**The Cryosphere
Discussions**

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

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PREAMBLE:

I very much thank the two reviewers for their thorough analysis of my article and for their valuable comments, annotations and suggested improvements. They had been carefully considered and most of them will be accounted for when revising the manuscript. Comments and corrections regarding the writing of the manuscript will also be widely accepted. Answers and explanations to all detailed questions and annotations raised by the reviewers are provided in the following.

ANSWERS TO COMMENTS BY REVIEWER 1:

Main comments:

RC1-1: My only concern with the method is that in the cross-validation procedure persistence and periodicity have not been considered, i.e., the leave-out-window is always one data point. For instance, the paper cited for the cross-validation method (Marzeion et al., 2012) considered persistence in the record. Please discuss why you did not account for autocorrelation and seasonality in your albedo record. Also, this would be a good opportunity to spend one or two sentences on the cross-validation method, and direct the reader to recent glaciological applications (Marzeion paper as you do, but also Hofer et al. (2010) on which Marzeion builds, or Koppes et al. (2011)). I am rather sure that many cryosphere researchers are not familiar with this method.

AC: The reviewer is right with this criticism of the cross-validation procedure. The albedo record indeed shows significant autocorrelations as the albedo evolution throughout the months of each mass-balance year is a continuous process and the albedo pattern of a specific month partly builds upon the situations in the months before. However, as the type of cross validation used will be changed from a leave-one-out cross validation to a k-folds cross validation in accordance with a suggestion of reviewer 2 the issue raised by reviewer 1 will, by then, not be given anymore (cf. AC to RC2-10). The comment regarding a more detailed description of the applied cross-validation method will be accounted for as part of the updated cross-validation section 4.3 and more comprehensive references will also be given.

RC1-2: The paper has an unusual structure, which makes it a bit hard to follow - even if it is very well written otherwise. In particular, there is no obvious "results" section, so at least you should re-name your section 4 "Model description and Results". Or put the model description in section 3 ("Data and model") and save section 4 for results.

AC: I understand the difficulties described by the reviewer regarding the differing structure of the paper without a clearly delimited results section. This is because of the fact that the aim of the paper is to present a newly developed type of model. Hence, the model description and the successful validation can be considered as the "results" of this study. To account for the justified annotation of the reviewer, an additional, short

paragraph will be added at the end of the introduction section that guides the reader through the structure of the paper.

RC1-3: In connection with equation (10), how do you use the term "logistic function"? To my knowledge logistic regression is a special type of generalized linear regression models (GLMs), and the logit model is often used to predict probabilities of an event. Please clarify in a brief sentence (and perhaps a reference) for the readers what your logistic function is (advantages, etc.).

AC: A logistic function is a specific type out of the family of sigmoid functions. This family of functions is used to describe many relations in natural sciences. It comprises several specific types of functions that are defined for all real numbers in x-direction and have a value range in y-direction that is delimited by two horizontal asymptotes. All types of sigmoid functions can be forced to the same upper and lower asymptotes. However, the slopes along their curves are dependent on the specific type of function. During model development different types of sigmoid functions (including hyperbolic tangent etc.) have been tested but the performance of the selected "logistic function" was best. To avoid any further misunderstandings within the readers of the paper the term "logistic function" will be replaced by the more general term "sigmoid function". This terminology can be considered as a sufficient characterization of the fitted type of curve as its formula is given in complete detail within the paper.

RC1-4: You say that initial MODIS albedo data are daily, from which you construct monthly values. Are the daily data sampled at the same time of day by the satellite throughout the year? Or is there more than one overpass per day (Aqua and Terra?)? Please clarify in section 3.2.

AC: The MODIS sensor that acquires the data used for automatic generation of the MOD10A1 surface albedo fields used in the presented study is carried by the Terra satellite. This is a polar orbiting satellite that covers the entire globe on a daily resolution. Satellite overpasses in regions near the poles are thus numerous each day (10

acquisitions of Vestfonna ice cap each day). The albedo information used for producing the daily MOD10A1 data are taken from the respective scene acquired closest to nadir. An additional sentence specifying these facts will be added to section 3.2 in order to give the reader a better understanding of the data basis that underlie the employed MODIS albedo data.

RC1-5: Precipitation from ERA-interim; Could you briefly indicate in section 3.3 how confident you are in these data? Precipitation from reanalysis data sets is usually problematic. I realize you cite your own work in this respect, but one sentence would probably be appreciated by readers.

