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Interactive comment on "The importance of insolation changes for paleo ice sheet modeling" *by* A. Robinson and H. Goelzer

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We thank the reviewer for constructive comments on our manuscript. It is clear from the review that some concepts in the manuscript can be explained better. Please find a point-by-point discussion below (reviewer comments in blue italics), and the revised manuscript attached as a supplement (changed text highlighted in orange).

I write this review prior to reading any other review on the Discussion, and so my review is completely independent.

This paper develops and presents a parameterisation for including orbital forcing in a PDD mass balance scheme. It then tests the parameterisation for transient simulations through the Holocene and Last Interglacial.

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Interactive Discussion

Discussion Paper

General Comments

(1) Utility of the results. I think that the paper (Henceforth R+G) should much more clearly explain when the developed parameterisation is of use. For modelling of the Last Interglacial (and many other time period, e.g. glacial-interglacial cycles), we have a good idea of the seasonal and latitudinal temperature response to orbital forcing over the ice sheet, from GCM modelling studies (e.g. Singarayer et al). As such, 'T' in Equation (1) can have a seasonal and latitudinal component, and this seasonality will vary according to the orbit. If this seasonality is known, e.g. from GCM simulations, then the PDD scheme need not be so naive as to have a constant T, as is used in this R+G paper, but a T that can vary with month and orbit. In this case, the varying 'S' in Equation (1) is taken account of within the GCM simulation, and effectively incorporated in the time-varying 'T'. So, I would argue that in the case of time periods where we have a reasonable idea of 'T', this parameterisation is of little use (because a standard PDD scheme could be used, along with a seasonally and time-varying T, so long as it was tuned appropriately as has been done in this R+G paper).

As such, I would not agree that changes in insolation are often not accounted for by PDD schemes. If the PDD scheme uses a temperature which has been obtained from a GCM which includes orbital variability, then the T will have been obtained from a surface energy balance calculation, which does take into account changes in insolation. [e.g. p339, line 25; p338, line 3].

However, the parameterisation may be of use where we have an idea of the annual mean temperature relative to modern, but no idea of the seasonality. I think this would not happen very often, but it may be possible. As far as I can tell, the utility of this parameterisation is limited to this special case, where only a 'reduced model' is available.

For example for the LIG and Holocene transient cases, I would argue that a better (or at least, equivalent) method would be to drive with a time-varying T, rather than a

constant T and time-varying S.

We understand the reviewer's concerns, however the arguments given are not correct. Exactly what we show here (and what is shown by van de Berg et al., 2011, for example) is that a temperature anomaly only contributes to part of the total paleo melt anomaly. This is not accounted for in PDD because it assumes a constant ratio of the temperature and insolation contributions to melt. We show that this ratio changes depending on insolation, thus it cannot be captured by an imposed temperature anomaly – even if the temperature anomaly is itself induced by insolation changes.

Additionally, a constant temperature anomaly was only used in the first section of the manuscript to be able to easily isolate the insolation contribution to the melt anomaly. However, in the transient coupled simulations, a transient monthly temperature anomaly from a global climate model was imposed to show a more realistic case. It can be seen in Fig. 7 that even if insolation changes are "effectively incorporated in the time-varying 'T'", the direct absorption of additional shortwave radiation is also very important.

We have refined the discussion of the goals and the key results of this paper, to make the main points more clear.

(2) Wider applicability. Is there any evidence that the parameterisation works outside of Greenland, and for more varied orbital forcings? The parameterisation has been presented as of utility for 'paleo ice sheet modeling', and 'valid over all paleoclimatic conditions' (p348, line 26) – to back up this claim it should be tested for other ice sheets and orbits. For example, a true test of the parameterisation would be to attempt to simulate a Glacial-Interglacial cycle of the Laurentide and Fennoscandian ice sheets. This would test the model outside of Greenland, and also for inception-favorable orbits, such as at âĹij115ka.

We believe that the parameterization is applicable to other domains, as we have made no particular assumptions related to Greenland in deriving it. While additional tests

would be useful, our goal here is to quantify the impact of insolation changes on paleo modeling through an example of the Greenland ice sheet. For glacial inception, in our formulation, negative insolation anomalies lead to lower melt, but also in this case, melt is decreasing to zero so the effect is low. We have addressed this point in the Discussion, however additional simulations for other domains lie outside the scope of this work.

(3) Model robustness. Related to the above, this paper tests a new parameterisation of a model relative to a non-simplified version of the same model. However, the non-simplified model itself is never actually tested or evaluated. It is no good providing an approximation to a model if that model is itself wrong. How can the authors justify e.g. the form of Equation 1, or the constants within it, relative to observations?

The non-simplified model has been used and well validated in several published studies both for Greenland (Robinson et al., 2010, 2011, 2012; Fitzgerald et al., 2012) and for the Eurasian ice sheets (van den Berg et al., 2008). Additional references have been added to the text.

Specific Comments.

1. P339, line 3. The important thing here is the resolution and complexity of the GCM, not of the ice sheet model. It is always the GCM which is the limiting step in transient coupled simulations (unless the GCM is phenomenally over simplified)

This may be true when simulations use a coupled GCM. However, for most studies today, including our own, other more intermediate complexity approaches are used for simulating transient ice sheet evolution over glacial cycles. We have added the word "transient" here.

2. Line 1 of abstract. The second statement does not follow from the first. For example, if ocean temperature and coastal marine processes are the most important process for retreat of large ice sheets, then surface melt is much less important than e.g. calving

and marine instability.

We have clarified our logic in the manuscript to explicitly mention "climatic forcing at the surface".

3. Equation (1). What are the units of M? given that there is a Δt in the equation, I guess that M must be 'per timestep' which is odd. Here it would be good to point out that T is a constant, not varying with time? And that it does not have a latitudinal component? Also, does S vary with season/month and/or latitude?

This notation is historically consistent, but not very convenient. We have reformulated the equation to eliminate Δt.

Insolation (S) is spatially and seasonally explicit, therefore no latitudinal component is required in the melt equation. T can vary both with time and space, as this equation is applied locally at each grid point. For some idealized experiments we chose to fix the regional temperature anomaly, but of course the input temperature to the melt equation was locally determined. We have clarified this in the manuscript.

4. Also, what is 'S'?. From Figure 1 I am guessing that this might be at 65oN in June?

See above.

5. P341, line 2. I do not agree here. It is certainly possible that changing insolation could also have an effect on emissivity and albedo (e.g. through cloud changes or snowfall changes), and so it is not correct to completely separate them.

We have shown with our model that such an approach is indeed feasible to provide first-order accuracy. We have added a clarification in the revised manuscript.

Technical Comments.

Equation (2): use a symbol other than 'a'. It is too much like alpha. Use e.g. 'A'.

We prefer to keep "a".

P345, line 25: Figure 5 should be Figure 4?

This should be Figure 5 as shown.

Figure 5 is never (correctly) referenced in the text (should be around p346, line 8). Please add a legend to Figure 1.

See above. Also, a legend was provided with the original manuscript.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/8/C683/2014/tcd-8-C683-2014-supplement.pdf

Interactive comment on The Cryosphere Discuss., 8, 337, 2014.