Replies to Interactive comments on "Influence of ablation-related processes in the built-up of simulated Northern Hemisphere ice sheets during the last glacial cycle" from Roger J. Braithwaite

by S. Charbit et al.

General Comments:

This is a very interesting paper and tackles an important issue for ice sheet modeling. In the first sentence of his seminal paper, Reeh (1991) says "Models of the dynamics and thermodynamics of ice sheets and glaciers depend on the boundary conditions at the ice-sheet surface involving mass balance and surface temperature". Cynics will say that Charbit et al (2012) arrive at a very similar conclusion after 22 years of more work by many people. In particular, Charbit et al (2012) find that glacier inception depends critically on the representation of temperature variability for their chosen mass balance model (the positive degree-day PDD model) as well as the representation of meltwater refreezing. They do briefly discuss alternative mass balance models, e.g. the (more fundamental?) energy balance approach, although they still prefer the PDD model for modelling over the last glacial cycle.

In this review, I confine myself to commenting on the temperature variability and the refreezing, which lie well within my areas of competence. The other review, by L. Tarasov, is wider ranging.

Comment 1: *At several points in their paper (e.g. line 19 on page 4902), Charbit et al (2012) say that the PDD method was first proposed by Reeh (1991). This is untrue and must be corrected. Braithwaite (1977, 1984) was the first to formulate the contribution of random temperature variations to the positive degree-day total (PDD), and to identify the standard deviation of temperature variations (denoted by σ in the present paper) as an important parameter for calculation of PDD. The annual melting was then calculated by the MB1 model (Braithwaite and Thomsen, 1989) as the sum of monthly melt values calculated from mean temperatures for individual months using the Braithwaite (1984) model. The contribution of Reeh (1991) was to estimate these monthly mean temperatures from an annual temperature cycle described by a sine wave and then apply these temperatures to the calculation of annual PDD using the σ parameter. Aside from its applications to ice sheet modelling, the modified PDD approach of Reeh (1991) explains the long-known empirical relation between summer mean temperature and annual accumulation at the glacier ELA (Ohmura et al, 1992 and Braithwaite, 2008).*

From the above, I suggest changing "The PDD method has first been proposed by Reeh (1991)" to say "The original PDD method of Braithwaite (1984) was modified by Reeh (1991), and many workers use his modified version". I think this fairly summarises the situation and gives proper credit to both myself and the Niels Reeh.

Reply to Comment 1: We are very sorry for this confusion. In the revised manuscript we made clearly the distinction between the early work of the reviewer (Braithwaite, 1984) and that of Reeh (1991). The related modifications can be found in the introduction and in section 2. The reference (Braithwaite, 1984) has also been added in the list of references.

Comment 2: *Braithwaite (1984) calculated monthly degree-days for standard deviations in the range of 1, 2, 3, 4 and 5 deg in his Table 1 but assumed σ = 4 deg for the two examples he discussed. From my own later work, I would now prefer the lower part of this range, e.g. σ = 2 to 3 deg, but Charbit el al (2012) correctly say that the higher part of the range has been commonly used in the literature. I personally find the suggestion of Fausto et al (2009) quite plausible that σ increases from a relatively low value near the ice sheet margin, with higher temperatures, to a higher value at higher altitudes and lower temperatures.*

Reply to Comment 2: We are fully convinced of the necessity of considering the spatial dependency of the temperature variability. This has been mentioned in the conclusion (both in the initial and the revised version of the manuscript)

Comment 3: *It is still difficult to describe meltwater refreezing in a physically realistic way that is computationally economic (Reijmer et al. 2012). Reeh (1991) refreezes all melt up to 60% of the annual accumulation and allows runoff for melt in excess of this 60%. He gives no explanation or reference for this 60% figure, which therefore looks very arbitrary and ad hoc, but there is a sound physical basis for this figure. In the late 1980s, Niels Reeh and I discussed the refreezing of meltwater, and Reeh was aware of my MB1 mass balance model (Braithwaite and Thomsen, 1989) where refreezing was estimated using a simple density mixing model, see equation (4) in Braithwaite et al (1994). In this model, snow must be converted to impermeable ice by meltwater refreezing before any runoff is allowed. The ratio of annual melt to annual accumulation at the runoff line is given by the densities of impermeable ice and pre-melt snow. With the values that we assumed for these densities in the late 1980s, the melt/accumulation ratio at the runoff line is 0.6, and Reeh (1991) assumes this value without explanation. After a very strenuous field trip in spring 1992 to measure firn densities in the lower accumulation area of the Greenland ice sheet, Braithwaite et al (1994) refined this ratio to 0.58. Pfeffer et al. (1991) estimate a ratio of 0.7 with a more refined refreezing model but Braithwaite et al (1994) suggest this can be adjusted down to 0.62 if the Braithwaite density values are substituted into the Pfeffer equation. Shumskii (1964, p. 416) suggests a melt/accumulation ratio of only 0.4 at the runoff line but that can be adjusted upwards using the densities from Braithwaite et al (1994). The simple density mixing model (Braithwaite et al, 1994) does not allow any runoff from melting snow, while in real life there may be some runoff from a shallow snow cover, with underlying ice, if the snow has been brought to the melting point by latent heat release from refrozen meltwater. The density mixing model may therefore slightly overestimate the amount of refreezing.*

