delay in peak climatic conditions in the northern latitudes compared with southern latitudes attributed to persistent iceberg melting at the beginning of the LIG. As a result they infer weaker North Atlantic overturning circulation during the early LIG compared with the late LIG. Comparison with their model results, however, shows that insolation changes alone cannot explain this weakening and that in addition freshwater input is required. The results presented here provide an insightful comparison between model and data for the LIG and emphasises the need for climate - ice-sheet modelling in order to understand the peak climatic conditions of the LIG. As such, this manuscript addresses relevant scientific questions within the scope of CP.

Authors Thank you.

Specific Comments:

Referee 3 Below are specific comments/questions that should be addressed, however, before publication: 1. There is virtually no description of the model used. A few sentence describing components, resolution etc would be useful. Furthermore, the performance of the General Circulation Model for modern day is not discussed at all in the model simulation description in section 2.2 and would be beneficial to the reader in order to put the LIG results into context.

Authors In the methods section 2.2, we added a few sentences describing the atmospheric, oceanic, sea ice and continental components and the resolution of the IPSL-CM4 model. The readers are referred to Marti et al. (2010) for a detailed description of the components and the coupling methodology. We also briefly summarized the performance and main biases of the IPSL-CM4 model under preindustrial conditions in the North Atlantic (with references to more detailed descriptions).

Referee 3 2. A comment regarding the ocean model spin-up would be useful. The authors describe the length of the simulations accordingly as 250 yrs (126ka), 800 yrs (122ka) and 700 years (115ka). Is the ocean fully spun-up in the 126ka simulation? It is possible that you may not be making a fair comparison between the 126ka and 122ka simulations. Also, please state how long the 126ka simulation with a melt-water pulse included was run for. This is important because previous work with coupled climate ice-sheet models (e.g. Ridley et al., 2005) have shown that although freshwater input under a warm climate can lead to a decline in the overturning circulation they showed recovery after a few hundred years.

Authors The shortest simulation at 126 ka (250 years) presents a stable AMOC over the last 150 years of the simulation. This feature indicates that AMOC has reached a quasi equilibrium at the end of the 126 ka simulation and makes possible the comparison of the three experiments. This is now mentioned in the methods section 2.2 of the revised manuscript. We added a new Figure 3 showing the temporal evolution of the AMOC maximum for the 4 simulations considered here and the preindustrial one.

The 126 ka simulation with computed meltwater was also run for 250 years (added in the revised version). At the end of the 250 years, the AMOC weakened by 6 Sv and no stable AMOC state was reached (new Figure 3). The recovery of the AMOC simulated by Ridley et al. (2005) occurs after 400 years of simulations, when the additional freshwater input (reaching at most 0.06 Sv) from ice sheet melting was almost negligible. We believe that this recovery is mainly related to the diminution of the freshwater input. In our study, the melting does not diminish at the end of the simulation, which is a weakness of our experiment design. Therefore, despite the short length of our “126 ka meltwater” simulation, the large AMOC reduction computed here makes such an AMOC recovery very unlikely in our study. It never happened with this model in other interglacial contexts such as the early Holocene or the mid-Holocene (Braconnot et al.), for which longer simulations (750 years) with freshwater input are available.

We added a paragraph in section 4.3.2 (discussion on the Magnitude of the computed

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freshwater flux and North Atlantic response) to clarify this point. We now wrote in the revised manuscript: “Previous work on future climate with coupled climate ice sheet models (e.g. Ridley et al., 2005) indicates (1) the melting of the Greenland ice sheet (additional freshwater input reaching at most 0.06 Sv) under warm conditions, (2) a small AMOC weakening (1-2 Sv, i.e. < 10 % of the simulation without ice sheet melting) caused by the meltwater input and (3) an AMOC recovery ∼400 years later, when most of the additional freshwater input ceases. In our study, the freshwater input is still present at the end of the “126 ka meltwater” simulation (250-year long) and stabilized around 0.17 Sv. This feature explains the large AMOC reduction at that time (6 Sv, i.e. 50 % of the AMOC overturning at 126 ka, Figure 3). The long-term evolution of the AMOC hence remains uncertain in our study. Future work should include longer simulations of the LIG climate with interactive ice sheets in order to determine the long-term impact of ice sheet melting on the AMOC.”

