The Cryosphere Discuss., 8, C292–C295, 2014
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The Cryosphere
Discussions
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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 #1

Received and published: 24 March 2014

Review of “The importance of insolation changes for paleo ice sheet modeling” by A. Robinson and H. Goelzer, The Cryosphere Discussions.

This manuscript aims to quantify the effect of insolation variability on surface melt rate of the Greenland ice sheet. Most effort is devoted, however, in developing a method to implement insolation variability into common PDD methods. In general this article is well written and the research is well performed, discussed and analyzed.

The most major comments are the following

1) The correction factor a (Equation (4)) and accompanying tuning factors (Equations (5) and (6)) make no distinction between a snow and ice surface. However, this distinction is in general made in the PDD schemes employed for ice sheet

model simulations on (inter)glacial and longer timescales. By using two different PDD factors for a snow and ice surface, the effect of the lower albedo is implicitly included in the PDD method. Given the importance of the surface properties, the method should as much information on the surface state as is commonly available in common PDD implementations. Therefore, the authors should estimate/tune a value of a for snow and ice surfaces.

2) The results in Figure 3 don’t agree with what I would expect from physical arguments. The authors should spend much more time in explaining this graph. It strikes me that for high temperatures, the value of a converges to a single value of $8.3 \cdot 10^{-10}$. I guess this value is representative for melt over ice surfaces at sea level; at least that is what I expect after using the values of Robinson et al. (2010). A little more reasoning makes me assume that higher values are thus reached for ice melt at a higher elevation, because then the transitivity of solar radiation increases. Lower values thus are found if a surface is covered by snow for a part of the month. This transient behavior can be described much better if a is specified for snow and ice separately. Still, I don’t understand (completely) why a reduces to a step function during winter.

These two points reminds me of the main concern I have against the PDD method. The PDD method assumes equal solar conditions for different latitudes and through glacial time periods. This paper proposes a method to solve the last omission but weakens the common argument for the PDD method, namely, that it is extremely simple. For PDD, one needs 1 input variable, temperature, and 3 tuning variables, namely two PDD factors, a temperature threshold. Here, 1 input variable (Solar insolation) and tuning parameter (the factor a) are included. In the ITM method (Equation 1), one has two input variables (Temperature and insolation), 5 more or less well defined parameters (τ_a, a_s (snow and ice), c and λ) and two physical constants. I do understand that people are not easily changing method, but honestly, I would advise readers to start using ITM instead of including a solar component in PDD.

3) That brings me to my third concern. As Robinson et al 2010 have shown, ITM has a better latitudinal representation of melt than the PDD method. Therefore, the authors should test their melt-correction on a PDD model driven by REMBO instead of on an ITM model with constant solar insolation (Figures 5-8). Now, they show that the correction works well using ITM, but not yet that it works well while using PDD. This affects sections 5 to 7.

Minor comments
Throughout:
The authors should keep in mind that surface melt is not equal to runoff; a significant fraction of the melted snow will refreeze. In simple ice sheet models, refreezing is not always calculated, so one needs an estimate of runoff, not of snow melt. This has to be discussed mentioned somewhere.
Solar insolation anomalies aren't equal for each latitude and month of the year. Please explain what kind of anomalies was used. Furthermore, investigate the error one makes if one single anomaly value is used instead of a monthly and latitudinal varying anomaly.

P 338, L11-13: This sentence is not clear at first reading, one understand this sentences after reading the manuscript. And 'The spatial pattern exerts'.
P 338, L19: "equally" ? Please rephrase this sentence.
P 338, L26: "that" -> "which"
P 339, L1-2: Besides this, there are marine cores with glacial deposits that give clues on the ice sheet extent, e.g., Coville and others (2011), Science 333, p 620-623.
P 339, L17-end section: At the other end of the spectrum there are PDD models that derive from one single temperature value runoff rates across the Greenland Ice Sheet. Here, however, REMBO is used, which is a model of intermediate complexity. REMBO employs ITM, and with and without insolation correction. Make clear that this middle road is used here.
Furthermore, describe in a separate paragraph what this manuscript will discuss. Now

it is mixed in this paragraph.
P 340, L11: M is defined as melt *per time step*. Please denote this explicitly.
P 341, L11-16: Give (somewhere) the dimension of a.
P 341, L21: Add something like: "For comparison, insolation anomalies typically range from -50 to + 80 W/m^2."
P 344, L 15: How is $T_{min,sum}$ determined? If by tuning, state that here.

Figure 1: Explain why the melt anomaly lags to the insolation anomaly (in June). My guess is that it due to the fact that the maximum insolation anomaly moves from spring for early Eemian to fall for late Eemian. So, around 122 ka BP July and August insolation is higher than at 125 ka BP. But this is not discussed in the text. If this is the right reason, insolation anomalies for JJA or July and August should be added in the upper figures of figure 1. Furthermore, this explanation has to be added – or if this is not the right reason – the correct reason should be added.
Figure 3: See comments earlier. Furthermore, 10e-10 is computer notation, change this to $10 \cdot 10^{-10}$.
Figure 4: Where is the dot of June?

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