AC: The downscaling of the ERA-Interim precipitation data proved to produce reliable results at the regional scale of the ice cap. The polynomial function used to distribute precipitation over altitude was calibrated by Möller et al. (2011a) using in situ measured snow water equivalent data obtained from a large set of snow profiles acquired over parts of the ice cap (Möller et al. 2011b). The reliability is underlined by the fact that the given relation holds for four consecutive accumulation seasons as stated in Möller et al. (2011a). According to the reviewer's suggestion an additional paragraph that briefly summarizes these facts will be added to section 3.3 in order to give the reader a clear picture of the reliability of the precipitation data used.

RC1-6: Figure 7: It is hard to see the main message in this figure. Wouldn't it be easier for the reader if you show the deviation as histogram for each parameter instead of the scatter plots? Also, why is observed albedo shown? As I understand the point of interest here is the deviation from the reference model, not from the observed albedo.

AC: The suggestion of the reviewer of showing the albedo sensitivity towards the model's parameters as histograms instead of the bubble plots in figure 7 is already accounted for in slightly different sense in figure 8. The box plots presented in this figure represent a condensed version of histograms differentiated in both space (altitude) and time (months). The scatter bubble plots of figure 7 relate FAM-modelled

albedo to observed albedo instead of IAM-modelled albedo because of the following: The model sensitivity section is not part of the calibration/cross-validation of the FAM. It is intended to give the reader the opportunity to comprehend how strong potential errors in the calibration of the model's parameters would impact on the modelled albedo values. Hence, a relation to the IAM is not mandatory in this sense. The choice of observed albedo as part of the scatter plots has been done in order to give the reader an additional impression of the overall modelling performance of the FAM when compared to MODIS-derived "real" albedo fields.

Minor comments:

RC1-7: 983/16: "(e.g., Brock ...)"

AC: The respective text passage will be changed accordingly.

RC1-8: 983/17-18: I am not sure if you can cite work in preparation. Maybe "Sauter, pers. comm."

AC: The respective text passage will be changed accordingly.

RC1-9: 983/19: delete "distribution and" (spatio-temporal variability already implies a distribution)

AC: The respective text passage will be changed accordingly.

RC1-10: 983/23: "it varies mainly with terrain elevation." - At this part of the paper a reference would be good.

AC: This statement is backed by the analysis of the MODIS data used to build the albedo model. No other studies on the surface albedo of Vestfonna ice cap exist so far and a reference could thus not be given. However, a sentence will be added that explains that the respective statement is derived from MODIS data.

RC1-11: 984/5: replace "inappropriate" by "difficult". It is not inappropriate to do high-resolution calculations, if the data basis permits.

AC: The reviewer is right and the respective text passage will be changed accordingly.

RC1-12: 985/13: Please specify "They".

AC: "They" refers to the "precipitation sums" of the previous sentence. The two sentences will be connected in order to clarify the meaning of "They".

RC1-13: 994/6: "slightly" (typo)

AC: The typo will be corrected.

RC1-14: 996/12-13: "should be fairly constant" (add fairly); There are also errors in the input data that could account for the non-constant parameters, even if all responsible driving forces would be captured by the input variables.

AC: As suggested by the reviewer, the word "fairly" will be added to the respective sentence.

RC1-15: Table 1 caption: "deviations of xxxx air temperature"; Please add the time scale for xxxx (daily?).

AC: The specification "daily" will be added to the table caption.

RC1-16: Figure 1: For convenience it would be good to add a scale bar and label the contours.

AC: The contours will be labeled according to the reviewer's suggestion. A scale bar is not necessary in my opinion because of the metric UTM coordinate system used in the map. Hence, I would like to refer from adding a scale bar for reasons of map simplicity and thus clarity.

RC1-17: Figure 2: "versus xxxx surface albedo"; Please add the source for xxxx (MODIS?).

AC: The term "MODIS-derived" will be added to the figure caption.

RC1-18: Figure 3: "Table 2" (typo)

AC: The typo will be corrected.

RC1-19: Figure 4+5 captions: You must explain what the boxes, vertical lines and crosses represent. Not all readers might be familiar with this type of plot.

AC: The reviewer is right, a short description of the box plots is missing in the captions of figures 4, 5, 6 and 8. Hence, it will be added to the caption of figure 4 and referred to in the other three captions.

RC1-20: Figure 8 caption: Define the colors (it is quite clear in connection with Fig. 7, but each figure needs to be self-explanatory).

AC: A hint towards the consistent colour context within figures 7 and 8 will be added to the caption of figure 8.

RC1-21: Figure 9: I would remove the y-axis break. Simply start the y-axis at 0.02 or 0.03.

AC: The y-axis break will be removed and the new axis will start at 0.02 according to the reviewer's suggestion.

