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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.

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Anonymous Referee 1

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.

The main objective of this work was the development of an efficient and simple SMB downscaling technique of GCM outputs in order to run the GRISLI GrIS model. Indeed, nowadays there are already various SMB downscaling methods of different degree of complexity (e.g. Gallée et al. 2011, Helsen et al. 2012, Edwards et al. 2013, etc.), . The method uses primarily the near-surface air temperature, the solid precipitation, the snowmelt and the SMB of the GCM, which already allows to better represent the surface energy balance than if only temperature is used (as, for example, in the case of the Positive Degree Days technique, PDD). In order to better explain the method, we added the following block-scheme (Fig.1) into the revised version of the paper. In order to be applicable to outputs from any CMIP5 GCM, the statistical laws were designed for the whole GrIS without considering different regions as done in e.g. Helsen et al. 2012, Edwards et al. 2013. As noticed in the paper (Sect.6, P.3186, L.11), we fully admit that this results in smoothing the real spatial SMB gradients, which may hamper the accuracy of the method. Nevertheless, even if the method may seem to be too simple compared to other techniques, a detailed evaluation, that we present in the paper, shows that the method performs well (e.g. Fig.6 b, c vs. d; Fig. 9b; Fig. 11), including the capacity to reproduce the inter-annual variability of the SMB. As we considered different climate scenarios to calibrate it, we claim that the found statistical laws represent the actual characteristics of the link between Greenland SMB and temperature. Thus these laws can be applied to the outputs of not only CNRM-CM5.1 GCM, but also to any other GCM. Moreover, the corresponding analytic formulae that we derived are not actually linked to any particular spatial resolution and therefore can be successfully used for GCM output of any resolution. Only the horizontal interpolation from the input

GCM fields will be different (see block II in the scheme Fig. 1). This makes the method universal and useful, meeting the need for a fast, simple tool for SMB downscaling, that can be used online to refine SMB spatially within a coarse-grid GCM. In order to support our claim that the method is general, we prepared Fig.2 (not included in the paper –> Figure 2: Top row: Annual mean raw SMB simulated by CNRM-CM5.1 at 150-km resolution (a), 50-km resolution (b), and MPI-ESM-LR at 200-km resolution (c). Middle row: same as top row, after horizontal bilinear interpolation to 15-km resolution (d), (e) or 5-km resolution (f). Bottom row: (g), (h) and (i) represent the SMB after statistical downscaling of respectively (d), (e) and (f).), which illustrates the use of our downscaling method for preindustrial simulations run by three different models run: 150km CNRM-CM5.1 to 15km, 50km CNRM-CM5.1 to 15km and 200km MPI-ESM-LR to 5km. In particular, it shows that the downscaling method significantly smoothes the initial SMB differences between the low-resolution (LR) and the high-resolution (HR) runs of CNRM-CM. Furthermore, it makes it possible to derive a realistic very negative SMB along the South-Western ice-sheet margin which was not simulated in the initial LR and HR CNRM-CM runs. Hence, even if other more sophisticated tools do already exist (but cannot be easily transposed to IPCC-class GCMs such as CNRM-CM5.1), the main question we want to answer in our paper is: what is the added value of the downscaled SMB compared to the coarse grid SMB modeled by the GCM ?

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).

As mentioned in the previous point, due to the design of our downscaling technique,

