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Interactive comment on "Fractional snow-covered area: Scale-independent peak of winter parameterization" *by* Nora Helbig et al.

Anonymous Referee #1

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This paper provides updated calibration and validation of a previously proposed model for fractional snow cover in mountain regions as a function of snow depth, its standard deviation, and two alpine terrain parameters (mean slope and horizontal feature correlation length). Whereas previous work calibrated the model based on snow depth and terrain information from only a couple of geographic locations, the current study pools information from a much broader range of sites using multiple observation techniques.

General Comments

My largest concern is what portion of the data set was used to calibrate the model (Section 4.3) vs evaluate the model in Section 4.4.1. The description of the methods in Section 4.3 wasn't clear (see further points made below) and seems to suggest that 80% of the data was used to calibrate the model and then the results in Table 1 and

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Figure 10 were obtained using 100% of the data (or was it the remaining 20%?). Please clarify these points. If this is indeed the case, I don't think it's appropriate to refer to the results in Section 5.4.1 as an "evaluation."

Since the data set wasn't split for calibration and validation (line 364), I think the evaluation by region is particularly important and it will provide a better sense of how differently new data is likely to perform compared to pooled performance of the calibrated model. You state this explicitly on line 389, but it could be emphasized more. It also indicates that climate models would benefit from observations across as broad a selection of alpine regions as possible. While this study uses data from across a greater number of regions than your previous work, it may still not sample enough regions to be applicable to global mountain snow. Is it possible to provide a sense to the reader (perhaps in the discussion) of how well the distributions shown in Figure 5c represent values from global snow-covered mountain ranges? What about how much variability is there in ξ across your pooled data vs globally? Would interannual variability in snow in a particular mountain region affect the values calculated for σHS? I'm guessing it wouldn't if the snow and terrain is deep but perhaps in particularly low water years it might.

While you cite Helbig et al., 2015 and Essery and Pomeroy, 2004 in the introduction there is a lot of context from those two papers which is essential to understand this current study, and I think this manuscript would benefit from including more thorough explanations and context from them. I've tried to specify several examples in the comments below which I think would help but there may be others.

Specific comments

L100-106: I think the terminology "peak of winter fSCA" parametrization causes some confusion here. I recognize that sigma_HS is being calibrated by mid-winter (March) data, but from your previous work (Helbig 2015) you are expecting that the parametrization will apply during accumulation and melt as well. Furthermore the fSCE you describe in eq 2 is calculated from Helbig et al. (2015) assuming melt events resulting in a SCD curve. This paper never mentions the accumulation season or melt until the very end at line 445. Unless you have changed your opinion on whether or not the formulation used here and in Helbig et al., 2015 can be extrapolated outside of the peak season, I think you should mention this at some point in the introduction. If you are truly concerned that it can't be extrapolated outside of the peak-snow season I think you need to justify its potential use.

A visual representation of L, ξ, dx,dy would be helpful. E.g. representational lines on Figure 3, another panel in Figure 3, or at least explicitly refer the reader to previous work (e.g. Fig 2 of Helbig et al., 2009).

Equation 1: I think it would be helpful to state that equation 1 has been shown to reasonably parametrize fSCA for both nonmoutainous and mountainous regions, while the relationship in Eq 2 is derived using only mountain data (at approx. seasonal peak, if you'd like). And/or state this distinction in the introduction (e.g. at line72, "While the standard deviation of snow depth introduced by Essery and Pomeroy did not depend on subgrid terrain characteristics, the formulation shown in Equation 2 was introduced by Helbig et al. (2015) in order to better model Equation 1 in mountainous terrain."

You refer the reader to Helbig et al 2015 at the start of Section 3, but it's not clear if this is to describe the domain sampling procedure, or even if the same method used in the 2015 paper is used in this manuscript. The 2015 reference specifies 12 domain sizes between 50 and 3000m were randomly sampled. In this manuscript there are 20 bins shown on Fig 4. Please provide additional information on how each data set/scene is decomposed into domain sizes.

L207: The symbol HS is being used to represent both the domain-average snow depth and the high-resolution observed snow depths at fine scale resolution (e.g. figures 5a, 6a, lines 73-100). I suggest you distinguish these uses.

L240: Do you sample the autocovariance in each domain 40 times? Why do you single out L=3km and then say you find inflection points for each domain size L?

L253: This is the first time you use sqS, and sigma_sqS. Again for context it would be good to mention that you are repeating previous analysis that established μ and ξ/L as the most important correlates, and you are examining these two variables to compare to results from Skaugen and Melvold, which you do in the Discussion section.

L267-280: While I understand the results shown in Figure 9, I couldn't understand your description of the methods used to produce them. I suggest removing/reordering the first 4 sentences from this paragraph. The discussion of domain size dependent fitting only confuses things when you then discuss the fit to the entire pooled data set. I suggest beginning the paragraph with "Fit parameters were first calibrated for the entire data pool yielding c = 0.6589 ($\pm$0.0037) and d = 0.5638 ($\pm$0.0043) with the 90 % confidence interval. . . . . larger than the previously derived constants a, b in Eq. (2) (cf. Figure 9). For each step-wise domain size between 200 m to 5 km scale-dependent parameter values are also fit from the data (cf. individual colored lines in Figure 9)." At this point please provide a more complete description of the sub-sampling used to derive c, d for each step-wise domain size. What does 80% mean? Are the parameter values fitted from all the data within a randomly chosen domain of the appropriate size and this process is repeated 500 times? For domain sizes above 1km there are <500 domains total so are the same values just replotted? After this description, you can continue on with the discussion of how the parameters increase with L and the subsequent fitting of c(L) and d(L).

Fig 9: Please use a different description on the legend in place of 'f(L) – Eq. (3)' which can read as 'f(L) minus Equation (3).'

Section 4.4.4: Does the different choice of domain aspect ratio (square vs rectangular) affect the differences described in this section?

L335-337: Please rephrase for clarify: "at these scale lengths." I think you are saying something like "Above scale-lengths of ~200m all three effects (precip/wind/radiationinteractions) come into play, while we think there are different physical effects which establish the breaks at 20 and 60m," but please confirm. Also consider rephrasing "scale-independent parameterization", since the parametrization incorporates scale information from the sub-domain terrain parameters as well as in the constants (c(L), d(L)). Perhaps something like "The results presented here indicate that the model described by (eqs. 1 and 2) is a reasonable fSCA parametrization in mountainous terrain for spatial scales between 200m to 5km." Given that you are aiming to have this used as a fSCA parametrization in climate models which can still use grid scales as high as ~50-100km please comment on the extrapolation of your results substantially beyond 5km.

L343: Do you mean "for spatial scales between 0.5km and 1km"?

L357: "Furthermore, larger (about 17% and 45% , respectively) but overall consistent constant fit parameters were obtained compared to those from Helbig et al. (2015) based on a more limited number of data sets and just two geographic regions (cf. a, b. . ."

L411-413: I'd suggest that the appropriate standard for how different parametrizations perform is the range of MPE seen among different regions, not the difference between your previous calibration and the current one.

Technical Corrections

L185: 3m to 5km

Discussion, several places: "origin" as a verb -> "originate"

L379: I'd suggest splitting this sentence in two.

L400: rephrase

L395: "decrease from 80cm. . ."

L409: "sensitivity"?

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