Referee 3 3. The authors are correct to state that the freshwater input is idealised and not realistic and that coupled climate - ice-sheet simulations are required to ‘realistically’ model this effect. However, they do not discuss in any detail what other mechanisms could affect the discrepancy between model and data. For example, more detail on the model sensitivity to sea-ice changes would be advantageous including model dependency. Another example could be changes to precipitation patterns that would occur over a smaller Greenland ice-sheet (e.g. as proposed by Otto-Bliesner et al., 2006). This would change the atmospheric freshwater input and hence potentially affect the overturning circulation.

Authors We added a new section 4.3.4 to discuss alternative mechanisms that could induce the early LIG climatic pattern that we identify, in the context of other existing modelling simulations. In this section, we discuss the (1) the mechanisms (high positive NAO index) leading to a cold North Atlantic at the beginning of the LIG (Felis et al., 2004; Kaspar and Cubasch, 2007), (2) the mechanisms (increased Arctic freshwater budget, warming effect of North Atlantic surface waters on their density) responsible for

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a weakened AMOC simulated during the early LIG (Khodri et al., 2003; Gröger et al., 2007), and (3) the influence of changes in the Arctic freshwater budget on the AMOC and North Atlantic climate. Please note that, despite a careful reading of the article by Otto-Bliesner et al. (2006), we were not able to identify in this paper the changes in precipitation patterns over a smaller Greenland ice-sheet that are mentioned by the referee. Instead, we considered the work by Gröger et al. (2007) and Khodri et al. (2003).

In this new section, we also highlight that the sea ice processes identified in our study are not specific to the IPSL-CM4 model. Similar mechanisms have been reported by Hu et al. (2004) to explain the future AMOC evolution simulated with the Parallel Climate Model. However, these mechanisms do not come into play in the AMOC weakening simulated by Gröger et al. (2007) during the early LIG. Please see the revised manuscript for further details. We conclude at the end of this new section: “In summary, our simulations are so far the only ones that reproduce both the cooling/freshening in high northern latitudes and AMOC weakening documented during the early LIG by proxy data. This characteristic tends to support our assumption of persistent ice sheet melting at the beginning of the LIG. However, other models are able to simulate part of the LIG climatic pattern (North Atlantic cooling or AMOC reduction) identified here without freshwater input. The variety of mechanisms involved (e.g. high NAO index, Arctic freshwater budget, temperature effect on density) leaves the question open to alternative explanations. Further investigation with different models is clearly required to disentangle the climatic forcing and mechanisms regulating the LIG climate.”

Referee 3 It would also have been interesting to perform sensitivity studies to different more ‘realistic’ freshwater inputs. Although I realise this is not possible I think that it warrants mentioning at least as future work.

Authors Please see here our reply to the last specific comment of the second referee.

Referee 3 4. It is also important to remember that this comparison with data only in-

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volves one model simulation and therefore weakens the conclusions to an extent. It would be very beneficial in the discussions and conclusions section to discuss these model results in the context of some of the previous studies that already exist. For example, comparisons could be made with Gröger et al. (2007, Palaeoceanography) who found a weakening of the overturning circulation at 126ka with only atmospheric freshwater inputs required. Furthermore, the sensitivity of different models to overturning circulation changes observed for future climate simulations should also be considered as outlined in the IPCC (2007) report.

