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Climate of the Past Discussions

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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We sincerely thank the anonymous referee for his valuable comments, which were of great help in revising the original manuscript. Our responses (AC) to the referee's comments (RC) are given below. We hope to submit a revision version of the manuscript as soon as possible.

RC: Quiquet et al. explore the last interglacial (LIG) Greenland Ice Sheet GIS) contribution to sea level rise using an ice sheet model forced with a climate signal constructed using a combination of proxy information and climate model snapshot simulations. The approach is one reasonable way to constrain the GIS contribution to the LIG sea level high stand and I commend the authors for tackling this difficult problem. I recommend major revisions to this paper prior to potential publication, however, to address (or challenge) a few outstanding issues I have, very generally to do with: -method description and validity -discussion and evaluation of major results -implied level of originality These are addressed in more detail in 'Major Comments'. 'Minor Comments' are below. I have avoided a detailed proof-reading since I would like to see the revised manuscript and also since I think it could be proof-read in greater detail by the authors themselves prior to re-submission to reduce concept repetition and verbosity.

Major comments:

RC:-How is the composite of RACMO and MAR 12-month cycles of P and T composited (combined together) and how is the composite calibrated against accumulation records?

AC: Your question is also raised by P. Applegate. We will give the same answer: We started from the finding that the precipitation maps used (both MAR and RACMO) presented large discrepancy with the measured values at ice core locations. 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. Conversely, RACMO presented a dry bias for GRIP, NGRIP and NEEM (around 50 %), and a relatively good agreement at DYE3 and Camp Century. A simple altitudinal and latitudinal weighting of these two maps allow us to construct the composite map. The following sentence (was added to) the revised version of the manuscript: "Accumulation rates from MAR and RACMO have been compared with 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 altitudinal and latitudinal weighting between these two precipitation fields has yielded an overall better agreement (Figure X.)". A plot of the performance of the

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composite map relative to accumulation records was added to the revised version of the manuscript (please see Fig. 3 in our reply to Dr. P. Applegate).

RC: -I think the method of precipitation scaling needs to be much more justified, since it likely plays a large role in calculation of paleo-SMB. Why was this exponential form adopted and why was 0.11C chosen as default Y in the precipitation scaling? The short mention of the remarkably high sensitivity of your results to this one scaling parameter in a very important parameterization should be expanded. More discussion of this point (expanding on Fig. 8) is somewhat critical, since a critical reader could legitimately worry that you simply tuned this parameter to achieve a desired GIS contribution to LIG sea level.

AC: The exponential form for the precipitation changes with respect to temperature changes has been widely used in previous similar studies (e.g. Ritz et al., Clim. Dyn., 1997; Huybrechts, QSR, 2002; Greve et al., AoG, 2011). The reason is that this expression results from ice core layers counting (Johnsen et al., Tellus, 1989; Dahl-Jensen et al., 1993). In the revised version of the manuscript, we provide this argument and cite these studies. For the calibration, simulated age-depth relationship at ice core locations has been compared to GICC5 timescale (Sec. 2.3). The Y coefficient has been adjusted to obtain a good agreement in depth at least two major events, the Younger Dryas ($\sim$11.5 ka BP) and the Laschamp event ($\sim$40.8 ka BP). The high value of this parameter is due to this calibration. At North GRIP, an uncalibrated value (eg. 0.07, as in Huybrechts, QSR, 2002) results in a 200 m error in Younger Dryas depth and a 350 m error in Laschamp event depth, whereas after calibration of all the parameters the errors is less than 20 m (see Fig. 3). We agree that the high value of this parameter tends to reduce the ice sheet instability to climatic warming and could explain the low values we estimate for sea level rise. However, we think that our calibrated value is more appropriate here than previous values used in other studies.

RC: -Use of methane record: I agree that it is an indicator of 'climate'. But since methane signals are strongly filtered at the equator, how confident are you that Antarctic methane signals are globally representative? Ultimately, how well does the EPICA Dome-C methane actually correlate to NGRIP d18O time series, for periods where both records exist? What was the exact relationship used to convert the EPICA methane record to a synthetic extension of the NGRIP d18O record? I think it is critical that the level of correlation, and the actual derived scaling relationship, needs to actually be printed and discussed. If the correlation is poor, any derived scaling relationship between the two would be questionable.

