

Interactive comment on “Contribution of Greenland ice sheet melting to sea level rise during the last interglacial period: an approach combining ice sheet modelling and proxy data” by A. Quiquet et al.

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The valuable comments of Patrick Applegate were really appreciated. In the following, we intend to respond to each individual remarks and the revised version of the manuscript will be soon transmitted. Referee’s comments are identified by RC and authors’ by AC.

RC: I was happy to see this new article by Aurelien Quiquet and colleagues, which uses ice sheet modeling to estimate the Greenland Ice Sheet’s contribution to sea

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level change during the Eemian warm period. As Quiquet et al. note, the Eemian provides an important analog to possible near-future conditions on Greenland, and investigation of this period may lead to better projections of future sea level change. Results from other studies using similar methods diverge widely; Alley et al. (2010, Quaternary Science Reviews) note a possible range of 1-5 m sea level equivalent, or about 15-70% of the ice sheet’s present volume (7.3 m sea level equivalent; Bamber et al., 2001, J. of Geophysical Research; Lemke et al., 2007, Fourth Assessment, IPCC Working Group 1, ch. 4). More work on this important question is clearly needed.

RC: This study includes some important methodological advances over past work. In particular, Quiquet et al. use an ice sheet model with a relatively good treatment of ice streams and ice shelves. To my knowledge, only one previously published ice sheet modeling study that covers the Eemian period includes these advanced dynamics (Fyke et al., 2011, Geoscientific Model Development – please cite this paper). The paper also takes advantage of some new global climate model simulations of the Eemian, from two different climate models. The temperature anomaly curve in Figure 2 helps overcome the limitations of ice core records from Greenland (no published, continuous records extend into the pre-Eemian period, as needed for satisfying simulation of the Eemian itself). Although not quite new, the study also includes the tracer method from Lhomme et al. (2005, Quaternary Science Reviews; originally from Clarke and Marshall, 2002, Quaternary Science Reviews – Quiquet et al. need to cite this paper). The use of this tracer method allows comparison of model results to ice core data (and must have been a great deal of work to code!).

AC: We included the missing reference to Clarke and Marshall (2002).

RC: However, a critical-minded, but not very careful, reader might miss the advances in the paper, and I recommend extensive changes to help bring out the paper’s good qualities. The authors should include descriptions in the abstract and introduction about what is new about the paper, explain more clearly how this study relates to earlier work (including Quiquet et al.’s earlier paper in *The Cryosphere*), and proofread the

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text carefully. I recommend "minor revisions" because I think the study does not need many new analyses to be complete; changes to the text should be largely sufficient.

RC: I also need to say up front that I cannot reconcile Quiquet et al.'s estimates of contributions from the Greenland ice sheet to Eemian sea level rise with earlier work, particularly that of Kopp et al. (2009, *Nature*) and Lhomme et al. (2005). Kopp et al. Used well-founded statistical methods and paleo-sea level data to estimate a 95% probability that Northern Hemisphere ice sheets (largely Greenland) contributed at least 2.5 m sea level equivalent to overall Eemian sea level change. This statement is a bit buried in the paper – check the left-hand column on their p. 866. Lhomme et al. came up with a range of 3.5-4.5 m for the Greenland contribution to Eemian sea level change. Quiquet et al. find an *upper bound* of 1.5 m for this quantity. In other words, Quiquet et al. fundamentally disagree with earlier studies on their key finding, including the study that is their nearest methodological predecessor (Lhomme et al., 2005). I didn't find any explanation of this disagreement in the paper – I would expect to see it in the Discussion. A satisfying explanation of this discrepancy is absolutely needed for the paper to be publishable. In particular, Quiquet et al. should please explain what methodological choices they made that are 1) different from those of Lhomme et al. and 2) tend to make their Eemian ice volume change estimate smaller than that of Lhomme et al. How robust are the assumptions underlying these choices? Could a reasonable person make different choices and obtain a larger estimate?

AC: We disagree indeed about the present reconstructions from earlier works, with higher simulated volume during the LIG period. We identify mainly 4 reasons for that: First of all, the major difference with earlier work is probably the temperature anomaly used. As far as we know, in previous studies of this type, the temperature anomaly was deduced from a mixture of GRIP O18 and Vostok deuterium records. It is the case in Lhomme et al. (2005), Greve (2005), Huybrechts (2002) and Marshall and Cuffey (2000). Due to the seesaw effect, timing and amplitude of temperature variations in Antarctica and in Greenland are poorly correlated, rendering the use of Vostok