the very different resolution between the coarse and fine grid should not cause fundamental problems. So, when we first submitted the paper, we did not compare the downscaled 150-km resolution SMB with downscaled SMB from a higher resolution run. However, our downscaling technique has been evaluated in various contexts: We validated the results of SMB downscaling along the K-transect by comparison with the in-situ observations, as noted by the reviewer The output from Crocus forced with ERA-Interim (mainly snowmelt), which was used for establishing the statistical laws, was validated against MAR RCM simulations driven by ERA-Interim for the time period 1981-2011. As shown in the paper the results are in good agreement (Fig. 6d, f; Fig. 10-11). Finally, we validated the downscaled and simply horizontally-interpolated CNRM-CM5.1 SMB with a high-resolution simulation of SMB performed with Crocus forced by atmospheric forcings extracted from CNRM-CM5.1 simulations (Fig. 6a, b, c, d; Fig. 10-11). As shown in the paper, the downscaling of CNRM-CM5.1 SMB improves the representation of SMB, compared to only interpolating the CNRM-CM5.1 SMB. The downscaled SMB is closer to that simulated by MAR and Crocus than if only an interpolation is applied. This is especially clear along the ice sheet borders, where the altitude difference between the actual and the GCM surface orography are largest. In addition to the previous points, that we presented in the first version of the paper, and in order to meet the reviewers' expectations, we performed an additional 50-km resolution simulation that we ran with CNRM-CM5.1. Without any correction, the SMB produced by this simulation is clearly more realistic that in the 150-km resolution (see Fig. 2a and b). We applied our downscaling technique to this simulation (see Fig. 2h). However, compared to the downscaled 150-km resolution simulation, the initial differences are considerably reduced. This supports that our approach, which consists in downscaling SMB within our low-resolution coupled system is a reasonable alternative to running our coupled system at a much higher resolution, which would be too computationally expensive.

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

In our work we were mostly interested in establishing statistical laws between precipitation, snowmelt, SMB on the one hand and SAT on the annual scale as our ice sheet model which we are coupling to GCM is driven by annual means of these variables. As written in our paper (P.3179, L.5), we follow Fausto et al. 2009, so that the annual value lapse-rate is considered homogeneous over the whole GrIS and is set to -6.309 °C/km (P3176, L7). However, we truthfully note (P.3186, L.11) that since we choose to fit annual mean data with functions of only one variable, namely annual mean SAT, the data on the basis of which we establish our statistical laws presents some spread due to different SAT annual cycles. In the paper, we also shortly discuss the possibility to decrease this spread by using multiple regression on the basis of monthly data. We also performed such a multiple regression on the basis of two predictors – annual and July mean SAT and compared its output with a one-predictor regression (only annual mean SAT). We found that on the one hand the two-predictor regression provides more stable statistical laws for precipitation, snowmelt and SMB. But on the other hand it increases the complexity of the technique, while the final results of the downscaling through these two techniques are very close.

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).

Our results are not in contradiction with Fettweis et al. (2013) which show that precipitation variability is poorly correlated with temperature variability, or Franco et al. (2012) about the complex physics of precipitation formation in different regions of the GrIS. In Fig. 1a of the paper we plotted the precipitation rate against SAT for different climate scenarios, showing a relatively large dispersion (R^2=0.57). While recognizing that there is no systematic relationship between SAT and precipitation, and that local effects can be at play, the found statistical relationship links precipitation changes with SAT changes over the whole GrIS over an annual basis. Similar strategies linking the precipitation rate and SAT have been successively used in previous modeling studies (e.g. Ritz et al., 1997; Huybrechts, 2002; Greve et al., 2011, Quiquet et al., 2012). Hence our technique builds upon past experiences. An important feature is also that our exponential coefficient is very close to those used in these works (P3172, L5). Observational studies, like Dahl-Jensen et al. (1993) where past accumulation rates are reconstructed from records of ice-cores, provide a similar link between temperature and accumulation rate anomalies.

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.

The referee pointed out an important point which is clarified in the revised version of the paper. We fully agree that the expansion of the bare ice area is a critical process which impacts the GrIS SMB, acting as a source of positive feedback at time scales of more than several years . Crocus accounts for this process via the possible apparition at the surface of old snow and ice layers at grid points where the snow layers accumulated during the cold season have melted out. Multi-year snow and ice layers have a specific albedo in the model. However, the SMB simulations with Crocus over the GRISLI grid were not designed for the prediction of the future evolution of the GrIS but for an evaluation of the SMB downscaled from CNRM-CM low-resolution SMB. Since the latter uses only a very simple uni-layer snow model D95 which does not distinguish snow from ice, we decided to start the different Crocus simulations from the same 2D initial state. This 2D initial state was obtained from several 30-year spin-up runs over the period 1980-2010 with meteorological forcings from ERA-interim, to ensure a 2D variability in equilibrium with the variability of the 2D climate conditions. Only 10-year snapshots were used for Crocus simulations because the necessary forcing data from CNRM-CM were not available over the whole period.

