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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. Please find a point-by-point discussion below (reviewer comments in blue), and the revised manuscript attached as a supplement (changed text highlighted in orange).

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.

We believe we have made a strong case that (1) changes in insolation are indeed important for simulating paleo melt and (2) that if it is necessary for a modeling group

to continue using PDD, they could at least include a correction factor for changes in insolation. Additionally the correction term is not just meant for PDD implementations, but it helps us to better understand the insolation effect. We added a few sentences to the Introduction to reflect this perspective.

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.

It is true that albedo is implicitly accounted for in the albedo method. However, it is a common misconception that this means a change in insolation is indirectly accounted for. The use of fixed PDD coefficients defines the ratio of the contribution to melt from insolation and temperature according to the present-day distribution. If insolation increases and temperature remains constant, this would imply a higher ratio of melt due to insolation, which is exactly what is missing from the PDD approach.

The reason our correction factor does not need a separate tuning for snow and ice is that our formulation of the factor "a" as a function of temperature acts as a more continuous proxy for surface albedo. In this way, all surface types are taken into account in a more continuous way rather than with a binary "snow" or "ice" distinction. We agree that other formulations are potentially possible, but we found this to be the most straightforward when balanced with the accuracy of the correction.

In the revised manuscript, we have added text to better clarify this motivation and the formulation of the correction factor.

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 Âů 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.

We appreciate the suggestion to explain these results in more detail, as they are not initially intuitive. Furthermore, we found that the diagnosed values of a shown in Fig. 3 contained extra points from the grid over the ocean that should not be included. In our model, the value of a = 8.3e-10 corresponds to a surface near sea level (tau 0.5) covered with wet snow (so an intermediate surface albedo value of 0.6). In the revised manuscript we explain the characteristics of "a" throughout the year to make this more clear. We also adjusted Fig. 3 and 4 using only ice and land points.

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 (τa, as (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.

We would agree with the reviewer here. Including additional parameters in the PDD method to account for insolation does increase its complexity to the point that it may be easier to implement a simple albedo model as Robinson et al (2010) and others have done. Nonetheless, even though options are available many groups continue to use PDD for consistency, etc. Therefore, we felt it was important to provide an alternative, which would certainly be an improvement over not accounting for insolation anomalies at all. Such a parameterization furthermore builds a bridge between more advanced methods and historically important work. We have added a couple of sentences in the Discussion to address this point.

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.

Because the parameterization is applied at each grid point, it accounts for insolation changes properly regardless of how the "present-day melt" rate is calculated. We chose to include only calculations with ITM (both full and reduced versions) to keep the manuscript concise and the results fully comparable and consistent. It is already well known that different melt models produce different spatial patterns, so it would not be very informative to show curves calculated with PDD. In other words, PDD + our correction factor is not expected to equal ITM. Our correction factor is, however, expected to properly account for insolation changes given any melt model (including PDD) that is calibrated for present-day insolation.

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.*

This is true, but in our case all simulations employ the same refreezing model. Insolation does not play a role in refreezing, or at least in any refreezing models that we know of, therefore it makes sense to quantify its contribution to melt directly. We choose not to discuss refreezing here, since our goal is not to evaluate the accuracy of simple approaches but to quantify the role of insolation changes in calculations of melt.

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.

We will make it clearer in the text that the anomalies used here are spatially and seasonally explicit. Such an approach would not work well for a single representative insolation anomaly for the whole domain, as temporal differences over the grid and seasons can be quite large.

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'.

This sentence has been improved for clarity.

P 338, L19: "equally" ? Please rephrase this sentence.

This sentence been changed.

P 338, L26: "that" -> "which"

This sentence been changed.

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.

This has been added.

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.

This has been addressed.

Furthermore, describe in a separate paragraph what this manuscript will discuss. Now it is mixed in this paragraph.

Thank you for the suggestion. A new paragraph has been added.

P 340, L11: M is defined as melt per time step. Please denote this explicitly.

Indeed, including the time step is somewhat awkward, and was a holdover from previous references. We eliminated the time step and instead provide the equations as melt rates in units of m/s.

P 341, L11-16: Give (somewhere) the dimension of a.

This has been included.

P 341, L21: Add something like: "For comparison, insolation anomalies typically range from -50 to + 80 W/m2."

This has been included.

P 344, L 15: How is Tmin,sum determined? If by tuning, state that here.

This is determined by tuning, which now has been stated more explicitly in the revised manuscript.

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.

We have now modified Fig. 1 to show the temporal changes in insolation over the whole summer season. This clearly shows that the peak summer insolation is later than the peak June insolation. This has been clarified in the results.

Figure 3: See comments earlier. Furthermore, 10e-10 is computer notation, change this to 10 Âů 10−10.

This has been revised.

Figure 4: Where is the dot of June?

This was an oversight concerning the y-limits of the figure, in part due to the masking issue noted above. This has been adjusted.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/8/C676/2014/tcd-8-C676-2014-supplement.pdf

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