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Discussions
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Interactive comment on "Improving a priori regional climate model estimates of Greenland ice sheet surface mass loss through assimilation of measured ice surface temperatures" *by* M. Navari et al.

Anonymous Referee #1

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Review of

Improving a priori regional climate model estimates of Greenland ice sheet surface mass loss through assimilation of measured ice surface temperatures

by Navari and others

Summary

This paper presents a proof-of-concept of assimilating remotely sensed surface temperature (Ts) data into fields from a regional climate model (RCM) to improve the rep-

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resentation of near surface climate and mass loss fluxes over the Greenland ice sheet (GrIS). The method is essentially applied offline (i.e. not applied while running the RCM) and takes advantage of the correlation that exists between Ts and near surface temperature, melt, evaporation/condensation and incoming shortwave/longwave radiation. These correlations are then used to update, in a single step, the near-surface output fields of a regional climate model. The updated fields are subsequently used to force an offline snow model (CROCUS) to re-assess GrIS mass loss terms. Because of lacking remotely sensed Ts data when it is cloudy and other data quality issues, current time series of Ts over the GrIS are too sparse for a direct application/test of this method. Instead, a synthetic time series was produced that deviates substantially from the median; results show that the data assimilation brings the data closer to the (synthetic) truth, with a more narrow uncertainty band.

General comments

The science of data assimilation into regional climate models is original and important. Moreover, this application to the Greenland ice sheet (GrIS) makes the study suitable for publication in The Cryosphere. The methods -including the uncertainty analysis- appear sound, and the figures are of good quality. However, the paper is too long, as is the reference list. The paper is written in a rather technical style, which in combination with the length makes it a hard read for the non-specialist. If these and the issues raised below are addressed, the paper can be published in TC after which, what I believe are, relatively minor revisions.

Major comments

The introduction at places reads like a review article, which is also reflected in the amount of citations. Please select only the most relevant studies to cite, and also try to avoid duplicate citations (i.e. citing the same paper several times). To shorten the remainder of the paper and improve its readability, consider moving part of the methods to an appendix.

It is unfortunate that a lack of data precludes a real practical application at this time. Not only would it present a stronger proof of the applicability of the technique, it would also show that the available data are up to the challenge. The fact that this paper presents a proof of concept should be reflected in the title ("e.g. ...feasibility of...") and also discussed in the main text.

Nothing is said about how data assimilation in general could help improve the forcing models; i.e. by assimilating vertical profiles of humidity (using for instance radio occultation) into an RCM, its prediction of e.g. precipitation could also be improved. Could results be further enhanced by including MODIS albedo? Please comment.

p. 3214, l. 6: "Surface and sub-surface melting (which ultimately contribute to runoff) are dictated by the evolving snow temperature driven by energy inputs." This statement is not formally correct. Surface melting is driven by the SEB imbalance once T_s reaches the melting point, after which it remains constant. So during melting, variations in T_s cannot be used to infer melt rate. Moreover, subsurface melting in a model is only possible when subsurface heat sources are allowed, such as the penetration of shortwave radiation; it is not clear whether this is the case in CROCUS. If so, please mention it; if not, subsurface melting cannot take place.

p. 3214, l. 10: Equation 2 misses a source term associated with the refreezing of percolating meltwater. It is true that the SEB determines the upper boundary condition (T_s) to force snow temperature, but in Eq. 3 melt is (I assume, because it is missing from the equation) incorporated in Q_g, which is normally assigned to the subsurface conductive heat flux. Not explicitly including melt in the SEB equation is not logical, as Q_g can still be nonzero under non-melting conditions. Please consider to reformulate the SEB equation.

Technical comments

Abstract, l. 7: "... there is considerable disparity between the results from different methodologies that need to be addressed." But these disparities are not addressed in

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this paper.

p. 3208, l. 11: " While recent estimates..." The estimates listed are made for certain periods; it is now well known that over recent years surface processes have outpaced dynamical changes (e.g. Enderlin et al., Geophysical Research Letters, 2014).

p. 3213, l. 6: Please consider reducing the amount of citations.

p. 3213: is MAR not also forced at the top by ERA-Interim? If so, please mention this.

p. 3214, l. 6: what is meant by 'subsurface melting'? Is shortwave radiation allowed to penetrate the snow/ice? Otherwise, no heat sources would be available to enable subsurface melt in a model.

p. 3227, l. 19: "Sublimation and evaporation play an important role in the GrIS surface mass loss 20 (Lenaerts et al., 2012) and after runoff are the main components of the GrIS SML." This is true for drifting snow sublimation, which was included in Lenaerts (2012) but not in CROCUS or MAR. Ordinary surface sublimation is typically three times smaller than surface and drifting snow sublimation together.

p. 3218, l. 22: " uncertainty of precipitation estimates from different modeling frameworks are less than that of the other terms (Fettweis, 2007)". Vernon and others (2013) show that this is not true in general; moreover, large intermodel differences occur also in melting, refreezing....