AC: In order to create our index we plot the d18O values against the methane values for the first 122.3 ka, when records of both are available. We perform a simple linear regression to derive the conversion coefficient. The correlation coefficient of the two proxy was found to be 0.70 (based on 721 points). The following is the conversion formula used:

d18O (t) - d18O (0) = [CH4(t)] x 0.027 - 53.528

With d18O in per thousand and [CH4] in ppbv.

We cannot of course expect a linear relationship between methane concentrations in Antarctica and Greenland surface temperature. The main natural sources of methane are tropical and boreal wetlands. The main sink of methane is oxidation in the troposphere by reaction with hydroxyl radicals (OH). These OH molecules get consumed by oxidation of isoprene and other volatile organic compounds. Our understanding of how these sources and sinks evolved over the last glacial-interglacial cycle is still poor. Even if the timing of methane concentration variations was approximately the same for both hemisphere, the latitudinal concentration gradient is likely to have experienced some changes over the last glacial-interglacial cycle. In addition to these inter-hemispheric differences, we agree that methane concentration is only a rough approximation of Greenland surface temperature (Sanchez-Goni et al., QSR, 2008 for the amplitude of the DO events).

RC: -An identical argument applies to the use of SST proxies: how well does this record

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actually correlate to NGRIP d18O?

AC: Subpolar North Atlantic marine cores have already been studied for their high sensitivity to regional climate change (e.g. McManus et al., Nature, 1994). Consequently, we chose a North Atlantic marine core rather than the methane record over the last interglacial. A stronger correlation between d18O at North GRIP and ODP980-SST compared to EDC-methane was already suggested by Masson-Delmotte et al., PNAS, 2010. We used the same previously explained methodology for the conversion of SST to d18O. The correlation coefficient was found to be 0.86 (calculated on 61 points), and the linear relation used is:

d18O (t) - d18O (0) = SST(t) x 1.337 - 53.574

With d18O in per thousand and SST in degC.

RC: -I don't understand how the three records (original NGRIP d180, SST-derived d18O, and CH4-derived d18O) are combined/blended/composited to give the one composite d180 record (i.e. the one supplied in the Supplementary Information). For example, are there discontinuities when you switch from one record to another?

AC: NGRIP d18O is used for the 0 – 122.3 ka BP period. Between 122.3 ka BP and 128.6 ka BP, SST-derived d18O is used. For ages older than 128.6 ka BP, we used CH4-derived d18O. To avoid artificial abrupt changes in climate, the records were joined where the records were closed enough (at 122.3 and at 128.6 BP). In Fig. 1, we added vertical bars for 123 and 128.6 ka BP, and we edited the caption (please see the figure attached in our reply to P. Applegate).

RC: -Use of 0.35 as the default isotopic slope: similar to the precipitation scaling parameter, the ability of this non-physical parameter (in a simple but critical parameterization) to affect the results is not discussed enough, in my opinion. More justification or discussion needs to occur for using this value, again to reassure the reader that this very tunable value wasn't simply set to generate a pre-determined GIS LIG sea level contribution.

AC: Again, the response will be similar than for the precipitation ratio parameter. This parameter, along with the precipitation, are the two parameters which have the greatest influence on the simulated age-depth relationship. We indeed tuned this value to obtain a better agreement with the observed age-depth relationship (GICC05), but it has not been changed for the LIG. We acknowledge the fact that this isotopic slope has probably not remained constant during the ice ages, but only very weak constraints exist to bound this parameter.

RC: Quiquet (2012) is not the first to identify, analyze and use the GCM 'anomaly' approach in the context of ice sheet/climate modeling. See Vizcaino et al (2010) for a brief review, and link from there to other relevant studies. I recommend referencing some of these earlier studies instead of Quiquet (2012). See Pollard (2000) for a good earlier study. I think referencing earlier work is quite important.

AC: We agree, an "e. g." would have been much more appropriate. In the revised version of the manuscript, we cite earlier studies (e.g. Charbit et al., QSR, 2002; Kirchner et al., QSR, 2010).

RC: -I recommend explicitly describing how the anomaly approach is 'modified' to work with the 126 ka climate as the zero-anomaly state.