ANSWERS TO COMMENTS BY REVIEWER 2:

Main comments:

RC2-1: 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.

AC: This initial and very general comment is specified by the reviewer as part of other comments (RC2-3, RC2-7 and RC2-8). It is thus answered in detail in the related ACs.

RC2-2: The manuscript highlights repeatedly (abstract line 5, 11; section 4.1) that

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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 me of a good behavior 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.

AC: The reason for different responses of the model to similar forcing at different altitudes lies in the varying influences of snowdrift processes on two of the three input variables of the albedo model, i.e. cumulative snowfall and rain-snow ratio. This fact is mainly related to curvature variations along the elevation profile of the ice cap as described in detail in my AC to the more specific but content-related RC2-8. In this AC I also discuss the question of future applicability of the model.

RC2-3: 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 inter-

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polation 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.

AC: The aim of this study is to present a minimal albedo model, i.e. a model that is based on as little as possible input data. In the here presented case these are air temperature and precipitation only. Most of the existing albedo models also rely on these types of input data but additionally also include a temporal component related to snow age that is incorporated as a discrete model parameter. However, this is the critical issue that leads to the fact that these kinds of models could not be applied at Vestfonna (and probably most of the large Arctic ice caps). Snow age is primarily influenced by the elapsed time since the previous snowfall. However, strong snowdrift conditions completely destroy this relation as they also result in newly deposited snow (cf. AC to RC2-7 and AC to RC2-8) and it would thus be necessary to implement a snowdrift modelling scheme when these types of albedo models should be applied. This, in turn, would blow up the complexity of the model substantially and would, most probably, prevent any application in long-term future projections due to limitations of computational power. Hence, under the conditions present in the study region a statistically based albedo model without a discrete, daily-resolution, temporal parameter is needed. I have to admit that this main chain of reasoning is not clearly presented in the introduction as it is in its present form and the reviewer is thus right with his criticism. Hence, the introduction will be revised accordingly so that it will clearly outline the above stated context. Together with other revisions that will be done in relation to the snowdrift issue raised by the reviewer (cf. AC to RC2-7 and AC to RC2-8) this will draw a much clearer picture of the intention of the presented model and the necessity of its development.

RC2-4: 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, 2010b). The author counters this argument by stating that the comparison between MODIS and a temporary AWS on Vestfonna does not warrant

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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 (2010a) 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 (2010a) correction should be applied to the MODIS data in this study as well. This is critical for publication.

AC: This is the only reviewer comment that I have to refute for a number of reasons outlined in the following. Indeed the reviewer is right that MODIS albedo data are known to have proven biases or inaccuracies under high zenith angles. However, there are different MODIS albedo datasets around that cannot be treated equally in this respect. The reviewer comment builds upon statements regarding the MCD43 datasets that are analysed in detail in several papers by Wang and Zender that recently provoked some criticism (Schaaf et al., 2011). My study, however, employs MOD10 datasets. These two products (MCD43 and MOD10) are based on distinctly different processing algorithms and moreover present different final products (Stroeve et al., 2006). The MCD43 datasets contain separate black sky albedo (BSA) and white sky albedo (WSA) information while the MOD10 datasets directly provide a linear combination of both. This fact per se prohibits the application of the proposed Wang and Zender (2010a) correction as this one refers to the MCD43 data structure only. Moreover, there are profound arguments for the reliability of the here used MOD10A1 dataset under conditions at Vestfonna ice cap: Argument 1) Wang and Zender (2010a) build their argumentation and correction algorithm on the fact that raw MCD43 albedos get unrealistically low from a physical point of view under high zenith angles. As outlined in the manuscript, this behaviour was not at all observed at Vestfonna ice cap for the MOD10A1 data used. Regarding this fact the reviewer points to potential AWS inaccuracies. However,

