Author's final response to "Heinrich event 1 : an example of dynamical ice-sheet reaction to oceanic changes"

The authors want to thank the referees and the editor for the helpful review. We have here discussed in detail all the points addressed in the reviews.

Anonymous Referee #1

Received and published: 21 June 2011

Review of: "Heinrich event 1: : :" by Alvarez-Solas

General comments: *This is an important paper that serves to document a new mechanism for Heinrich events that overcomes a long-standing set of inconsistencies and contradictions in previous mechanisms. The work is well presented, well articulated (with the exception of a few areas where the English can be corrected, as indicated below), and convincing. The work builds on previous theory, including aspects of previous theories that were part of mechanisms that have not succeeded. The modeling demonstration both illustrates the working of the mechanism and indicates various scales of response (e.g., sea level rise rates) that can be addressed with observation.*

I strongly support the publication of the paper essentially "as is". Some readers will find the model description very short, however I believe that there is sufficient documentation in the on-line supplement and in cited literature to allow a reasonable scientist to be able to reproduce the experiments.

We are very glad that the reviewer strongly supports publication. We have discussed in detail all the following points and we have decided to slightly enlarge the model description as suggested by the editor.

Specific comments:

Abstract, it would be helpful to give a time range for H1 (some readers may not remember).

The time range has been mentioned in the abstract of the new manuscript version. We included the expected time range for H1 (18 to 15 kyr B.P) where it is cited for the first time.

I*t would be great if the paper had line numbers: Where it says: "Recently, the sensitivity of the glacial AMOC to the wind-stress strength was investigated by integrating the model to equilibrium with the Trenberth et al. (1989) surface wind-stress climatology multiplied globally by varying factors alpha [0.5,2] (Montoya and Levermann, 2008)." It is not clear what the factors are and what "climatology" means: is "climatology" a vector field? And what does it mean to multiply "globally" as opposed to simply multiplying?*

The wind-stress "climatology" is indeed a vector field. And the alpha factor represents the number by which the module of the mentioned vector field has been multiplied in Montoya and Levermann, 2008. The above former sentence has been replaced in the new version by:
"Recently, the sensitivity of the glacial AMOC to the wind-stress strength was investigated by integrating the model to equilibrium with the Trenberth et al. (1989) <u>climatological surface wind-stress vector field scaled by a globally constant factor alpha</u> Є [0.5,2] (Montoya and Levermann, 2008)."

"Ice streams velocities and ice-shelves behavior" should be rewritten: ice stream velocities and ice shelf behavior.

It has been modified.

In subtitle 3, change "ice-streams" to "ice-stream" (no need for plural).

Also corrected.

It is not clear what this sentence means: "In the glacial simulation, NADW takes place in the Nordic and Labrador Seas (not shown)."

This sentence was intended to mean in the glacial background simulation with the CLIMBER-3alpha model North Atlantic Deep Water Formation in the model occurs in the Nordic Seas and the Labrador Seas. We realize the word "formation" was accidentally omitted and therefore the sentence lacked full meaning; we have thus corrected accordingly.

In this sentence: "To investigate its potential effects on the LIS," what does LIS mean? Is this the Labrador Ice Shelf, if so, can something be said about this in the previous section where model spin up is described, i.e., when does the LIS first appear?

LIS means Laurentide Ice Sheet. It is now defined in the abstract as well as in the introduction where it is used for the first time.

Change "for ice shelves breakup" to for ice shelf break up (it is presumed plural).

It has been corrected in the new version.

In Figure 1 it might be a nicer figure if a 4th panel were added (this would balance the array from 2 by 1 to 2 by2). The panel to add would be one which shows ice type, e.g., where is the floating ice shelf, where are the ice streams that are going to respond to the loss of the ice shelf, etc. This would be a kind of "ice-sheet parts map".

We appreciate this comment and we have followed the suggestion by showing a 4th panel devoted to illustrate the different parts of the ice sheets depending on its dynamics. (See first figure attached here).

In figure 3, the black and blue color scheme for the top two panels isn't easy to see. Maybe the blue can be lighter? Also all the panels would benefit if there were vertical lines added (and labeled by letters?) to signify when the ice-shelf basal melting starts, when the ice shelf has collapsed by 90%, and other such things.

Both suggestions were considered. The blue of figure 3 is now lighter as well as we added a label which indicates the duration of the oceanic subsurface warming, the ice-shelf breakup – i.e. 95 % of surface reduction - (A) and the phase of missing ice shelf (B). (See second figure attached here).

In figure 4, and in the text ... is the sea level rise rate for all the ice sheets (including the Fennoscandian, which I see is part of the model), or just that due to the loss of ice in response to the H-event forcing?

The mentioned sea level rise and that shown in figure 4 corresponds only to the Laurentide contribution derived from the oceanic subsurface warming, so, indeed just due to the Laurentide ice loss in response to the HE forcing.

Anonymous Referee #2
Received and published: 14 July 2011

I thoroughly enjoyed reading the manuscript by Alvarez-Solas et al. It presents a beautiful new idea in an ice-sheet/ice-shelf modeling framework and provides an interesting twist on our understanding of the dynamics of Heinrich event 1. The paper is well written, but a few minor comments may help to further improve this significant contribution.
It is in fact a pity that this paper was not submitted to a higher profile journal.

We are very glad that the reviewer finds the paper interesting and worth publication. The paper was, in fact, submitted to other higher profile journals, but unfortunately was never sent to reviews.

