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Interactive comment on "Simulating last interglacial climate with NorESM: role of insolation and greenhouse gases in the timing of peak warmth" *by* P. M. Langebroek and K. H. Nisancioglu

Anonymous Referee #2

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The authors present results from LIG (Last Interglaciation) time slice simulations using the "Norwegian" Earth System Model NorESM which is identical to NCAR's CCSM4 except for the ocean component which has been replaced by the Miami Isopycnic Coordinate Ocean Model. By and large, the experiments are certainly interesting and deserve publication. However, in its present form the analysis of results is very superficial only. In-depth analyses of physical processes to explain the observed temperature trends are missing. Moreover, model-data comparison is based on visual inspection only and hence statements like "The simulations capture the general trend of last inter-

glacial summer SSTs, as shown by four sediment cores in the North Atlantic" (p.4460) seem to be unjustified. I therefore suggest a major revision of the manuscript taking into account the following points.

Specific comments:

1) The authors conclude that "the simulated summer temperatures follow the general pattern of the sediment core data" (p.4457, l.5). This conclusion is based on visual inspection alone (Fig. 4) and difficult to follow. It is unclear how the authors define "general pattern". The SST record of core MD95-2010 shows a warming of about 3 K from 130 ka to 125 ka and a subsequent cooling of the same magnitude until 115 ka. The model shows a warming of only 1 K from 130 ka to 125 ka, resulting in a temperature bias of about 3 K around at 125 ka. The model-data disagreement is even worse for core ODP 980 at 130 ka, where the model has a cold bias of almost 4 K at 125 ka. For core EW9302-JPC2 the model fails at simulating the dramatic temperature increase around 125 ka. The model-data comparison should be more quantitative. Which trends are simulated, which are not? Does the model capture the reconstructed temperature variance? For which time slice is the model-data mismatch best/worst? Statistical parameters should be used but this would probably require the use of more proxy records as e.g. in Lunt et al. (2013, Clim. Past) (Why do the authors restrict their model-data comparison to these four records in the North Atantic?) A recent example for an insightful model-data comparison can be found in Milker et al. (2013, Clim. Past), albeit for another interglacial period. Proxy-related uncertainties can explain only a part of the model-data mismatch, but it would be helpful to include error bars/envelopes for the proxy records into Fig. 4 considering both uncertainties in the paleothermometry as well as in the age model.

2) What are the reasons for the modelled temperature trends? Direct insolation forcing can explain only a part of the variance. In particular, core CH69-K09 shows a positive temperature trend through the LIG which seems to be partly captured by the model. The authors briefly mention changes in the subpolar gyre extent as a possible cause

for the observed temperature trend in the mid-latitude North Atlantic. Some figures illustrating these circulation changes should be included. What is the reason for the changes in the gyre circulation? How do the westerly winds behave?

3) It would also be helpful to include more information on the model cold bias observed for the two northermost cores MD95-2010 and ODP 980. Does the model also produce a cold bias for the modern climate (maybe due to shortcomings in the simulation of oceanic or atmospheric heat transports)? Again, more in-depth analysis of physical mechanisms is needed. The same holds for the explanation of the Southern Ocean early LIG temperature maximum which obviously does not follow the local insolation during summer (DJF). The short paragraph on p.4459, l.23-28 is insufficient. The statment "summer insolation is efficiently stored and results in warm surface temperatures also in winter" is meaningless. It rather appears that, to first order, local winter (JJA) insolation controls the Southern Ocean temperatures year-round. Or maybe another season (SON, MAM) may play a crucial role in driving the year-round temperature trend. An in-depth analysis of this interesting phenomenon should be carried out.

Minor points:

1) p.4441, l.5: The 7 m higher sea level inferred by Kopp et al. (2009) was not during the early LIG, but rather after ca. 125 ka BP.

2) I suppose experiments 125 ka and 130 ka are identical to those published in Lunt et al. (2013, Clim. Past). If this is correct, please state so.

3) How are ozone and aerosol distributions treated?

4) Is a fixed modern calendar used for the definition of months and seasons? If so, this may cause some problems as shown by e.g. Chen et al. (2011, Clim. Dyn.). Unless the authors use a fixed-angular calendar they should discuss why the use of a fixed-day calendar does not affect their results and conclusions.

5) p.4455, l.21: "In the SH the early last interglacial summer/autumn insolation is en-

hanced, while winter insolation is reduced". I think it's the other way round.

6) p.4456, l.12: Northern hemisphere polar amplification during DJF is clearly visible in Fig. 3 in response to GHG forcing. Please discuss. Moreover, how do the results compare to previous LIG simulations by e.g. Yin and Berger (2012, Clim. Dyn.) in terms of the individual roles of GHG and orbital forcing?

7) p.4460, l.25: The authors only discuss the possible influence of meltwater on early LIG high-latitude cooling. However, northern hemisphere ice sheets probably contributed to a global sea level drop of about 20 m around 130 ka BP (Kopp et al., 2009, Nature), i.e. the remnants of big glacial ice sheets might have substantially affected high-latitude climate through albedo and topography in a similar way as in the early Holocene (see Renssen et al, 2009, Nature Geo.). Please discuss.

Interactive comment on Clim. Past Discuss., 9, 4449, 2013.