The Cryosphere Discuss., 5, C1931–C1935, 2012
www.the-cryosphere-discuss.net/5/C1931/2012/

The Cryosphere Discussions

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

Anonymous Referee #2

Received and published: 15 February 2012

This paper presents simulations of the Greenland ice sheet (GIS) for the last interglacial period carried out with the 3D thermo-mechanical ice-sheet model (SICOPOLIS) based on the shallow ice approximation. The climate forcing is provided by a 126 ka simulation of the IPSL_CM4 model. The results show that the most vulnerable region of the ice sheet during the Eemian is the northeastern part due to increased ablation (in response to warmer climate) and small accumulation rate. The behavior of the Greenland ice sheet under warm conditions (warmer than pre-industrial climate) has a range of paleo- and future climatic implications that deserve being thoroughly investigated. However, I found that some of the arguments presented in this study are not fully convincing, the impact of the assumptions are not discussed, and additional references to previous studies should be mentioned. Therefore, the paper needs a number of revisions, (addressed below) before being published.

Specific comments:

1) Comparison to previous studies

According to the presentation of the general scientific context in the introduction, it appears that previous model studies reached the conclusion that the most vulnerable part of the Greenland ice sheet is the southern one. In other words, this study appears as the first one to predict a greater instability of the northeastern sector (e.g. sentences such as "in contrast to previous studies" (page 3519, lines 27-28) should be removed). This presentation is misleading. Although, the authors mention the papers from Lhomme et al. (2005), Tarasov and Peltier (2003), Robinson et al. (2011) and Fyke et al. (2011) in the section "Discussion and summary", these studies provided Eemian simulations of GIS rather different from those provide by Cuffey and Marshall (2000) and Otto-Bliesner et al (2006). They should therefore be cited in the introduction.

2) Initialization of the ice-sheet model

Ice and bedrock topographies are initialized with modern observations. This assumption is justified in the absence of any stronger constraints on the GIS geometry during the Eemian. However, nothing is specified concerning the initial temperature in the ice. The evolution of the ice sheet from 130 to 115 ka will likely be influenced by the past surface climate that is recorded in the vertical temperature profile. Actually, temperature exerts a strong influence on ice flow velocities through ice viscosity. Therefore, the vertical temperature profile must be properly initialized to account for the past history of Greenland. I suggest the authors to follow the classical way used in glaciological modeling studies (i.e. to perform a $\sim$ 100 kyr spin-up simulation). Neglecting this spin-up phase must have strong impacts on the results that should otherwise be discussed.

3) Greenland ice-sheet simulation

I don't understand why the transient simulation (130 – 115 ka) is not used to infer the

response of the Greenland ice sheet. I understand that the climate forcing (interpolated between both snapshots) is only an approximation of the real climate. However, forcing the ice-sheet model with 126 ka conditions during several thousand years clearly overestimate the ice mass loss and the GIS contribution to sea-level rise. This method only provides an upper limit of the sea-level rise contribution. By the way, I wonder how is the simulated ice volume at the end of the simulation (i.e. after 10 000 years). It could likely be reduced to almost zero. On the other hand, there is no objective reason to examine the loss of Greenland ice after 5000 years, a duration which is estimated thanks to the transient simulation, under fully different forcing conditions.

4) Stability criterion

Page 3523, line 2: Assuming that dH/dt only depends on accumulation and ablation is equivalent to only examine the direct impact of surface climate. Why do the authors neglect the lateral transport of the ice ? This approach seems to rely on huge assumptions that should be better justified. Moreover, as far as I can understand, the relation between precipitation and altitude has not been accounted for in the downscaling scheme. This can have huge consequences in the evaluation of the stability criterion that should be discussed.

Other comments

Abstract :

The approach followed in the present study should be briefly described in the abstract

Line 12: “Despite moderate melt rates” seems to be in contradiction with “strong melting”. The sentence should be reformulated.

Line 13: “Fast ice flow” is mentioned, but to my knowledge, SICOPOLIS is not able to simulate fast ice flow.

As outlined in the recent study of van de Berg et al. (NatGeo, 2011: doi:/10.1038/NGEO1245), the analogy between the present interglacial period and the

Eemian is not straightforward. Therefore, the last sentence in the abstract appears to be an overstatement.

Model description

Page 3520, line 15: Neglecting ice streams is maybe a widely used approximation, but that cannot be qualified as valid for the Greenland ice sheet. This statement should be removed.

Page 3520, line 17: “of the upper boundary” could be removed.

Page 3520, lines 21-22 : the insolation parameters of the IPSL_CM4 115 ka simulation are certainly not adjusted to the 126 ka values. Otherwise there wouldn’t be any difference between 126 and 115 ka simulations since GHG concentrations are fixed to their pre-industrial levels in both cases.

Page 3520, lines 22-23: the sentence “while this is generally . . . not realistic for the Greenland ice sheet” is not clear. Does the good approximation refer to the 126 ka insolation. If so, why isn’t it realistic for the GIS?

Section 3.1

Rephrase “the duration of Eemian melting”. How do the authors define the melt duration ? Is it defined (as I guess) as the period over which the average ice thickness of the ice sheet experiences a strong decrease ? If so, according to figure 2, this simulated duration could be accurately quantified, and I don’t understand why the authors cannot be more précised than “between 5000 and 7000 years”.

I think that additional figures are necessary in this section. Firstly, I suggest the authors to display the evolution of the simulated ice volume instead of the average ice thickness, or to also display the evolution of the GIS surface. Moreover, it could be useful for the reader to have an idea of the climate forcing (temperature and precipitation fields) for both 126 and 115 ka periods.

Page 3522, line 26: Figure 4 (middle) represents the anomaly of accumulation between 126 and 0 ka. This figure shows that the accumulation difference between both periods is small, not that the accumulation is low.

Page 3522, line 24: Figure 4 does not show that melt rates are lower in the northeastern part than in other parts.

Ensemble simulations

It is surprising that the 130 ka CCSM3 simulation provides similar results to those inferred from the 126 ka IPSL_CM4 climate forcing, It seems to me that Otto-Bliesner et al (2006) used the same simulation and obtained a rather different configuration of the GIS (mentioned by the authors). Is there any difference between both simulations?

Robinson et al (2011) showed that any change in one model parameter could lead to rather different ice sheet configurations. This should be commented in the discussion.

Table 1 : What are the parameter values used in the standard simulation? Maybe an explanation of the role of these parameters should be useful.

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