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deuterium record as a proxy for Greenland temperature highly questionable. We don't argue that our combined methane-SST is the ultimate proxy for Greenland temperature, but we expect a better correlation in timing at least. Our temperature anomaly during the LIG period exhibits a rather smooth pattern, different from the sharp signal obtain with the Antarctic deuterium record in the previous mentioned studies. We can also add that there is some evidence that the NEEM site was glaciated and that the ice there recorded a LIG period with temperature around 8 degC (Dahl-Jensen personal communication). Previous studies, with higher simulated retreat, generally simulated a much higher warming at NEEM, due to site elevation change. The study of the NEEM ice should bring valuable information about the in-site temperature change and site deposition temperature change, but outputs from this campaign have been too recent to be usable for the present study. We also have to acknowledge the fact that our anomaly method may slightly underestimate the warming over the ice sheet during the LIG. GCMs were run over the exact same topography for the LIG and for the present day, whereas it is more likely that a reduce ice sheet may be responsible for large scale perturbations on atmospheric circulation. Our work also differ from earlier works from the calibration procedure we employed. We do have a different temperature amplitude of the glacial-interglacial transition, as well as a different precipitation correction. Our values have been obtained while comparing simulated age-depth relationship with the GICC05 timescale. This calibration against age-depth relationship is a strong constraint for the ice sheet model parameters, especially the climate-related ones. GRISLI has been used in our work with a 15 km² grid, which is generally finer than what it has been used in previous studies. We should expect a better representation of the ice margin processes, such as ablation. Previous studies providing estimates of Greenland melting contribution to sea level rise during the LIG period use only the SIA approximation. Our hybrid ice sheet model is here an improvement.

Being more specific, you are right, Lhomme et al. (2005) follow a similar approach than ours. Nevertheless the model they used was a SIA-only ice sheet model (University of British Columbia ice sheet model). Also, all the above listed difference are also valid

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when comparing from the work of Lhomme et al. (2005).

The work of Kopp et al. (2009) is a probabilistic assessment of sea level rise during the LIG based on local sea level indicators. The methodology here is radically different. Attribution of local sea level rise to the major ice sheet reductions is however also subject to uncertainties already listed in the Kopp et al. (2009) paper. For example: "Many factors other than the changing volume of water in the ocean modulate the influence of melting ice sheets on LSL. These factors include: the direct gravitational effect of the distribution of ice, water and sediment on the sea surface (or geoid), solid Earth deformation and its associated gravitational signature, perturbations to both the magnitude and orientation of the Earth's rotation vector, and time-varying shoreline geometry, as well as changes in ocean and atmosphere dynamics. In addition, LSLs are influenced by tectonic uplift and thermal subsidence."

As you suggested, we will try to emphasise better the main differences of our work from previous ones in the revision version of the manuscript.

RC: The authors' new temperature anomaly curve is clearly important; however, other studies have performed similar "blends" of Greenland and Antarctic paleoclimate records (Marshall and Cuffey, 2000, Quaternary Science Reviews; Huybrechts et al., 2002, Quaternary Science Reviews; Greve, 2005, Annals of Glaciology; Barker et al., 2011, Science). Quiquet et al. should explain why their spliced record is preferable to the others, and show a comparison of their reconstruction to that of Barker et al. (2011). Placing the Barker et al. curve in the background of Figures 1 and 2 would help a great deal.

AC: As mentioned in our last remark, all previous ice sheet modelling studies have used a temperature anomaly curves built up on the Vostok deuterium record. We assume that the well mixed atmospheric methane is a better proxy for Greenland temperature. We acknowledge that the work of Barker et al. (2011, Science) is an original approach to reproduce the temperature variations in Greenland, using a bipolar seesaw model.

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We indeed have compared our index with Barker et al's. However, Barker et al's variations is not reproducing the O18 variations observed in the NGRIP record, especially during the LIG and that's the main reason why we use our own index. In the revised version of the manuscript, we added this index in Fig 1, and we now explain why we didn't use the Barker et al's index (see in the following Fig. 1 and Fig. 2).

RC: The authors should proofread the revised manuscript carefully before resubmitting it, and they should also proofread future manuscripts *before* the initial submission. At least one of the reviewers on Quiquet et al.'s earlier paper in The Cryosphere also asked for more careful proofreading. To help catch mistakes, I recommend reading manuscripts out loud.

AC: Thank you for the suggestion, we will do our best to improve on this point in future submissions.

RC: The paper includes a number of unsupported statements, some of which I will flag in my detailed comments below.

RC: As far as I can determine, the GRISLI model is not publicly available. The Grenoble group should consider releasing the model code to guarantee that their papers are fully reproducible by other scientists. Other groups have released their codes on the Web (PISM, Glimmer-CISM, SICOPOLIS...). This practice has led to wider use of these models and an increase in publications for the scientists who wrote the models. Also, I have heard that the highest-impact journals (Science, Nature) will no longer accept papers based on closed-source models.

