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Interactive comment on "The Greenland ice sheet: modelling the surface mass balance from GCM output with a new statistical downscaling technique" *by* M. Geyer et al.

Anonymous Referee #1

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This paper presents a statistically based technique to downscale 150 km GCMs outputs to 15 km with the aim of forcing an ice sheet model. This downscaling technique is based on a (too ?) simple relationships between the SMB components and near-surface temperature changes. Finally, they discuss the impact of their downscaling techniques to their future projections.

However, in respect to papers recently published in TC (e.g. Helsen et al. (2011), Franco et al. (2012), Edwards et al. (2013), Fettweis et al. (2013), . . .), this paper is not really innovative and the presented method seems to be too simple to reliably force an ice sheet model. However, I suggest to accept this paper only if the authors resolve all the critics listed hereafter, even if I am aware that this could be a big job for them.

1. Franco et al. (2012) showed that SMB can be reliably extrapolated (using time and spatial varying corrections) to resolutions 2-3 times lower that the original one by running a RCM at different resolutions. Here, there is a factor 10 between the raw outputs and the extrapolated ones and constant (in space/time) corrections are used !! In addition, the downscaled output are only compared with K-Transect measurements and not with a run of the model at higher resolution. CNRM-CM5 could be run at 50-75km of resolution on the 10-yr snapshots to check the downscaling (in particular for the precipitation).

2. Section 3.1: The authors use a constant (in time and space) temperature lapse-rate to correct the 150 km x 150 km TAS to the sub-grid 15 km x 15 km topography when they extrapolate TAS at 15 km. What is the value of this lapse-rate ? Knowing that this technique is mainly based on TAS differences, using a constant lapse rate is too simple and unjustifiable in view of the spatial and temporal (e.g. winter vs summer) variability of the temperature vertical gradient ! For example, the melt amount over the 15 km x 15 km ablation zone is quadratically depends on the chosen gradient ! Using a temperature lapse-rate based on the neighbour CNRS-CM5 pixels for each pixel and time step is more reliable and not difficult to compute!

3. Section 3.2: For downscaling the 150 km precipitation at 15 km of resolution, they use the same correction for the whole ice sheet and this correction is based on the temperature. Firstly, as already shown in Fettweis et al. (2013) and confirmed in this study, the precipitation variability is very poorly correlated to the temperature variability. Secondly, the precipitation variability is very spatially depend. As explained in Franco et al. (2012), higher elevations mean in some areas/climates higher precipitation and in other areas/climates lower precipitation and therefore, a constant correction (in space and time) can not be used here. Finally, the 150 km x 150 km smoothed topography used in CNRM-CMS5 can not resolve the 15 km x 15 km sub-topography (as shown in Fig.9) and then, some processes as the barrier/foehn effects can not be simulated at a resolution of 150 km. Except by using RCMs or physically based disaggregator (e.g.

Agosta et al., 2013), 150 km x 150 km precipitation can not reliably be interpolated at 15 km x 15km by using a simple temperature-based correction, since the 150 km CMRM-CM5 topography compares very badly with the 15km ETOPO1 based one (Fig. 9).

4. Section 3.3: The authors claim that their snowmelt relationships increase less with temperature than Franco et al. (2013). The snowmelt increase from Franco et al. (2013) is mainly due to the expansion of the ablation zone (i.e. the bare ice areas) as the result of the snowpack compaction and successive summers with very high melt rates over the current climate percolation zone. In this study, the authors use 10-yr snapshots but it is likely not enough to allow the expansion of the ablation zone. How does CORCUS be initialised at the beginning of the 10 years ? Is there a spin-up time depending on the 10-yr climate conditions ? Why using 10 yrs snapshot and not a continuous simulation with CROCUS? The memory of the snowpack over Greenland is generally higher than 5 yrs.

5. Section 3.6: In section 3.5, SMB seems to be the sum of the downscalled SMB components but in Section 3.6, the authors seem to downscale directly SMB. It is not clear what they use.

6. Section 4: The authors valid their outputs over current climate with RCM outputs and K-Transect measurements. But, their results seem to be calibrated to mainly have reasonable SMB rates at the scale of the whole ice sheet in average. Knowing that their aim is to force ice sheet models, how does their model compare locally with the RCM outputs or ERA-Interim-CROCUS? Showing differences in Fig. 6 should be useful. Along the K-transect, they associate their disagreements with the observations to the surface albedo. But the albedo spatial variability is mainly driven by the precipitation spatial variability and a validation of their 150 km x 150 km precipitation fields is needed here since their 150 km x 150 km topography is bad. Line 21, pg 3179, the authors critic the ERA forced CROCUS simulation due to quality of the precipitation fields from ERA as a result of the smoothed topography used in ERA (given at a resolution of ∼75

km). This suggests that using 150 km x 150 km precipitation fields is even less justified ?

7. Section 5: The authors explain that the melt/elevation feedback is not taken into account as they use a fixed topography. But the well known melt/albedo feedback, which should be larger, is also not taken into account in respect to the Fettweis et al. (2013) simulations for example. Therefore, before coupling CNRM-CM5 with an ice sheet models, coupling CNRM-CM5 with CORCUS seems to be more priority. Finally, they claim that the CNRM-CM5 forced simulations miss the recent melt increase because of the CNRM cold temperature bias. But, as explained in Fettweis et al. (2013), the recent melt increase is mainly the result of general circulation changes (negative NAO conditions) in summer (the melt has been normal in 2013 because no anomaly in NAO). But, as the other CMIP5 GCMs, CNRM does not project changes in NAO and this is the main cause why CNRM does not simulate the recent melt increase.

Ref:

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