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

Anonymous Referee #2

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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.

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.

(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 ~115ka.

(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?

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)

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.

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?

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

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.

Technical Comments.

Equation (2): use a symbol other than ‘a’. It is too much like alpha. Use e.g. ‘A’. P345, line 25: Figure 5 should be Figure 4?

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

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