AC: The temperature perturbation provided in the Supplementary Information is the assumed temperature change with respect to present day value at NGIP, which, in our study, is taken as the temperature changes over the whole of Greenland. The no-anomaly experiment just adds this perturbation to the present day climate (RACMO/MAR fields for precipitation, and EISMINT parametrised near-surface air temperature). Without the topographically-induced temperature change at 126 ka BP, the near surface air temperature is the same as for the present, plus a uniform (throughout the year) +5 degC. This is in contrast to GCM-derived temperature estimates for 0-126 ka BP. These estimates/the GCM output is superimposed on the present day climate fields to generate a snapshot of the climate at 126 ka BP. We cannot use our temperature perturbation directly on top of these new snapshots as we would be double-counting the LIG warming. Consequently, we simply lower our temperature perturbation by 5 degC, in order to remove the LIG warming. In the revised version of the manuscript, we are trying to add the following: "The previously prescribed index was designed to obtain a zero-anomaly at 0 ka BP and has to be modified for use with the anomaly method in order to present a zero-anomaly at 126 ka BP. This modification is a homogeneous lowering of the previously described index by its own value at 126 ka BP (5 degC)."

RC: -The study uses GCM snapshots at 126 ka, that incorporate modern GIS geometry. Thus, the circulation patterns don't reflect any changes in geometry between 126 ka and present-day. Are you comfortable with the assumption that GIS at 126 ka had very similar geometry to the present-day? If it didn't, then circulation patterns generated by these paleo-GCM simulations (particularly around Greenland) are somewhat in error, compared to the real patterns during the LIG.

AC: It is more than likely that the geometry of the Greenland ice sheet during the LIG was not the same as it is today and the circulation pattern has obviously changed. We think that the change in the orbital forcings is the main driver for the difference, but we agree that a change in Greenland topography should have been included in the GCMs runs. As a first step, we have carried out a 'drastic' sensitivity study in which the CNRM model is run under 126 ka BP orbital forcing conditions, but where the whole Greenland ice sheet has been removed (exp NoG). We compared this experiment with our 126 ka BP experiment which includes the modern Greenland ice sheet topography (exp G). We calculated the difference in near-surface (2m) air temperature between NoG and G. As expected, there is a large warming over Greenland (about 20 to 25° temperature increase in NoG compared to G, depending on the season). More generally, the warming (NoG-G) north of 60°N ranges from a few °C in summer to nearly 10°C in winter. Assuming that these anomalies could scale up with the changing topography, the temperature signal caused by the modified ice sheet may indeed not be negligible. By contrast, there is no clear signal for precipitation. Without an estimate of the ice-sheet topography during the LIG to be prescribed in the atmospheric model, it is difficult to obtain more constraints. More refined sensitivity studies, involving topography estimates from our ice-sheet simulations, could be the subject of further studies.

RC: -Northern pattern of retreat is mirrored in other studies (some realizations of Stone 2010, Fyke 2011, Born 2012). Conversely, much other evidence/modelling cites significant southern dome retreat. Discussion acknowledging this debate and your experiment's contribution to it should be included. For example, I would like to see a physical explanation for mainly northern retreat in your model.

AC: From our point of view there are two possible main reasons for explaining the northern retreat in some models, but not others: Bedrock topography. While the South is dominated by a mountainous topography and above sea level bedrock, the North presents generally has a smooth retrograde slope. Several studies have reported ice sheets instability caused by these slopes (e.g. Pattyn et al., TC, 2012). In this regard, datasets of bedrock topography are highly important. Stone et al. found that the use of Bamber et al. (2001)'s dataset instead of Letréguilly et al. 1991 could to a large extent explain why the North and the South behaved differently in response to the same forcings. Ablation zone representation. In northeastern Greenland, the very narrow ablation zone of about 10 km for present day climate (Boggild et al., Journal of Glaciology, 2010), cannot be resolved by our coarse 15 km2 grid. Due to very low precipitation rates in this area, a slight error could lead to drastic changes. As raised in Quiquet et al., TC, 2012, the surface elevation change feedback on temperature is likely to amplify the collapse of the North, but dampen it in the South. More studies addressing these issues are needed. The pattern of retreat has been already discussed in Quiquet et al. (TC, 2012) and as we did not try to run GRISLI with another bedrock dataset it is not possible at this point to make a firm statement. However, we can confirm that the North is much more sensitive to climate than the South in the current version of GRISLI.

RC: -Why does including atmospheric circulation decrease GIS sensitivity? I think the authors need to more clearly identify specifically WHY including circulation (as they have) decreases the GIS sensitivity so dramatically. i worry that instead, the decrease in sensitivity is primarily an artifact of the anomaly+index approach. Also, since the atmospheric circulation change in the climate models over GIS is likely not fully correct due to both intrinsic model wind biases and use of present-day GIS geometry in these simulations, it is not clear to me that any circulation-induced change in sensitivity found here is actually realistic.

