

Interactive comment on “Melting of Northern Greenland during the last interglacial” by A. Born and K. H. Nisancioglu

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This manuscript is generally well written and presents new simulations of the Greenland ice sheet during the last interglacial period, driven by results from coupled climate simulations for specific time slices. The main finding of this study is the identification of an instability of the northeast part of the ice sheet, due to the small local accumulation rate changes which cannot compensate for enhanced melt. This is therefore an original contribution and perfectly suited for The Cryosphere.

There are however a number of issues that should be improved and I recommend major revisions before the manuscript is suitable for publication.

Major comments

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- The state of the art is not sufficiently reviewed, and a number of recent LIG GIS simulations are not cited (e.g. studies by Lhomme et al, 2004; Tarasov and Peltier, 2003; Robinson et al, 2011; Stone et al, 2011; Van den Berg et al, 2011). The later study is particularly useful to discuss due to the discussion of the direct insolation impact vs the indirect impact through the response of climate to orbital forcing. There must be a detailed discussion of the different climate forcing scenarios used by the other ice sheet models (derived from climate models, or derived / extrapolated from paleoclimate records).

- The exact procedure developed to transfer the climate model outputs to the ice sheet model is not sufficiently discussed. This is an important source of uncertainty and sensitivity tests may be useful.

- The uncertainties linked with the (climate model) climate forcing must be discussed. The IPSL model results may significantly differ from other simulations of the LIG (response to orbital forcing), for instance regarding seasonality effects. So far, no systematic climate model intercomparison has been produced. Masson-Delmotte et al, QSR, 2010 show the differences in simulated temperatures from published climate model results at that time for central Greenland. There should also be large differences in simulated precipitation changes which should be discussed in this manuscript as the small change in N Greenland accumulation is a crucial for the non linearity of the ice sheet response. It is possible that different sea ice changes between different climate models could have different implications for N Greenland moisture transport (see for instance a discussion in Masson-Delmotte et al, Clim Past, 2011). This paper also discussed the differences between projections (2xCO₂) and LIG climate, which have different seasonal implications for the IPSL model, and question the analogies. It cannot be ruled out that patterns of LIG temperature or accumulation changes could be strongly dependent on the model pre industrial climate biases (see Yoshimori and Abe Ouchi and J Clim, 2011) as shown for climate projections.

- The uncertainty linked with the lack of representation of the impact of the GrIS melt

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on climate should be discussed (e.g. Swingedouw et al, J. Clim. , 2009).

- Examples of temperature and accumulation results from the coupled climate model and their extrapolation using the insolation index should be illustrated. There are uncertainties linked with the specific impacts of obliquity or precession.

- The sensitivity of the ice sheet model results to the initial state of the ice sheet should be discussed (e.g. temperature profile within the ice sheet).

- There must be a discussion of the LIG elevation changes and implications for the comparison with ice core results. This was partly discussed in Masson-Delmotte et al, Clim Past, 2011 and it would be very interesting to see what are the implications of the GrIS topography for different ice core sites.

- Results. The first paragraph is not well suited here and should be moved to the introduction.

- Ensemble simulations. Please summarize briefly the methodology. Explain the differences between CCSM3 and IPSL climate results (temperature, accumulation, stability criterion. . .). This section is very short and should be expanded.

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Minor comments

- Introduction and status of global climate during the LIG. Please refer to recent syntheses (e.g. Turney and Jones, JQSR, 2010 or Mc Kay et al, GRL, 2011). Regarding the atmospheric composition, higher resolution ice core records spanning the LIG are now available from ice cores other than Vostok (e.g. Loulergue et al, Nature, 2008; Lourantou et al, QSR, 2010; Masson-Delmotte et al, PNAS, 2010); they confirm values comparable to pre-industrial levels but higher than during the early to mid Holocene. They are key questions in the approach of CAPE (2006) or Clark and Huybers (2009) which compile the warmest intervals without discussing their synchronicity. There is recent evidence for asynchronicity (bipolar, Masson-Delmotte et al, PNAS, 2010; in the

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pollen records, Davis and Brewer, Clim Dyn 2009; in the Arctic deep sea sediment records, Bauch et al, QSR, 2011). Pollen data were also used to discuss South Greenland climate (De Vernal and Hilaire Marcel, Science, 2008).