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this is rather unlikely as up to 6 AWS were run on Vestfonna and adjacent glaciers since 2008 (partly unpublished data) and none of their records show a corresponding bias between MOD10A1 and in-situ measured albedo values. Hence, as the Wang and Zender (2010a) correction is developed for a substantially different MODIS product under substantially different framework conditions that could not be proven to exist at Vestfonna for the MOD10A1 product used, it is per se not justifiable to apply any similar correction in the here presented study. Especially as a correction algorithm for the MOD10 product family has not been developed and/or evaluated so far. Argument 2) Stroeve et al. (2006) evaluated the performance of the MOD10A1 daily albedo product over the Greenland ice sheet, i.e. the same region where also Wang and Zender did their studies. They found mean biases between MOD10 albedo and MCD43 BSA/WSA of 0.04/0.08, with MOD10 values generally being higher than MCD43 values. The largest differences occur at lower albedo values of less than 0.7. According to Wang and Zender (2010a) the lowest albedo values of MCD43 contain the largest errors as the inherent systematic bias reduces albedo to unrealistically low values. Hence, as MOD10 albedos are generally higher than MCD43 albedos the errors can per se be considered to be smaller in MOD10 than in MCD43. This argumentation is in accordance with findings of Stroeve et al. (2006), i.e. with their comparisons of MODIS albedos with AWS in-situ measurements on the Greenland ice sheet. Argument 3) Schaaf et al. (2011) recommend a limitation of the usage of MCD43 data to dates with local noon zenith angles of below 70°. Among other sites, they show that a considerable negative bias in the MCD43 albedo data at the NGRIP site in northern Greenland (comparable latitude to Vestfonna) occurs only in periods with a local noon zenith angle of above ~70°. At Vestfonna ice cap the local noon zenith angle is below 70° in the period mid April to end of August and thus over the entire ablation season. As the presented albedo model is intended for usage in mass-balance studies, this is the period where reliable results are crucial. This means that even the MCD43 albedos that can be considered to have larger errors than the ones of MOD10 (cf. Argument 2) are eligible for usage in the here presented study. Hence, it is reasonable to assume

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that MOD10A1 albedo data can be employed all the more. Résumé) I very much like to refrain from doing any correction of the employed MOD10A1 albedo data because of the reasons outlined above. However, I have to admit that comprehensive additional explanations and discussions regarding potential errors in the MODIS data have to be added to section 4.5 of the manuscript. Especially regarding the application of the model in future mass-balance projections and in case of a potential extension of the ablation season further into September under conditions of future climate warming it is crucial to inform the reader about the zenith angle issue and discuss the resulting implications for albedo-model accuracy. Taken together, I think that it is better to discus the influence of a potential error then applying a correction that is based on facts whose validity could not be proven for the study site. Hence, I aim at extending section 4.5 by additional, related paragraphs outlining the major facts of the above presented context.

RC2-5: 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.

AC: This valuable suggestion of the reviewer will be accounted for by adding an additional paragraph describing this issue to section 3.2.

RC2-6: 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 conditions?

AC: So far, the analysis done on AWS data acquired on Vestfonna does not reveal any clear systematic variations in air temperature lapse rate. Moreover, the given value of 7.0 K km-1 has been successfully applied in a climatic mass balance model of Vestfonna that was calibrated using in-situ stake measurements distributed over the entire altitudinal range of the ice cap (Möller et al., 2011a). This supports the reliability of the

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used air temperature lapse rate.

RC2-7: 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.

AC: The scaled precipitation profile given in equation 5 is indeed based on in-situ snow water equivalent measurements presented by Möller et al. (2011b) and thus implicitly accounts for snowdrift influences on accumulation. However, there are differences between the influences of snowdrift on accumulation and on surface albedo characteristics from a process-based point of view. With respect to the variables rain-snow ratio and cumulative snowfall the process of mass transfer during a snowdrift event is relevant. With respect to albedo it is the alteration of reflectivity of the snow surface that matters when referring to the relevant process. Both of these processes yield transient results that are, however, measured on different time scales. While the snow-pit derived precipitation profile yields the final outcome of all snowdrift related mass transfer during the entire winter season (long-term result), the MODIS-derived albedo data represent a specific situation at a given instant of time (short-term result). Hence, any given snowdrift event is differently represented in the two datasets used for model calibration. Regarding mass transfer, the entirety of the influence of the snowdrift event is covered by the dataset. Regarding albedo alteration, the transient influence of the snowdrift event, i.e. the instantaneous albedo variation at a specific instant during the event, is captured in the MODIS scene. As the presented albedo model, however, calculates on a monthly resolution, these short-term albedo variations during any snowdrift event are averaged before being included in the calibration procedure. Hence, the model indeed implicitly includes the effects of snowdrift on rain-snow ration and cumulative snowfall as outlined by the reviewer. However, the different temporal aspects with respect to model resolution and MODIS observation result in potential discrepancies

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between modelled and observed surface albedo. This rather complex context was indeed not described adequately in the manuscript and I thank the reviewer for detecting and bringing out this shortcoming. Accordingly, section 3.3.2 will be extended by an additional paragraph describing the background of the given precipitation profile. Moreover, the corresponding parts of the discussion section will be updated by additional explanations outlining the above given context.

RC2-8: 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.

