Response to Referee #2

Willeit and Ganopolski show the importance of considering the effects of snow aging and dust on the snow albedo and consequently on satisfactorily simulating glacial cycles. The article is well written and its relevance is properly justified. In my opininon, the novely of the paper does not lie directly on the results but on the presentation of the parameterizations for accounting on the mentioned effects on snow albedo. Accordingly, the main weakness of the study is reproducibility. The authors should expand on the snow albedo parameterization in order to other groups being able to reproduce (and benefit from) the current study.

We would like to thank the reviewer for his comments on our manuscript. We have responded to the issues raised by the reviewer below. The original reviewer comments are in black, our responses in blue.

General comments

About reproducibility:

Ice sheet – climate coupling represents a considerable ongoing effort for modeling groups. The authors of this article have already convincingly shown in previous studies the necessity of accounting for the snow albedo reduction from ice aging and dust in order to successfully simulate a deglaciation. This article furtherly contributes to this idea and presents the needed albedo parameterizations to do so. This later aspect can be of great importance to groups currently starting to couple GCMs to thermodynamical ice sheet models. Thus, these parameterizations need to be accordingly described.

In the revised version of the paper we included a more detailed description of the albedo parameterization in the model.

1. In page 3, line 14, the snow age factor parameterization is described:

1.1 It might be obvious, but the reader could wonder whether the aging of the snow can simply be computed as a function of temperature and snowfall. Please, elaborate on this and add references.

In reality, the aging of snow is a very complicated process and physically based modeling of snow aging effect on albedo requires high resolution multilayer snowpack models forced by realistic synoptic variability of all climate and hydrological components (e.g. Brun et al., 1992; Flanner et al., 2007). And, as intercomparison projects show, even such complex models simulate very different snow albedo. Needless to say that such approach cannot be used in the CLIMBER model. This is why we have no other choice as to use the only two available characteristics – temperature and precipitation – to parameterize the effect of snow aging on surface albedo. These two characteristics exert primary control on snow aging effect because the growth rate of ice crystals depends strongly on surface temperature while frequent precipitations reduces average size of snow crystals at the surface of snow cover. Our parameterization is simple but still captures the first order effect. This is definitely much better than doing nothing because, as our manuscript shows, this effect is vitally important for simulating realistic glacial cycles.

1.2 The definition of T_0 is missing.

Has been included.

1.3 The age factor is used to represent the grain size. And Fig.1 shows grain radius. How is CLIMBER-2 translating each other? It is linear? Please provide the related expression.

The relation between snow grain radius and snow age factor is now included in the paper.

1.4 Fig.1: Besides the pure snow case, CLIMBER-2 seems to be underestimating the albedo compared to the two other parameterizations. Why? A potential explanation is given by the sentence: "... explained by the choice of the imaginary refractive index of dust". Please, be more specific. On the other hand, the effect of the alternative parameterizations on simulating the glacial cycle is described in the Results section, but it s not explained. I imagine this can simply be a matter of "tunning". Re-calibrating the age factor (or other components of the model) for the two alternative approaches will produce a successful ice-volume evolution. If this is the case, please acknowledge in the paper. Otherwise, the reader remains wondering about the realism of the different approaches.

Yes the difference can be mainly explained by the choice of the imaginary refractive index of dust, which, as shown in different studies and as also mentioned in this paper, varies largely as a function of mineral dust composition. One value might be more appropriate for one source of dust and another for a different source. It is therefore problematic to use one constant global value. On the other hand, tracking poorly known properties of dust based on its origin seems challenging.
In the revised version of the manuscript we now mention that a retuning of the model could possibly allow to successfully reproduce glacial cycles even with alternative albedo parameterisations. The message of the paper should not be that a successful simulation of glacial cycles is possible only with the CLIMBER-2 representation of albedo, but that slightly different schemes could result in very different outcomes.