- Greenland ice cores. The identification of LIG ice has recently been strengthened for GISP2 and GRIP (Landais et al, JGR, 2004; Suwa et al, JGR, 2006). For other ice cores, the identification of LIG ice remains equivocal and I recommend a cautious use of existing indirect evidence due to huge chronological uncertainties (“dated to be older. . .”). The introduction may also mention the unpublished results from the NEEM ice core and evidence for a large segment of LIG ice (ex Foresta et al, AGU abstract, 2011).

- Model description. The comparison with existing proxy data in the vicinity of the GrIS should be expanded. A comparison with ice core data has been conducted by Masson-Delmotte et al, CP 2011 using additional simulations with the LMDZiso atmospheric model including the representation of water stable isotopes. This comparison showed the questions linked with changes in central Greenland ice sheet elevation and with the LIG isotope-temperature relationships.

- Some statements are difficult to understand, such as “orbital parameters are adjusted to 126 ka. While this generally is a good approx. for the Eemian, it is probably not realistic for the GrIS”. There should be a figure showing the time evolution of orbital parameters and therefore the major changes in excentricity, precession and obliquity from 130 to 115 ka, and the limitation of the use of one snapshot coupled model result. This should be combined with Figure 2. Examples of temperature and accumulation results from the coupled climate model and their extrapolation using the insolation index should be illustrated.

- The methodology to transfer climate model outputs at the grid size of the ice sheet model is not sufficiently described.

- NEEM should not be introduced page 3522 but before (general introduction). This

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discussion must be expanded and the model results regarding elevation at different ice core sites must be presented.

- Stability criterion. The ratio between ablation and accumulation obtained from different sources of information (e.g. ERA40 for present day, IPSL or CCSM models for present day, 130, 126, 115 ka, projections) could be compared and discussed.

- Discussion and summary. Discuss the climate forcings used by Cuffey and Marshall (2000), Lhomme et al (2005), Otto Bliesner et al (2006) and implications for accumulation/ablation ratios compared with yours. The paragraph page 3525 (lines 20-24) should be further expanded and supported.

- I am not a specialist of isostasy, but it seems that the combination of ice thickness and isostasy is critical for the comparison with field data (e.g. page 3526, lines 12-21). There is no discussion of the representation of isostasy in the model used here.

- The comparison with ice core data should be expanded by discussing the model results (elevation changes at the ice core sites, implications for comparison with climate model results).

- I recommend to separate the discussion from the conclusions and perspectives. The analogy between LIG and future climate needs to be critically assessed given the very different seasonal or latitudinal aspects of the associated radiative forcings (see Masson-Delmotte et al CP 2011). The last paragraph (current changes and implications for N Greenland) should be placed in the discussion.

- What are the perspectives for this study. Uncertainties linked with climate parameters (interplay between temperature and accumulation) appear critical. Inter-model comparisons seem to be necessary and may be planned within PMIP3.

- Figure 1: show the results for the same period (1979-2001 appears to be common to the two time series). How do the IPSL model results for the control simulation differ from those?

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- Figure 2: not very useful, show the forcings, orbital parameters. . .

- Figure 3: what would be the temperature recorded at the ice core sites, given (i) the transfer of IPSL model outputs to the ice sheet surface and (ii) the elevation change? Please add a discussion to the statement that "pre industrial ice area and thickness are well reproduced", with a specific discussion of N Greenland.

- Figure 4. A lot of the caption text should be moved into the main text.

- Figure 5 should be combined with Figure 4.

- Figure 6 only makes sense with a discussion of temperature and precipitation outputs from the IPSL and CCSM3 models (maps?).

Interactive comment on The Cryosphere Discuss., 5, 3517, 2011.

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