Minor comments: - *the authors discuss a very important positive feedback for Heinrich events. Nowhere in the text is this mentioned. In fact, I think the authors would really gain fame with their results, if they added a little schematic figure, similar to the one attached here.*

We are very grateful to the referee for suggesting such a schematic figure that nicely summarizes the main idea discussed in the paper. We indeed decided to slightly modify the suggested figure and add it to the new version. This new figure (-figure 5- attached in this final response) is also discussed in the text:

"However, the associated freshwater discharge from the H1 event could further impact deep water formation, eventually leading to its shutdown. <u>This configures a feedback mechanism (Fig. 5) that explains why during Heinrich stadials the AMOC appears more perturbed than during non-Heinrich stadials, as suggested by proxy-data (Hemming, 2004; and references therein)</u>."

And:

"<u>As summarized in figure 5,</u> for H1 we assume, as suggested by proxies (Hall et al., 2006), that the early deglaciation of the Fennoscandian ice sheet resulted in enhanced freshwater fluxes to the North Atlantic, forcing the ocean into a state with weak Atlantic overturning and NADW south of Iceland, similar to a stadial period".

- I would recommend the authors to discuss figure 4 in more detail. Why is there a 2nd peak/plateau (around years 1200-2000)? What is the dynamics associated with this? Why does this occur only for one of the parameter configurations tested?

Figure 4 is discussed in more detail in the new version:

“The ice discharge reaches a maximum at the mouth of the Hudson Strait ice stream around 700 yr after the beginning of the subsurface warming in the Labrador Sea (Fig. 3), corresponding to the second peak in iceberg discharge into the Atlantic Ocean (Fig. 4). However, the enhanced ice flow surge is simulated for a time period largely exceeding the oceanic subsurface warming duration, translating in a second peak in sea level rise rate and an extended plateau of ice discharge after the main peak (see purple and gray curves respectively in figure 4). The time scale is set by the time needed by the ice streams to firstly respond to the perturbed longitudinal stresses at their mouth until their source (~1000 km far inland) and then to equilibrate under the new force balance at the grounding line.”

Concerning the different response depending on the parameter configuration, it is now discussed thoroughly in the supplementary information (where parameters are shown and where figure 1 is discussed):
“Two main parameters were considered for exploring the phase space of the announced mechanism: the κ-parameter determines the magnitude of basal melting changes as a function of oceanic temperatures, and the υ^2-parameter (=*f* x10-4 in legend of figure 1) represents a basal friction coefficient in ice streams. As shown in figure 1, for high values of the κ-parameter a rapid and pronounced first peak of ice discharge is simulated as a response of the ice-shelf collapse. On the other hand, the value of the *f*-parameter determines the long-term discharge derived from the ice acceleration as a consequence of the former buttressing removal. High values of this latter parameter result in a significant inland ice discharge (if the ice-shelf collapse is efficient enough; excluded for k=0.2 m/yr/K). However high dragging coefficients imply relatively low ice-stream velocities: the loss of thickness near the grounding line is then less pronounced (compared to low values of the *f*-parameter) and therefore the signal is less efficiently propagated inland, finally explaining the absence of an extended plateau of ice discharge after the first acceleration.”

- *Please be explicit about some issues regarding the physical consistency in the model set-up.*
a. The existence of a Labrador ice-shelf excludes the possibility for Labrador Sea Water formation in reality. However, this effect is not taken into account in the Climber model simulation that is used as a forcing

We agree wit the referee. Indeed climber3-alpha runs are not taking into account the existence of an ice shelf over the Labrador Sea, which in fact would

substantially affect deep water formation in this area. However, this aspect does not seem critical for the proposed mechanism; firstly, the main contribution to the subsurface warming in the Labrador Sea comes from advected Nordic Seas warmer waters; and secondly it can not be totally excluded that Labrador Sea deep water formation occurs in presence of the Labrador ice shelf, even if the process would be somewhat different from what is simulated by climber3-alpha (the latter is open ocean convection while the former one could be an analog of what is observed today under the Ronne ice shelf -salt rejection and plumes contribution to deep water formation-).
Nevertheless, as suggested by the referee, we added to the new version a sentence including this weakness in the model set-up (see next point).

b. I know that equations (1) and (2) are commonly used in offline-ice sheet model runs. But these equations assume that temperature and precipitation variations are homogeneous across the different ice-sheets, which I think is total nonsense. Stationary wave feedbacks are ignored and the ice-albedo effect is not captured in a physically correct way. I would urge the authors to just state the assumptions made when using this forcing upfront and discuss the caveats.

We also agree that perfectly synchronous temperature and precipitation changes as implied by our equations (1) and (2) are hard to conceive. And that this method also implies a sometimes overestimated albedo effect and is unable to consider the effects of any change in ice-sheets elevation on the atmospheric circulation. This (and the previous point) has also been addressed in the new version of the manuscript: (at the end of the Model set-up and experimental design)

“This method has been used in many studies to simulate the evolution of the cryosphere during the last glacial cycle (Charbit et al., 2007). Note, however, that the experimental setup used here does not resolve the coupled effects between ice-sheet—ice-shelf dynamics and atmospheric and oceanic circulations. Concerning the ice-sheet reconstruction, it implies that the dependence of atmospheric stationary waves on ice-sheet elevation changes is not considered, the ice-albedo effect could be overestimated and temperature and precipitation changes occur synchronously along the different ice-sheets all over the last glacial period. It also implies, that the direct effects of the simulated Labrador ice shelf on the Labrador Sea deep water formation can not be accounted for here. In spite of the current limitations in the experimental setup, the simulated Northern Hemisphere ice-sheet characteristics for 18 kyr BP (Figure 1) show good agreement with reconstructions in terms of volume and geographical distribution, and agree remarkably well with these in terms of ice-stream locations (Winsborrow et al., 2004)”.