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Interactive Comment

Interactive comment on "A minimal, statistical model for the surface albedo of Vestfonna ice cap, Svalbard" *by* M. Möller

Anonymous Referee #2

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Review of "A minimal, statistical model for the surface albedo of Vestfonna ice cap, Svalbard" by M. Möller

GENERAL

This manuscript deals with the development of a statistical model that aims to simulate the surface albedo of the Vestfonna ice cap (Svalbard). Only precipitation and temperature input fields are used to drive a statistical model that calculates surface albedo throughout the spring, summer and autumn seasons. The model is developed and tested using a suite of 64 monthly (8 years times 8 months) altitudinal profiles of surface albedo observations using the MODIS MOD10A1 albedo product.

The manuscript is generally well written and has been illustrated carefully. Its structure is clear and the writing is concise. It tries to tackle a relevant problem, namely to quantify how net shortwave radiation, a dominant term in the summertime surface energy budget of glaciers, changes under future climate forcing.

However, I have some difficulties understanding the rationale for developing this statistical model, especially because the introduction of this manuscript appears to me as strawman rhetoric, in which drawbacks of commonly used albedo methods are presented that I don't see, and the solution that is proposed suffers from the same shortcomings as the traditional models for albedo evolution. I will explain this below.

The manuscript highlights repeatedly (abstract line 5, 11; section 4.1) that it is built on temperature and precipitation fields. However, all fitting parameters are functions of altitude as well. This introduces different behaviour of albedo at different altitudes under the same forcing. So even if temperature and precipitation are equal at two different altitudes, the response of the albedo model will be different for these altitudes. In reality, it doesn't work like this: snow doesn't know at which altitude it sits. Of course, this is a statistical model and not a physically based one, but making a model so dependent on the present altitudinal distribution of albedo makes me worry about the application of the model in a future climate. While the author can convince be of a good behaviour of the model for present-day conditions, I am not too convinced that it "is suggested to be fully suitable for further application in broader energy or mass-balance studies..." (abstract line 22-23). An albedo model that is so tightly coupled to altitude will have problems to react to future forcings.

SPECIFIC POINTS

- Section 1: as mentioned above, I find the motivation for developing this model not well presented. Other albedo schemes are disqualified for Vestfonna because snow drift allegedly disturbs the albedo pattern. However, in section 4.5 it is discussed that the deviation between the results of the statistical model and the observations may be due to the same snowdrift processes. So first snowdrift is used as an argument to dismiss other models, later it is used to explain deficiencies in the new model. Furthermore, it is argued that traditional albedo models are not useful because the spatiotemporal resolution of the input data would be insufficient for such models. However, for any climate reanalysis or future projection scenario product, daily means are available that can perfectly be used in existing albedo schemes. Admittedly, the altitudinal interpolation will be a sensitive factor, but that is not different from the statistical model in this study. The use of the word inappropriate in line 5 of page 984 is inappropriate.

- Section 3.2: The assumption that MODIS albedo values represent the actual albedo is taken rather lightly. MODIS is known to have a severe bias at high zenith angles (Wang and Zender, 2010, RSE). The author counters this argument by stating that the comparison between MODIS and a temporary AWS on Vestfonna does not warrant a correction, but this is no good reasoning. That no seasonal signal in the bias is seen in this comparison does not mean that MODIS can be taken as is. There could be problems with the AWS, or with the representativeness of the AWS site for the MODIS pixel. Another paper by Wang and Zender (2010, JGR(F)) propose a correction for albedo at zenith angles higher than 55 deg, based on extensive comparison between MODIS and AWS observations in Greenland. The bias is structural and can be explained physically. Therefore, the bias for Vestfonna is also well constrained, even though the author cannot find it when comparing to AWS data. As a result, the Wang and Zender correction should be applied to the MODIS data in this study as well. This is critical for publication.

- Another small issue in section 3.2 is that MODIS scenes are biased towards clear days, when observed surface albedo is lower because of the lack of clouds. The author may want to include a brief discussion about this unavoidable bias.

- Section 3.3.1: A constant linear lapse rate of 7 K/km is taken for temperature. Over a melting glacier surface in summer, this lapse rate may be very different, just because the melting surface inhibits further warming of the air above it. Do the AWS observations on Vestfonna give an argument for using different lapse rates under melting

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conditions?

- Section 3.3.2: On what is the scaled precipitation profile in equation 5 based? If it is based on in-situ observations, then are the net effects of snowdrift processes not already included? In the discussion, snowdrift is used to explain discrepancies between model and observation, but if its effects are implicitly taken into account in eq. 5 then that is not valid.

- Section 4.1 and figure 2: I keep having trouble understanding why exactly there is a different response to the snow-rain ratio at different altitudes. It means that the model is strongly conditioned on present albedo behaviour at individual altitudes, which makes me skeptical about the use of the model for future projections.

- Section 4.2: it would be instructive to present not only RMSE values in figure 4, but also the mean difference between observation and model as a function of time. This tells the reader how well the temporal evolution is simulated by the model.

- Section 4.3: Cross validation of a model is always difficult when you have limited validation data. Leave-one-out cross validation (LOOCV) is an accepted statistical tool to cross-validate a model, but an important prerequisite is that the data must be temporally decorrelated. For albedo, this is certainly not true on a monthly time scale. Therefore, the current implementation of LCOOV is a misuse of the method. An possible solution would be to assume that the albedo evolution in one year is decorrelated to adjacent years (to some extent a reasonable assumption). Then, you could apply a LOOCV technique on the 8 years, by developing 8 models with 7 complete years as a training set and 1 year as a validation set. In statistical terms, this is a k-fold cross correlation, but with a specified partitioning of the subsamples, namely grouped per year. On a side note, I find the reference to Marzeion (2012) for the LOOCV not appropriate because this technique has already been well established for decades. Addressing the cross validation is critical for publication.

- Section 4.3: the mismatch between observed and modelled albedo is given in albedo units, but physically this is a meaningless statistic. Albedo means must be weighted with incoming solar radiation to be meaningful. Or better still, model performance should be expressed as RMSE and mean difference in net shortwave radiation rather than in albedo. A crude estimate of available shortwave radiation per month may be derived from AWS data on Vestfonna. In this way, the qualitative statements in section 5, lines 20-22, can be quantified better.

- Section 4.3: The systematic underestimation of albedo in the accumulation area may be acceptable, but that is only because the intended setup in glacier melt models does not provide a feedback between the albedo and the surface energy budget. In reality, having your albedo wrong by -0.1 (figure 5) will have dear consequences because there is a very strong positive feedback between albedo and melt.

REFERENCES

Wang, X., and C. S. Zender (2010), Constraining MODIS snow albedo at large zenith angles: Implications for the surface energy budget of Greenland, J. Geophys. Res. Earth Surf., 115, F04015, doi:10.1029/2009JF001436

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