AC: There is indeed no released version of GRISLI in open source. The reason is mainly because this code is also used as a research tool and is upgraded very often. It is very versatile and a whole variety of applications can be performed with it. The drawback is that it requires a lot of time and effort to produce a "standard version" and its documentation to make it a public tool. Even if the code is used in other places, GRISLI is mainly maintained (regular merging, major updates) by one researcher at

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the present time, Catherine Ritz. The lack of time is the main reason for the lack of a public version of GRISLI. However anyone interested in using GRISLI should get in touch with Catherine Ritz as she is usually very happy to share the model.

RC: /âĀĤ- Detailed comments âĀĤ-/

RC: Title: The title should emphasize what is really new about the paper. Many other studies "combin[e] ice sheet modeling and proxy data" with methods rather similar to those used here. Perhaps "Estimating the Greenland ice sheet contribution to sea level rise during the last interglacial period using an advanced ice sheet model" would more accurately reflect the advances made in this study.

AC: You were probably right, and maybe the model-aspect is not the crucial one. We now suggest as a title: "Greenland ice sheet contribution to sea level rise during the last interglacial period constrained by ice core data."

RC: Abstract Please begin the abstract with a one-sentence summary of the paper that explains what was done, how, and the significance of the results. Perhaps, "Long-term simulations of the Greenland Ice Sheet using an improved ice sheet model suggests a relatively low contribution to Last Interglacial Period sea level rise from Greenland melting." Consider rewriting the abstract according to the Nature template.

RC: 4-5: Please remove all parenthetical citations from the abstract. You can just say, "... as pointed out in the Fourth Assessment Report of the Intergovernmental Panel on Climate Change."

RC: 5: "warm up": odd word choice

RC: 9: "has survived": just "survived"

RC: 10: "...ice reduction during the LIG and its sea level rise contribution": partly redundant

RC: 12-14: "paleo data," "proxy data": what data do you mean? how did you do the

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confrontation?

RC: 13: "methodoly": spelling

RC: 15-16: how does your estimate compare to those from other studies?

AC: Here is the new version of the abstract, following the Nature template (basic introduction, detailed background, general problem, "here we show", main result, general context): "As pointed out by the 4th assessment report of the Intergovernmental Panel on Climate Change, IPCC-AR4 (Meehl et al., 2007) the contribution of the two major ice sheets, Antarctica and Greenland, to global sea level rise is a subject of key importance for the scientific community. By the end of the next century, a 3–5 C warming is expected in Greenland. Similar temperatures in this region were reached during the last interglacial (LIG) period, 130 – 115 ka BP, due to a change in orbital configuration rather than to anthropogenic forcing. Ice core evidence suggests that the Greenland Ice Sheet (GIS) survived this warm period but great uncertainties remain about the total Greenland ice reduction during the LIG. Here we perform long-term simulations of the GIS using an improved ice sheet model. Both chosen methodologies to reconstruct palaeoclimate and to calibrate the model are strongly based on proxy data. We suggest a relatively low contribution to LIG sea level rise from Greenland melting, ranging from 0.65 to 1.5 m of sea level equivalent, contrasting with previous studies. Our results suggest an important contribution of the Antarctic ice sheet to the LIG highstand."

RC: Introduction

18-21: this paragraph contains an awkward first sentence and is somewhat underdeveloped; what methods or proxies did Vezina et al. and Kopp et al. use to arrive at these conclusions?

AC: We split the sentence and add some information about Vezina et al. and Kopp et al. estimations. Here as the sentences: "Eustatic sea level highstands reconstructions from sediment studies suggest that the LIG sea level anomaly from present day stands

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for the highest in the last 200 ka BP (e.g. Vezina et al., 1999). Recent probabilistic assessment of sea level rise during the LIG based on local sea level indicators suggest a likely value greater than 6.7 m (Kopp et al., 2009)."

RC: 26: "is assumed to have been found": needlessly verbose; avoid using the passive voice

AC: Text changed here.

RC: p. 3347 1: the evidence for ice cover at Dye-3 during the Eemian is much more equivocal than at the other sites; see Alley et al. (2010, Quaternary Science Reviews)

AC: We moderate the sentences: "However, LIG ice is assumed to have been found at five deep ice core drilling sites (GRIP, GISP 2, North GRIP, Camp Century, and the latest one, NEEM), suggesting a fairly limited ice reduction. Ice older than the LIG period has been found at DYE 3 in South Greenland, but its interpretation is still debated (Alley et al., 2010)".

RC: 2-3: "Pollen and sediment studies..." Need references here, and some more explanation; how do these studies help?