AC: We are relatively confident in our conclusion that the differences in climate between 126 ka BP and the pre-industrial period (PI) are mainly caused by changes in orbital forcing. The surface topography of Greenland probably also has an impact, but it is difficult to achieve a realistic wind pattern around the coast of Greenland with the coarse grid GCMs. GCM simulations with different topographies were being conducted at the time of writing and could not therefore be included.

There is a clear discrepancy between proxy-based estimates of temperature change and GCM-derived estimates. We think that this is the main reason for the large difference in sensitivities between the standard standard experiment (only driven by proxies) and the anomaly experiments (driven by proxies but constrained by the output of GCMs).

On the one hand, GCMs generally suffer from a lack of variability, as observed in the proxy record. In particular the amplitude of major changes is generally underestimated in complex coupled models (Masson-Delmotte et al., Clim. Dyn., 2006). The 126 ka BP GCM snapshots used exhibit a LIG climate not drastically warmer than present day conditions. In particular, even if the summer temperature are generally higher, the mean annual signal is relatively weak, with colder winters. On the other hand, for our proxy perturbation method, we rely on the assumption that d18O can directly be converted into an annual mean temperature anomaly. Our study is arguably most limited by this assumption.

RC: Minor comments:

-How much does SSA take over at the ice sheet margins? Do regions using SSA blend to SIA regions? Can you provide a map/reference to where prescribed SSA regions occur? How would you expect these regions to change given significant LIG ice sheet geometry changes?

As suggested in our response to P. Applegate, we will include in the Supplement the map of allowed ice streams we used. SSA is generally activated at the ice margins, because activation depends on the basal temperature. As the geometry changes, the basal temperature changes, causing a change in the flow of the ice streams. There is systematically a SIA component of the velocity but only the SSA component is intermittent: ice streams cannot occur everywhere and their activation depends on basal conditions. Our "allowed ice streams" mask has been constructed based on present-day surface velocity thresholds and bedrock topography curvature criteria. Including bedrock topography allows us to bypass the constraints of present-day geometry, enabling potential ice streams to expand into today's ice free areas, and beyond ice covered areas inland.

RC: -Is 15km a too-low resolution to even make use of the SSA-SIA dynamics scheme? In other words, what is the typical cross-ice-stream width in Greenland? What happens when you use SIA-only?

We acknowledge the fact that 15 km is too coarse to identify individual ice streams. We compensate this by having wider but slower ice streams. For example, surface velocity does not exceed 10 km/yr in any grid point, as observed with Jackobshavn Isbrae. Our focus is to obtain realistic fluxes. One of the major advantages of the SSA approximation compared to the SIA approximation is that the slopes at the margins are smoother and then closer to the observations in the former than in the latter.

RC: -While I agree with Quiquet et al. that simple models are very useful, it is also possible that simple models badly misrepresent the system and thus give very wrong

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results, even if supplied with ’good’ proxy data.

Nno model is perfect, each has drawbacks and advantages. It is important to have a hierarchy of models. The same can be said for the atmosphere-ocean community with the use of RCMs/GCMs/EMICs. GRISL falls in the category of models which are designed for long-term simulations. It is important to mention that GRISLI has previously been compared, for short simulations, to the most sophisticated model of the GIS, ELMER/Ice (Gillet-Chaulet et al., TCD, 2012). It was found that where ice volume is dominated by ablation (the most important process in our simulations), the evolution of ice-sheet is very similar in both models. The results of this comparison have not been published but will be presented at a poster session at this year’s AGU fall meeting (Gillet-Chaulet et al.).

RC: -During initialization, why not use the Bamber (2001) geometry, since you are using the Bamber (2001) thickness? The use of different datasets would make initial conditions somewhat inconsistent - but maybe this is not important enough to worry about.

AC: We used another dataset, in order to have a more extended domain, including in particular the Ellesmere Islands, as during glacial the Inuitian ice sheet could have had an impact on the flow of the GIS. Surface topography appears slightly ’rough’ at the beginning, but the model generate its own consistent surface after less than a few centuries.

RC: -How are modified heat fluxes near ice cores different from Shapiro and Ritzwoller, and does this result in circular anomalies in the geothermal flux field, around where ice cores exist?