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

We agree this was not clear in the previous version of the paper. According to the defi-

nition of SMB (Eq. 1), in Sect. 3.5 we obtain the SMB statistical law (Fig.4) as the sum of the corresponding statistical laws for solid precipitation (dashed line), sublimation (dashed-dot line) and snowmelt (dotted line). The sum forms the SMB statistical law, without any statistical fitting to the observed SMB data (Fig.4). The fact that the SMB total curve fits well (R^2=0.83) the distribution of the SMB data confirms the relevance of the found statistical solutions. Then in Sect 3.6 we simplify the SMB statistical law (neglect sublimation, which is 1 to 2 orders of magnitude smaller than the other terms) and express it as B(T) = Ps(T) + R(T). The downscaled SMB is the sum of the downscaled snow precipitation and of the downscaled melt. This part has been rewritten, and hopefully clarified in the revised version.

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 ?

The revised version of the paper now includes the differences for fig. 6 and the corresponding discussion. As for the second part of the reviewer comment, indeed, sentences starting Line 21, pg 3179 did not intend to criticize the ERA precipitation field. At the contrary, they wanted to notice that the SMB simulated with Crocus over the K-transect is not very much degraded by the relatively low resolution of the ERA-interim precipitation field. It must be reminded that all Crocus SMB simulation results shown in the paper are those directly simulated by the model from 3-hour ERA-interim meteorological fields (no statistical treatments have been applied neither to the forcing data nor to the snowpack model outputs). We are aware that ERA-interim cannot resolve all the variability in precipitation induced by the complex topography from the coast to the top of the K-transect, affecting more or less the SMB simulations. Therefore, the relatively good agreement over the K-transect is quite encouraging. The paper has been revised in order to clarify this.

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.

The inability of the current snow scheme D95 of CNRM-CM5 to take into account the important melt/albedo feedback is a known weakness of our model which indeed limits our simulations. For the time being we are not planning to run Crocus within CNRM-CM for long-term ensemble simulations because of its much higher computational cost, but also since the implementation of Crocus in CNRM-CM is not compatible with the current structure of its surface scheme. In the case of CNRM-CM simulations, even if we attributed the explanation the underestimation of the melt increase to the too high snow albedo, we felt the reviewer's point about recent NAO anomalies was definitely

worth a more detailed investigation. Since it cannot be expected from any particular GCM to reproduce NAO anomalies within a given time-frame, it is actually rather difficult to assess the respective contributions of model biases and its own variability to the differences with observations. To this end, we performed an additional simulation (1981-2011) where temperature and pressure in CNRM-CM5.1 were nudged with ERA-Interim above the model's fifth vertical level (about 900 hPa). A new figure was inserted in the paper (reproduced here as Fig. 3 → Figure 3: Different quantities averaged over the GrIS. The curves in black and red respectively depict the MAR and nudged CNRM-CM5.1 simulations. Top row: Annual mean near-surface air temperature (left, in °C) and annual precipitation (right). Bottom row: annual snow melt rate and surface mass balance (respectively left and right)). Fig. 3 shows that despite the temperature nudging, the annual mean temperature is biased to the cold side. However, this bias is much more pronounced in winter than in summer bias and does not explain the lack of summer melting seen in the bottom left panel. By contrast, without any direct constraint on humidity, the total precipitation is very well simulated by CNRM-CM5.1 in nudged mode, particularly in terms of interannual variability (correlation with MAR: 0.86). The decrease in GrIS annual mean surface mass balance is still underestimated by the model, and this is clearly due to an underestimate of the surface melting by the model. To conclude on this point, this experiment demonstrates the efficiency of the downscaling techniques provided the low resolution GCM is able to reproduce the actual atmospheric circulation, with limitations due to residual model biases.

The revised paper is in the supplemental material.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/7/C2795/2013/tcd-7-C2795-2013-supplement.zip

Interactive comment on The Cryosphere Discuss., 7, 3163, 2013.