AC: Pollen was a reference to the work of de Vernal and Hillaire-Marcel (2008, Science). Sediment was a reference to Colville et al. (2011). We re-wrote the sentences to make the point clearer: "It is very likely that the southern GIS retreated further during the LIG than during the Holocene, as suggested by pollen studies (de Vernal and Hillaire-Marcel, 2008) as well as sediment studies (Colville et al., 2011)." These studies rely mainly on observations (palaeo-evidences) and can be used as bounds for models.

RC: 1-6: you should mention the work of Born and Nisancioglu (2011, The Cryosphere Discussions), who point out that ice loss can happen in the north as well as the south – in that case, inferences of small sea level contribution from the extent of the southern part of the ice sheet (Colville et al., 2011) become much more equivocal

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AC: The only reason why we didn't mention the work of Born and Nisancioglu is because it was still in discussion phase. We included now the reference to Born and Nisancioglu (2012, TC). We have to add that the pattern of retreat obtained with GRISLI is really similar to the one of Born and Nisancioglu (2012). Of course you are right about your point of criticism on the work of Colville et al. (2011) and that's maybe one of the reason why both field measurements and modelling are so complementary. We included a statement in the revised version of the manuscript about this issue: "Several ISM modelling studies have pointed out that the North of the GIS could present a larger retreat potential than the South (Stone et al., 2010; Fyke et al., 2011; Quiquet et al., 2012; Born and Nisancioglu, 2012). In this case a small sea level contribution deduced from the extent of the southern part of the ice sheet, as in the work of Colville et al. (2011), becomes equivocal".

RC: 7: "few studies": I would say that a fair number of studies provide an estimate of this contribution, including some that are not in Table 1. Huybrechts (2002, Quaternary Science Reviews), Colville et al. (2011), and Alley et al. (2010) should perhaps be listed.

AC: To avoid confusion, Table 1 contained solely ice sheet modelling studies. Since your comment, we decided to add the other studies of Huybrechts (2002) and Colville et al. (2011), as well as the paper of Born and Nisancioglu (2012).

RC: 9: "is one of the major issues": unsupported statement; why is this the case? the next few sentences explain different methods of estimating surface mass balance, but say nothing about why this diversity is a problem

AC: Reconstructions of SMB in the past is important because we have very few constraints on its natural variability on the millennial timescale. Several studies have pointed out the sensitivity of the ice sheet models to atmospheric forcing fields, in particular the previous studies of Quiquet et al. (2012, TC).

RC: 12: "amongst": just "among;" "amongst" is used almost exclusively in spoken

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language

AC: Text changed.

RC: 20: replace "largely" with "considerably"

AC: Text changed.

RC: p. 3348 3: "improve on the classical index formulation": I'm not 100% sure, but I think the approach endorsed by Quiquet et al. is actually quite standard – please see Pollard and PMIP Participating Groups (2000, Global and Planetary Change), Kirchner et al. (2010, Quaternary Science Reviews) and Greve et al. (2005) for examples of earlier studies that use the same approach. If Quiquet et al. are doing something different, they should explain here how their methods differ from these studies.

AC: We fully agree, the method we used is not new. However, none of the previous modelling studies, which provided an estimate of LIG Greenland melting and used the index method formulation, modify the classical formulation to include information of GCM snapshots. Cuffey and Marshall (2000), Huybrechts (2002), Tarasov and Peltier (2003), Lhomme et al. (2005) are in this category. However, similar methodologies as ours have been used on other ice sheet and/or other time periods (e.g. Charbit et al., QSR, 2002).

RC: 5-8: please provide more description of Lhomme et al: what did they do and what were their key findings? please also acknowledge other studies that use the Lagrangian tracer approach, such as Clarke and Marshall (2002) and Tarasov and Peltier (2003)

AC: Lhomme et al. (2005) implemented the provenance transport model from Clarke and Marshall (2002) into an ice sheet model (University of British Columbia ice sheet model). In addition to the standard comparison of the simulated present day ice geometry, they compared the simulated d18O vertical profiles to ice core records. Parameters which yield a good agreement between simulation and observation, are used to gen-

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erate scenarios of Greenland ice sheet state during the LIG. In the revised version of the manuscript we included the statements: "As in Lhomme et al. (2005) we used the transport model to simulate the age-depth relationship at several ice core locations. The comparison of ice core records to the simulations is used to define the optimal parameters of the ice sheet model. We differ from the work of Lhomme et al. (2005) regarding the model used (SIA only vs. SIA/SSA), the resolution (~25 km² vs. 15 km²), the temperature perturbation used (Antarctic deuterium vs. methane-SST) and the use of anomalies. We also haven't only calibrated on the age-depth relationship but also on the temperature profile, and the simulated volume and ice extent."