Authors Following the referee's comment, we weakened our conclusions and modified the text at the end of the discussion (see two comments above), of the conclusions (see hereafter) and the abstract (please see the revised manuscript). We now write at the end of the conclusions section: "Our model-data comparison also reveals the limits of our set of simulations performed with one ocean-atmosphere coupled model to reproduce the LIG climate evolution. The freshwater flux computed in this study is extremely high and cannot represent the real climate of the early LIG. In addition, other models can reproduce part of the climatic pattern (North Atlantic cooling or AMOC reduction) that we identify here at the beginning of the LIG without freshwater input. They bring a variety of processes into play (e.g. Arctic freshwater budget, NAO, temperature/salinity effect on density). These results highlight the critical need in the near future to (1) develop ice sheet-climate coupled models to improve the computation of freshwater fluxes and realistically simulate the magnitude of freshwater input and climatic responses, (2) perform transient experiments lasting few thousand years to investigate the long-term impact of ice sheet melting on the AMOC and North Atlantic climate, and (3) compare simulations performed with a variety of models to evaluate the climatic forcing and mechanisms that are most likely to influence the LIG climate."

In the new discussion section 4.3.4, we discuss our results in the context of the few existing modelling studies investigating the LIG climate evolution (Khodri et al., 2003; Gröger et al., 2007). Please see our reply to the third specific comment of Referee

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3 for an overview of the other mechanisms that we discuss in this new section. Note that the AMOC weakening simulated by Gröger et al. (2007) at the beginning of the LIG is mainly induced by warmer North Atlantic surface waters (temperature effect on density) and not by atmospheric freshwater input (“In contrast to most other studies the weakening THC in the early Eemian in our study is mainly due to the increased SSTs.”, end of section 4.1, page 12 of their manuscript). We however discuss in the new section 4.3.4 the influence of changes in the Arctic freshwater budget on the AMOC using the results of two studies (Khodri et al., 2003; Gröger et al., 2007).

Finally, as proposed by the referee, we mention in section 4.3.4: “Few modelling studies investigated the climate evolution throughout the LIG (e.g. Crucifix and Loutre, 2002; Khodri et al., 2003; Calov et al., 2005; Gröger et al., 2007). At the beginning of the LIG, they simulate an enhanced (this study, Crucifix and Loutre, 2002; Calov et al., 2005) or weakened AMOC (Khodri et al., 2003; Gröger et al., 2007) in response to insolation variations. This feature highlights the difference in overturning sensitivity in the models during the LIG, as already outlined for future climate simulations (Schmittner et al., 2005; Schneider et al., 2007).”

Technical Comments:

Referee 3 Abstract When mentioning the 126ka melt-water pulse simulation, please state this was an idealised simulation looking at an extreme scenario of freshwater input.

Authors This is now mentioned in the abstract.

Referee 3 Introduction P3242, line 25-28: please rephrase this sentence since it is difficult to follow.

Authors The sentence has been rephrased as: “This weakened LIG warmth in comparison to the early Holocene suggests that the temperature evolution in the Nordic Seas does not solely respond to insolation variations during the LIG”.

Referee 3 P3243, line 1: Please give the dates for the Late Saalian glacial periods in terms of thousands of years as well.

Authors The interval of the Late Saalian glacial period (160-140 ka) has been added.

Referee 3 Model simulations P3246, first paragraph: Please mention the preindustrial greenhouse gas concentrations for reproducibility.

Authors We modified Table 3 to include the eccentricity/obliquity/precession values, as well as the preindustrial values of greenhouse gas concentrations (280 ppmv for CO2, 270 ppbv for N2O and 650 ppbv for CH4) for higher clarity (and reproducibility).

Referee 3 Model data comparison P3256, line 29: Insert “the” before Fram Strait

Authors Done.

Referee 3 Figures: Figs 5 and 6. The shaded regions are not really red so perhaps change to orange. Figs 3 and 4 have the labels (a) to (d) going from top to bottom on the panels while Figs 5 and 6 have them going from bottom to top. Please keep this consistent.

Authors The colour of the shading has been changed to orange. Figures 5 and 6 (new figures 7 and 8) are now labelled from top to bottom. The cross references have been modified accordingly.

Interactive comment on Clim. Past Discuss., 7, 3239, 2011.