I hope the reader will not regard the above comments as too much of a historical digression but the simple point is that the refreezing scheme of Reeh (1991) is more physically based than it looks and may even be more realistic than some later schemes.

Reply to Comment 3: Thank you very much for all these explanations. In the revised manuscript, we introduced a summary of this comment in section 2 and, as suggested, we insisted on the fact that the refreezing scheme of Reeh (1991) is more physically based than it looks. We also added (Braithwaite, 1994) in the list of references.

Some detailed comments

Title of paper: “build-up” and not “built-up”.

This has been corrected in the title and in other places in the manuscript

.Lines 2-3 on page 4899: Please give a standard definition of mass balance.

A standard definition of the surface mass balance has been given following the recommendations found in the *Glossary of Glacier Mass Balance* edited by the International Association of Cryospheric Sciences (2011).

Lines 22-24 on page 4899: In my papers, I always cite Finsterwalder and Schunk (1887) for stating that melting only occurs under positive temperatures, which is certainly important for the development of the PDD concept, but the word "method" here is going too far.

We replaced the word "method" by "concept

Lines 26-28 on page 4899: Yes, it is fair to say that Reeh (1991) "proposed a new formulation of the PDD method".

Line 13 on page 4900: Does anyone suggest using a single DDF value for snow and ice? I think the authors mean one DDF value for snow and another DDF value for ice.

This is exactly what we wanted to mean. This has been more clearly specified in the revised version of the manuscript.

Line 19 on page 4902: As said earlier Reeh (1991) did not first propose the PDD method. See remarks above.

As mentioned previously, this has been changed (see "Reply to Comment 1 above)

Line 12 on page 4902: There is no real reason to assume σ = 4 deg.

We added this latter remark. Note however that we did not mentioned σ = 4°C, but σ = 5°C

Line 20 on page 4902: The cited author's name is "Braithwaite" and not "Braithwaithe".

Sorry for this typo. This has been corrected.

Lines 23-25 on page 4902: The maximum amount of superimposed ice cannot exceed 60% of the annual accumulation and the 60% figure is not "somewhat arbitrary" (see my comments above).

See Reply to Comment 3

Lines 6-23 on page 4903: Again, the PDD formulation of Reeh (1991) is not "early".
You should put in a reference to Braithwaite (1995) somewhere in this paragraph as he reviews these the high DDF values and shows that extrapolation of the surface energy balance to lower temperatures will lead to higher values of DDF. When performing the analyses described by Braithwaite (1995) I was more concerned about the possible changes in degree-day factors with rising temperatures, for example under global warming, than with changes at lower temperatures.

This reference has been added.

Equations (4) and (5) on p. 4905: I am concerned that these inequalities will give "steps" in computed mass balance, although they do partly take account of the temperature dependence of the degree-day factors predicted by the energy balance model (Braithwaite, 1995).

We added a sentence just after Equations (4) and (5) stipulating that these equations do not account for the impact of changing surface conditions without changing climatic variables.

Line 5 on page 4907: There is no real justification for σ = 4.5 to 5.5 deg.

We fully agree with Roger Braithwaite. However, in what follows we explain that the parameter may vary with geographical locations and altitude according to Fausto (2009). Moreover, we previously added a similar remark following the recommendations of the reviewer. Therefore, we do not think necessary to add again the same remark in the manuscript.

Sections 3 on page 4907 to section 5 ending on page 4920: I have no comments.

Lines 18-19 on page 4920. The whole purpose of Braithwaite (1995) was to study variations in degree-day factor by extrapolating the energy balance. Although not the immediate purpose of the present paper, I think somebody should do more work in this area. It may be correct that energy balance models are not suitable for on-line forcing of ice sheets over longer time scales but temporal and geographical variations in degree-day factors could be more thoroughly studied by running advanced energy balance models off-line, i.e. by updating the basic approach of Braithwaite (1995) with more sophisticated energy balance models at more sites.

A similar comment was inserted in the Conclusion section of original version of the paper. In the revised manuscript, we developed this idea following the recommendations of Roger Braithwaite.