RC: 20-25: "... facilitates the advance onto the continental shelf": unsupported statement; please demonstrate that this assertion is true, perhaps by rerunning one of your simulations in Figure 5 with the advanced ice dynamics turned off in the model – how do the results change?

AC: SIA only models do not represent explicitly the ice shelves. Consequently, when ice arrives on the ocean it is either cut-off (drastic calving) or forced to ground. An ice shelf module is required to have a realistic representation of the ice advance. If we switched off the ice shelves module, we would still have to parameterise the ice advance... GRISLI does build an ice bridge between Greenland and Ellesmere Islands during glacial times. Sensitivity of the Greenland ice sheet retreat during the last deglaciation to the calving parameterisation have been performed during A. Quiquet's Ph.D. The french version of the thesis is available here: <http://tel.archives-ouvertes.fr/tel-00704253>, in particular Chapter 5, Fig 5.12.

RC: 26: "this": this what? always follow the word "this" or "these" with a noun that makes it clear what you mean – in this case, "this error in modeled marginal slopes"

AC: Thanks for the advice, we did so.

RC: p. 3349 1-5: I realize this section is about the GRISLI ice sheet model, but a reader not familiar with the literature could get the impression that these are the only ice

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sheet modeling studies that have ever been done. Could you include another section before this that describes how ice sheet models in general work and lists some earlier, groundbreaking studies?

AC: We have tried to do so in the revised version of the manuscript, adding some sentences in the penultimate paragraph of the introduction, after mentioning the paleo-evidences and before the earlier ISM works on the LIG period.

RC: 6: "largely discussed": just "described"

AC: Done.

RC: 6-7: "previously mentioned": just "previous"

AC: Done.

RC: 7: "we only describe here the most relevant features": we only describe the most relevant features here

AC: Done.

RC: 8-20: Kirchner et al. (2011, Quaternary Science Reviews) provide an excellent description of the different flow regimes within ice sheets and ice shelves – please cite that paper here, and provide more explanation of the different flow regimes so that a non-glaciologist can more easily follow the paper

AC: Done.

RC: 10: "heigh": spelling; do you mean thickness?

AC: Yes, thickness.

RC: 11-13: please show this map in the supplement

AC: Good suggestion indeed, the map used for the potential ice stream regions will be included in the Supplement.

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RC: 20: somewhere around here, please explain how GRISLI finds the grounding line position – this model design choice is notoriously problematic in ice sheet modeling; perhaps reference Hilmar Gudmundsson's work on this issue

AC: We used a simple floatation criteria for the grounding line position. We added this information in the text, as well as a few sentences about this issue.

RC: p. 3350 1-8: this section contains many grammatical errors

AC: We hope that the corrected version of the manuscript is now ok.

RC: 2-3: "re-gridded to a stereographic projection...": too much information; delete

AC: Done.

RC: 4-8: how does this geothermal heat map and procedure for adjusting it compare to Greve (2005)?

AC: Our geothermal heat flux adjustment is indeed largely inspired by the work of Greve (2005). However, we tested the sensitivity of the age-depth relationship to this flux and we found a much lower sensitivity to this parameter than to the paleoclimate assumptions (temperature variation amplitude and precipitation correction). Thus, we used a very simple local modification on top of the map of Shapiro and Ritzwoller (2004). Basically, we applied a high value of this flux at NGRIP (135 mW.m⁻²), very low at DYE3 (20 mW.m⁻²) and very slight changes elsewhere. The modification is attenuated with the inverse squared of the distance, within a fixed radius (225 km).

RC: 23-24: unsupported statement; what proxy data, and how do you know that they are a good representation of past climates?

AC: We were referencing to d18O here, we made ourselves clearer in the updated version of the manuscript. You are probably right that these proxies are probably not as simply representative of past climates as we assumed they are, but still, they represent the only constraint on past climates we have...

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RC: p. 3351

1-3: how did you make this composite map?

AC: We started from the finding that the precipitation maps used (both MAR and RACMO) presented large discrepancy with the measured values at ice core location. We notice a wet bias in MAR for DYE3 (more than 35 %) and Camp Century (more than 45 %) and a relatively good agreement at other ice cores. Oppositely, RACMO presented a dry bias for GRIP, NGRIP and NEEM (around 50 %), and a relatively good agreement at DYE3 and Camp Century. A simple weighing of these two maps on altitude and latitude allow us to obtain the composite map. We added the following sentence in the revised version of the manuscript: "Accumulation rates from MAR and RACMO have been compared to measurements at ice core locations. Where MAR exhibited a wet bias (DYE 3 and Camp Century), RACMO showed a good agreement, while where RACMO was too dry (GRIP, NGRIP and NEEM), MAR was close to the observations. An altitude and latitude weighing between these two precipitation fields has yielded an overall better agreement (Figure X.)".