2. In page 10, the effects of considering aeolian and glaciogenic dust individually are discussed. The interactive aeolian dust representation is conveniently described in previous studies. I could not, however, find the equivalent for glaciogenic dust. How is glaciogenic dust generated in CLIMBER-2? Please provide the necessary information. Furthermore, when Fig.7 shows glaciogenic dust as a necessary condition for a full deglaciation.

More details on the processes generating glaciogenic dust in the model are included in the revised version of the paper. Additional details can be found in Ganopolski et al. (2010), Appendix A.

About discussing the necessity of including a dust cycle:

In the Conclusions section it can be read: "In this study we used an Earth system model of intermediate complexity to show that a proper parameterisation of snow albedo over ice sheets is a crucial ingredient for a successful simulation of the last glacial cycle." This and previous studies from these authors support this conclusion. Nevertheless, other models/groups have shown succesfull glacial cycle simulations without the necessity to invoke "a proper parameterisation of snow albedo". For example, in Abe-Ouchi et al 2007 CP and 2013 Nature, the ablation-isostatic adjustment feedback together with elevation and other feedbacks appear to represent enough processes to simulate the deglaciation.

First, the deglaciation simulated by Abe-Ouchi et al. is not so good. Fig 1d in Abe-Ouchi et al (2013) (and this is obviously their best simulation) shows that Northern Hemisphere ice sheets with the volume corresponding to 20 meters in sea level survived the last deglaciation. This is a lot and by our standard such experiment cannot be considered as successful simulation of deglaciation. Second, Abe-Ouchi et al (2007, 2013) used the PDD scheme. This scheme does not account for albedo at all but it can melt a lot of ice when necessary, partly for the wrong reason (see Bauer and Ganopolski (2017)). In that paper we demonstrated that with properly selected PDD parameters, we also are able to simulate a reasonably realistic glacial cycle (see Fig 10 in Bauer and Ganopolski, 2017). This fact, however, tells us nothing about the importance of surface albedo. The later can only be studies with the models which are based on the physically sounded energy balance approach.

The current main conclusion (see above) of this paper give rise to interesting related questions: Could CLIMBER-2 simulate a deglaciation without considering the effects of dust on snow albedo? If affirmative, which are then the key processes? Are those other processes equally realistic? Is all the relevant physics necessary for understanding deglaciations already contained in EMICs? ... I understand that the authors could see these questions as out of the scope for the current article, but I also believe the readers will appreciate further the current paper if a discussion on this aspect is included.

We here repeat what we have already written in response to one of the comments of reviewer #1. The fact that CLIMBER-2 is capable of reproducing glacial-interglacial cycles is the result of many years of work, which included model improvements, inclusion of new processes based on new process understanding and obviously also tuning of unconstrained model parameters. Even with a relatively fast model like CLIMBER-2 it is practically impossible to explore the whole parameter space and it could well be that other parameter combinations could lead to a successful simulation of glacial cycles. This is beyond the scope of the present study. The goal of this study is to clearly show that surface albedo plays a crucial role for ice sheet evolution and not only do fundamental processes affecting snow albedo, such as snow aging and dust deposition, play an important role, but even using slightly different parameterisations of the same process (e.g. the effect of mineral dust impurities on snow albedo) can lead to qualitatively very different results. This is a clear message to readers who are interested in simulating glacial cycles and more in general the long-term evolution of ice sheets.
We will include a bit more discussion on this aspect in the revised version of the paper.

Specif comments:

Page 1, line 10 and 14: Please use “light-absorbing ...” as later in the paper.

Changed.

Page 3, line 8: add “in” afer “snow albedo used...”

Done.

Caption figure 9: erratum: glaciogenic

Fixed.

References

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Flanner, M. G., Zender, C. S., Randerson, J. T. and Rasch, P. J.: Present-day climate forcing and response from black carbon in snow, J. Geophys. Res. Atmos., 112(11), 1–17, doi:10.1029/2006JD008003, 2007.