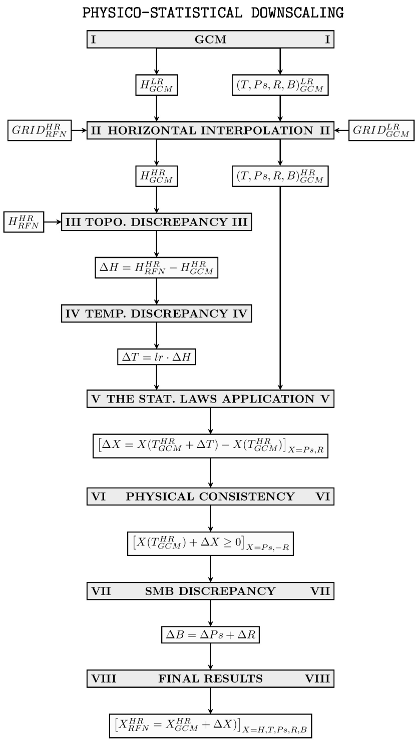

Fig. 1. Figure 1

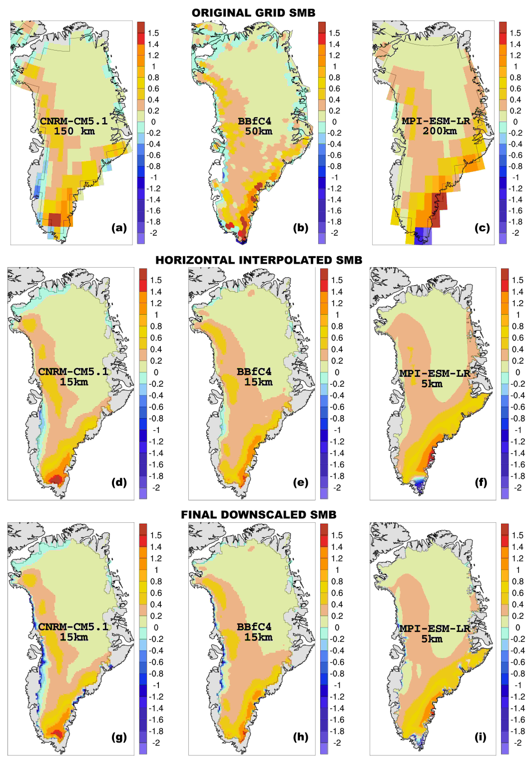

Fig. 2. Figure 2

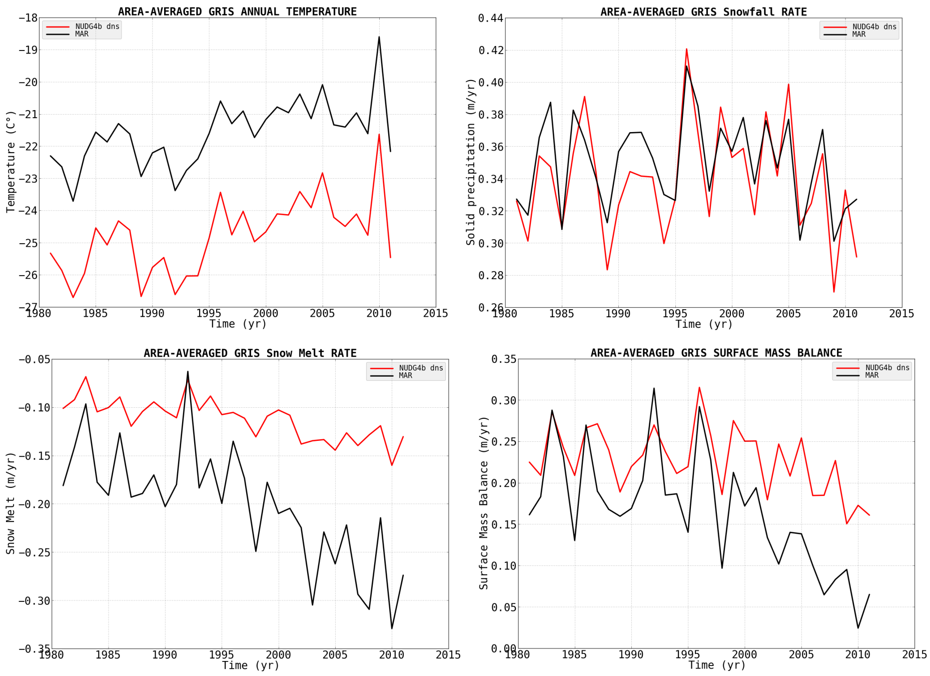

Fig. 3. Figure 3