RC: 2-3: "... in order to have good agreement...": unsupported statement; how do you know that the agreement is good? how did you evaluate this agreement? show a scatter plot with measured accumulation for the ice core sites on the x-axis and the modeled values on the y-axis; if the agreement really is good, the points should lie on a 1:1

AC: A plot (or a table) will be inserted in the revised manuscript. Here attached, please find the first attempt.

RC: line 10 and following: in your abbreviation for $\delta^{18}O$, the O should not be italicized

AC: Corrected.

RC: 11: I don't think you mentioned your splicing of the Antarctic records to Greenland

C2520

before this point

AC: Right, corrected.

RC: 19: "profile": profiles

AC: Thanks.

RC: 19: "This calibration is however ice model dependent": "This calibration is, however, ice sheet model-dependent"

AC: Done.

RC: 19: unsupported statement; show us that the calibration depends on the model, or explain why it is

AC: Simulated velocities have an impact on simulated temperature because of the advection term. Also, simulated elevation changes have a direct impact on the simulated surface temperature (due to the topographic temperature gradient). We included this consideration in the manuscript.

RC: 19-20: "of past evolution": on past evolution

AC: Thanks.

RC: 24: monthly values of what? the lapse rate?

AC: Yes, text corrected.

RC: p. 3352 eqn. 3: how does this approach compare to other studies? many other Greenland ice sheet modeling studies include a change in precipitation with surface temperature anomaly; see review in van der Veen (2002, Global and Planetary Change)

AC: Our procedure is relatively similar as we indeed consider a change in precipitation relative to a change in surface temperature. Thus, Eq. 3 was incorrect as it should integrate the temperature change caused by the surface elevation change. We added

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the surface elevation correction term in Eq. 3.

RC: 12-13: "even the most sophisticated RCMs disagree": unsupported statement; can you provide a reference?

AC: See Vernon et al. (2012, TCD) for a state-of-the-art surface mass balance model intercomparison for the Greenland ice sheet. Reference included.

RC: p. 3353 13: "more representative of the winter temperature": more representative of the local winter temperature

AC: More precisely, the $\delta^{18}\text{O}$ record is representative of the whole temperature experienced along the trajectory of water masses before deposition.

RC: 13: "while": use "whereas" instead

AC: Ok, thanks.

RC: 21: I don't think the Lemieux-Dudon time scale includes the ODP 980 core

AC: The inclusion has been provided in the work of Masson-Delmotte et al. (2010, PNAS). This reference has been added.

RC: 23: "works": no s here

AC: Done.

RC: p. 3355 5-12: "as close as possible", "close to," "similar to," "close to": how did you evaluate the match between your results and the data sets you were trying to match? did you do the matching "by eye," or did you try to minimize the root mean square error, or what did you do?

AC: We did not use any systematic minimisation. We used some numerical estimators (present day simulated volume and iced area, present day basal temperature, Younger Dryas and Laschamps events depths) but also some qualitative estimators (high surface velocity area and temperature profile shape). We acknowledge that

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score-minimisation procedure would be highly needed for future work.

RC: 18-19: "the ice extent is governed by... the ablation coefficients": this would be a good place to cite Greve (1997, Journal of Climate)

AC: Indeed, we added Greve (1997) and Ritz et al. (1997) in here.

RC: 23 and following: this paragraph, which spills onto the next page, seems to reproduce material that appears earlier in the paper; delete or condense

AC: We modified the text in this part.

RC: p. 3358 18-19: the last sentence of this paragraph is ungrammatical

AC: Hopefully corrected now.

RC: p. 3359 14: I couldn't find the reference to Members in the list of references 24: "approximatively": approximately

AC: Thanks, the mistakes are now corrected.

RC: p. 3360 Acknowledgements: use the active voice as much as possible here ("We thank so and so", instead of "So and so are thanked"), and check for grammatical mistakes

AC: Ok, done.

RC: References Each reference seems to end with one or more unnecessary four-digit numbers; delete these

AC: Done.

RC: Fig. 2 – is this the annual mean temperature? – "that +5 deg C": than – please show a comparison to Barker et al. (2011) here or on Fig. 1

AC: It is the anomaly, which is applied uniformly over the year. We included Barker et al. (2011) curve here (Fig. 1 and Fig. 2 here attached).

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RC: Fig. 3 – isn't it circular reasoning to use NorthGRIP as both a forcing and a constraint?

