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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.

Anonymous Referee #2

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Review of: Contribution of Greenland ice sheet melting to sea level rise during the last interglacial period: an approach combining ice sheet modelling and proxy data

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:

-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?

-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.

-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.

-An identical argument applies to the use of SST proxies: how well does this record actually correlate to NGRIP d18O?

-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?

-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.

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.

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

-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.

-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.

-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.

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?

-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?

-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 results, even if supplied with 'good' proxy data.

-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.

-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?

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

-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.

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

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

-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)?

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

-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."

-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?

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

-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.

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