Reviewer 2

Page 1, Line 10 to 18*: This paragraph of the abstract is somewhat confusing. It would be good if the authors could revise this section; I would suggest either by explaining the simulation setups in more detail or by putting more emphasize on the results and less on the simulations setup, given that they will introduce the setups in detail later.*

Abstract will be revised.

Page 3, Line 17 to 20*: Please introduce here the "offline" and "online" PDD approaches. This will help to understand what is meant by those two approaches (as they are not explicitly mentioned in the Section "Model description"). To understand the difference is crucial for interpreting the results.*

We will describe the difference between "offline" and "online" simulations in section 2.3 "Positive-degree-day (PDD) method" and in the Discussion section.

Page 4, Section 2.2*: The PDD approach is described in detail but the SEB approach is only briefly mentioned. Although the reference Calov et al. (2005) is given additional information regarding the setup would be useful. How is the downscaling from the 7x18 atmospheric grid to the higher resolution SICOPOLIS grid done? How are certain processes regarded when downscaling (e.g. height desertification effect)? Further, it would be good to mention that a one-layer snow model is used. Please also introduce the parameterization of the albedo, given that changes in the albedo of the ice sheet seem to be crucial for the simulation of the last glacial cycle.*

A detailed description of the surface energy and mass balance scheme (SEMI) is given in Calov et al. (2015) and it is not possible to repeat all details here. However, for readers' convenience we will add a paragraph where we briefly describe the coupling procedure and major parameterizations.

Page 6, Line 29-31*: While discussing the differences between the American and European ice sheet I am wondering how well CLIMBER represents the interactions between the two ice sheets. Previous studies (e.g. Liakka et al., 2016) have shown that the European ice sheet is significantly influenced by the American Ice Sheet. While dis- cussing reasons for the different responses of the*

European and American ice sheets these processes should be shortly discussed in regards to the presented results.

It is difficult to compare our modeling results with Liakka et al. (2016) because they performed equilibrium time slice experiments while we performed transient experiments. In the model running over the orbital time scales, ice sheets are never in equilibrium with climate. In our simulations, the Laurentide ice sheet does exert a strong cooling over the North Atlantic and significantly influences the European climate. However, it is important to note that due to coarse spatial resolution of CLIMBER-2, we only account for thermally driven atmospheric stationary waves but not for topographically forced. The omission of the latter may affect long-distance climate teleconnections.

Page 12, Discussion*: While the results clearly indicate that the SEB approach is superior to the PDD approach for simulating the last glacial cycle it would be good to point towards the weaknesses of both approaches. This might be covered by a more detailed description of the SEB in the method section (see above) or one or two sentences in the discussion section. Further, how realistic are the SEB results? Most of the results are integrated over the Northern Hemisphere but how is the spatial distribution? It could be good to see e.g. a comparison between the ice sheets derived with the SEB approach during LGM in comparison to LGM reconstructions on a spatial map.*

We believe that weaknesses of the PDD approach are obvious from our study. The SEB approach is entirely physically based and therefore the only right but of course, its implementation in the model, which does not simulate synoptic and intra-annual climate variability, requires a number of assumptions and additional parameterizations. We will discuss this is the Discussion section. As far as the performance of our standard run is concerned (including spatial distribution of ice sheets) it is discussed in detail in Ganopolski et al. (2010).

Page 11, Line 14-16 and Conclusions*: The authors state that the American melt depends largely on the snow melt factor, which can be attributed to the effect of dust deposition. I think the authors need to clarify how dust deposition and snow age interplay in the model. Is the albedo change a linear function of the snow age/dust or do other factors play in? What is the relationship between*

snow age (simply changes of snow properties) and dust deposition? Could it be other factors that cause these differences?

The albedo scheme is described in Calov et al (2005). Indeed, there is an interplay between aging and the effect of dust on snow albedo – the latter is stronger for the “old” show. We add further information in the Discussion section. We will insert a new Figure 12 (thereby replacing the former Figure 12) to show ablation series in response to the aging effect of pure snow and the aging effect of impure snow.

Page 11, Line 25 to 31*: Fig. 12 needs to be explained better. Please clarify this paragraph. Currently it is hard to follow the reasoning.*

The former Figure 12 will be removed. Please see our response to the comment above and also to Reviewer#1.

Minor Issues

The manuscript will be revised according to all minor issues.