AC: We use North GRIP as a proxy for the actual temperature changes in Greenland. The forcing is a $\delta^{18}\text{O}$ -time type. After all model calculations (precipitation correction and ice flow), if these calculations are correct, we should get the vertical profile of the ice age at North GRIP. In other words, the output is a $\delta^{18}\text{O}$ -depth. What we are testing here is the ability of the model to translate the $\delta^{18}\text{O}$ -time record to a $\delta^{18}\text{O}$ -depth output. It is this correction which is calibrated.

RC: Fig. 4 – the model domain includes the Canadian islands northwest of Greenland and Iceland, which most modeling groups cut away; does the inclusion of these islands explain some of the ice volume overestimate noted in the text? – you show the ice core data and results before you show us where the ice cores are located; switch Figs. 3 and 4 and show us where the ice cores are located on this figure – the caption needs some editing

AC: We've already responded to a part of your comment previously concerning the ability of our model to simulate the ice advance onto the continental shelf during glacial times. As mentioned earlier, we have performed several experiments during the last deglaciation changing the ice extent during the LGM. We concluded that we cannot find a strong correlation with a very extended ice sheet during the LGM and a "big" simulated residual present day ice sheet (with all parameters of the model fixed). We think that the overestimation of the present day volume is largely dominated by the poor representation of the eastern margin of the present day topography.

RC: Fig. 5 – please show this figure in terms of raw, undifferenced, simulated ice volume on the y-axes, instead of with the modern value subtracted out

AC: This modification will appear in the revised version of the manuscript.

RC: Fig. 7 – "melt may potentially occur at...": I don't understand this statement; why

C2524

is this significant? I think melt occurred at all of these sites during the 2012 ablation season – do you mean that your results are more likely to be correct because the model can produce melt at these sites?

AC: We mentioned that to stress the fact that the interpretation of the mentioned ice cores (DYE3, Camp Century and NEEM) is suspected to be difficult due to probable surface melting. Recently, the NEEM ice core seems to have recorded melt event during the LIG period (Dorthe Dahl-Jensen, personal communication). It is also interesting to mention that even with the melt event at the NEEM site, we still maintain a relatively extended Greenland ice sheet.

RC: Fig. 8 – "what is found in literature:" spelling, odd phrasing

AC: Corrected, "[...] values lower than the calibrated one, corresponding more to which has been used in previous studies".

Interactive comment on Clim. Past Discuss., 8, 3345, 2012.

C2525

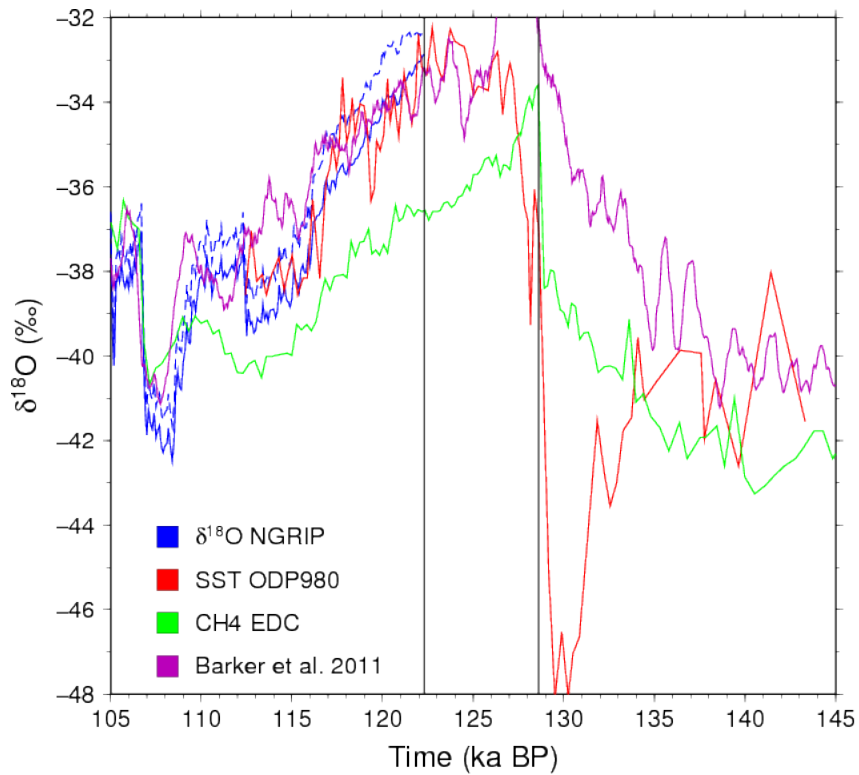


Fig. 1. Same as Fig. 1 in the original manuscript. In addition, Barker et al's temperature reconstruction is converted into isotopic anomaly following Eq. 2 in the original manuscript