AC: The model shows different responses to similar rain-snow ratios at varying surface elevations because of the varyingly strong influences of snowdrift on surface conditions due to curvature variations along the elevation profile of Vestfonna. Curvature is one of the terrain characteristics that mainly influences snowdrift – convex terrain regions show erosion while concave ones show deposition. Therefore, the discrepancies described in my AC to RC2-7 can be expected to show varying behaviour throughout the regional scale of the ice cap. Regarding the implications for model usage under future conditions this means the following: Surface curvature and exposure to synoptic winds will most probably not change much when the ice cap shows a retreat under ongoing climate warming. The dome-like shape will be remained during shrinkage of the ice body until it starts to disintegrate due to bedrock influences. Hence, the local slope angels will indeed change but the general pattern of curvature and wind exposure will not be altered significantly. Hence, there are warranted arguments that the build-up of the albedo model on the basis of present altitudinal variability of rain-snow ratio will also allow for an application under non-present conditions. However, this does not completely invalidate the objection of the reviewer. Any past or future application of the model has, of course, to be based on the assumption of stationarity of present conditions. This is a common and widely accepted drawback of all statistical models calibrated or trained

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under conditions that represent a specific instant of time. Nevertheless, I agree with the reviewer regarding the necessity of an appropriate representation of these model limitations and thus an additional paragraph will be added to the discussion section that discuses the above outlined facts in the light of the intended model application in mass-balance studies. Moreover, section 4.1 will also be extended by a related, explanatory paragraph.

RC2-9: 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.

AC: The reviewer is thanked for this valuable suggestion. Accordingly, figure 4 will be split into part A and B. Part A will contain the present content of figure 4 and the newly added part B a similar illustration showing the values of mean differences instead of RMSE. Apart from that, figure 4 will anyway experience changes in order to account for the changed cross-validation procedure (cf. AC to RC1-1 and AC to RC2-10). The major alteration will be the reduction of the illustrations to annual values. The monthly resolution will henceforth be presented in an additional table (cf. AC to RC2-11).

RC2-10: 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.

AC: The reviewer is right with his criticism of the cross-validation method applied (cf. AC to RC1-1). The evolution of albedo throughout each mass balance year shows to some extent similarities. This is, however, not because of a process-based correlation with the adjacent years. The beginning of winter snowfall at the start of each mass-balance year almost immediately returns the surface-albedo field to similar initial conditions. In the course of this mass-balance year the snow-albedo evolution is then only governed by climatic influences within this year up to the date of bare ice exposure. The assumption that the albedo evolution in one specific year is decorrelated to the evolution in adjacent years as it is addressed by the reviewer is, therefore, indeed largely valid. Hence, the suggestion of the reviewer to change the applied cross-validation method to k-folds cross validation will be adopted. This will not cause any considerable changes to the quality and reliability of the FAM as it is known from pre-work on this albedo-modelling study. Two types of cross validation have been tested in this phase: the chosen leave-one-out and the suggested k-folds cross validation. Calibration and cross validation by the k-folds method revealed an only slightly decreased accuracy of the FAM compared to the results of the application of the leave-one-out method. Hence, changing the applied cross-validation procedure according to the reviewer's demand will be a straightforward task. After implementation of the new methodology, all related text parts, figures and tables will be updated accordingly.

RC2-11: 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.

AC: The aim of the study is to present an albedo-modelling scheme. Therefore, it is

obvious that modelling quality and performance has to be shown in terms of albedo deviations. However, the reviewer is right that it might also be advantageous to show the changes in net shortwave radiation fluxes that result from the deviations between modelled and MODIS-derived "real" albedo as these represent the influencing forcing on glacier mass balance. Hence, in order to account for the reviewer's suggestion, an additional table will be introduced. It will present mean potential incoming short-wave radiation at 80°N on a monthly basis calculated from standard solar geometry. Moreover, the monthly albedo mean differences and albedo RMSE that are at present partly shown in figure 4 will also be shown in this table (cf. AC to RC2-9) as will be the resulting monthly mean differences and RMSE of net shortwave radiation.

RC2-12: 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 bud-get. 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.

AC: With this statement the reviewer is completely right. The underestimation of albedo in the uppermost parts of the ice cap is only tenable because of the minor amounts of surface melt throughout these areas and the related usage of the albedo model in the framework of mass-balance models. Regarding an application of the albedo model in simulations of future glacier evolution and mass balance this is also not a serious draw-back. As surface elevations will most probably diminish during the oncoming decades due to climate warming and resulting glacier shrinkage the underestimation of albedo in the uppermost central parts of the ice cap will likewise be reduced. To communicate this causal chain clearly to the reader a related note will be added to section 4.3.

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