AC: We applied a high value of this flux at NGRIP (135 mW.m-2), a very low value at DYE3 (20 mW.m-2) and very slight changes elsewhere. The modification is attenuated with the inverse square of the distance, within a fixed radius (225 km). This does indeed result in circular anomalies at the ice core locations.

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Interactive Comment

RC: -": : :we may have similar uncertainties regarding the LIG SMB" - this statement is unclear.

AC: As the GCMs diverge on the present-day simulated SMB of the Greenland (Yoshimori and Abe-Ouchi, 2012), we can expect even more discrepancies in the simulated SMB during the LIG due to even weaker constraints (palaeo-records).

RC: -Where are the monthly lapse rates from? Fausto (2009)? Also, can you comment on whether you think these lapse rates are dependent on geometry and changes to geometry.

AC: Yes, we constructed a seasonal sinusoidal cycle based on the values of Fausto et al. (2009). It is true that these lapse rates are dependent on geometry and therefore changes in geometry. Running an atmospheric model with a different topography for the ice sheet (e.g. Krinner and Genthon, GRL, 1999) would be very helpful. Helsen et al., (2011, TC) and Edwards et al. (2012, TCD in prep.) adopted a similar approach, to investigate the effect of changes in topography on surface mass balance. Nonetheless, a parameterisation of this kind for the topographic lapse rate would probably be model dependent. In addition, we can mention that even if this parameter is important for ice sheet advance and retreat, it is of second order importance for the climate assumption (used present-day climate, isotopic slope, precipitation ratio).

RC: -Reference Equation 2 after "assuming a simple linear relationship".

AC: Added, thanks.

RC: -Is the change in ice elevation used to correct the d180 signal derived from the ice sheet model as it runs?

AC: No. We created a similar index without taking into account the surface elevation changes and run the model a first time. The resulting surface elevation changes were then used to correct the d18O signal. A next step would be to re do the same thing again with the new simulated surface elevation changes.

RC: -For model calibration, how was the comparison between modeled and observed GIS states carried out - did you manually decide which parameter-set was best, or use an automated approach (e.g. Applegate, 2012)?

AC: We did not use any systematic minimisation. We used some numerics 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 a score-minimisation procedure would be highly useful in future work.

RC: -Why not use RACMO/MAR temperature fields in the calibration (since these are the fields are used in the actual experiments)? I would think one would want to calibrate the ice sheet model to the base climate forcing you will use in the experiments for consistency, even if that meant poorer performance at points where drill cores were taken.

AC: There was an error in the text of the manuscript. We used the RACMO/MAR combination only for the precipitation and the calibration, but also for the LIG experiments. We justify our choice of the EISMINT parameterisation on p. 3356. Thus, our calibration and experiment have been performed consistently. Of course, the error has been corrected now.

RC: Presumably RACMO/MAR gives better overall temperature fields than the idealized EISMINT field.

AC: It could be the case, but the answer is not that trivial. RACMO/MAR was run under 1958-2007 conditions which may not be representative of the pre-industrial climate which we ideally should have used. We are not arguing that the EISMINT near surface atmospheric temperature is the best available but these estimates enable us to ascertain with more confidence that surface temperature at the ice core location is in good agreement with observations, as this variable has a great impact on our results.

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Interactive Discussion

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RC: -Figure 2: why do you say "a warming greater that 5C is prescribed during the LIG?" Maybe mean "a warming of more than 5C is obtained during the LIG."

AC: Fig2 shows our temperature perturbation, which we derived from proxies. For an ice sheet model, this temperature perturbation constitutes a forcing. We will reformulate this in the revised version of the manuscript.

RC: -If models have a +3/5 summer dT at NGRIP at 126 ka but a near-zero annual-averaged NGRIP dT, does this imply that the model-derived winter temperatures at 126 ka are -3/5 colder than present-day?

AC: The amplitude of the seasonal cycle simulated by the GCMs is indeed stronger during the LIG.

RC: -One wouldn't need a full carbon cycle model to just change prescribed CO2 conditions to match Eemian values.

AC: This is true. Rewritted.

RC: -Are you sure that albedo fields remained unchanged for these GCM 26 ka simulations? I would expect albedo change in response to changing simulated Eeemian snow cover, at least.

AC: Neither IPSL, nor CNRM, contain a detailed snow scheme. Changes in albedo due to ageing of the snowpack (which depends on the rate of snowfall) are represented with a simple parameterisation. For example, the evolution of the snow pack albedo in CNRM-CM is based on a simple one-layer scheme following Douville et al. (1995), which accounts for changes in snow density.

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