C2526

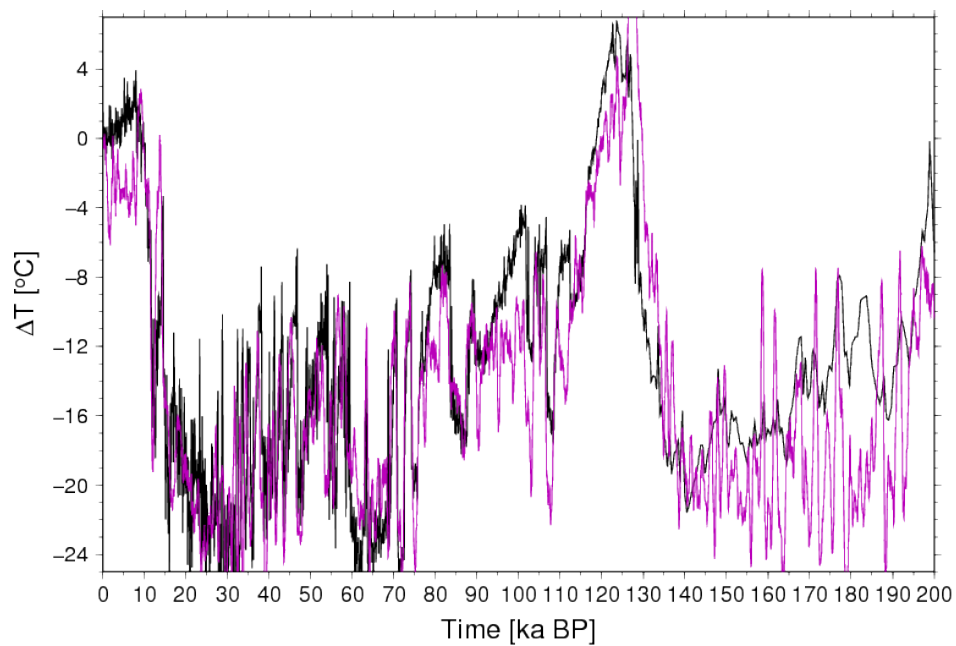


Fig. 2. Same as Fig. 2 in the original manuscript. In addition, Barker et al's temperature reconstruction is converted into isotopic anomaly following Eq. 2 in the original manuscript

C2527

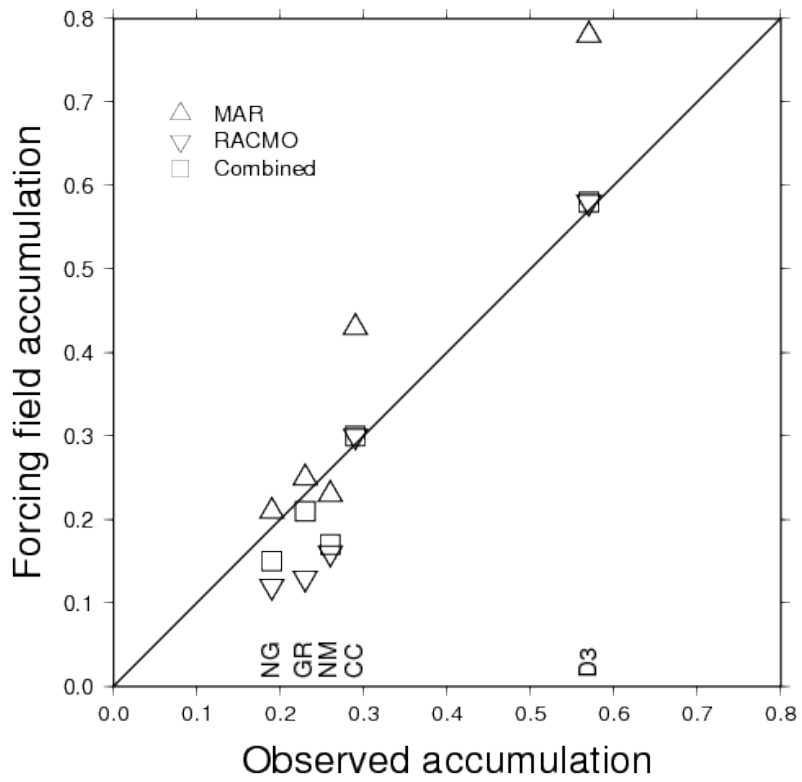


Fig. 3. Annual accumulation (m.i.e./yr) in the forcing fields (RCMs and combination) as a function of measured accumulation at the ice core locations. The 1:1 line is also represented.