Comments on “A distributed snow redistribution model“ by S. Frey and H. Holzmann

We thank the referees for their comments on the paper. We thank both referees for the language corrections and agree with them for a need to rephrase. We would like to respond on the comments. For clarification remarks of referee #1 are formatted in *blue italic*, referee’s #2 comments are formatted in *orange italic*, while our comments are formatted in black. Changes on the manuscript are formatted in *black italic*.

We edited our manuscript and believe we were capable of significantly raising its scientific quality.

Since comment (i) by referee #1 and some comments by referee #2 mention similar topics, we reply on both comments at the same time.

(i) The manuscript could be improved by clearly stating that a) lateral snow redistribution processes are either gravitationally or wind induced, b) these processes can either be modelled process-oriented or empirically, and c) You concentrate on wind-induced snow redistribution by means of an empirical approach. You should then extend Your literature and state-of-the-art review with relevant papers on exactly this (e.g., Helfricht et al. 2012, Dadic et al. 2010 etc. Base of all is Winstral and Marks (2002) and Winstral et al. (2002)).

The “snow towers “ in RR models is a very common problem, and, at the catchment scale, the usual way to address this has been to look at the input: too heavy precipitation gradient with altitude and/or too negative temperature gradient. In addition, the spatial frequency distribution of snow may be very influential for the dynamics of the snow reservoir.

I think the authors need to review the problem (of snow towers/ accumulation of snow over several seasons) properly. This includes reviewing the different reasons for the problem and show how other authors have solved the problem. The solutions proposed in this paper should logically emerge as a potentially better choice than the reviewed approaches. Include this in the introduction

We agree with this comment. However, the model does not only consider wind but is rather a conceptual description of all processes regarding snow transport on scales of 100s to 1000s of metres except of wet snow avalanches.

As referee #2 stated, the common way to avoid the existence of “snow towers” is to edit the meteorological input, mainly precipitation. Justification for doing so is (i) the underrepresentation of precipitation gauges in high alpine terrain and (ii) the high degree of errors in measuring solid precipitation. Adjusting meteorological input however needs at least one (time dependent) parameter and raises the question if we can trust these input data in general. Does the input in summer (e.g. rain) need to be changed to? We use meteorological parameters from the INCA dataset which already takes gradients regarding precipitation and temperature into account. One has to be aware of uncertainties in the INCA data set as well. However, the approach presented in our paper does not need any correction of meteorological input for snow accumulation issues. We agree to include a review in the introduction.

We did this by editing following paragraphs in section 1.2:

"A common approach avoiding intensive accumulation of snow is editing the meteorological input (Dettinger et al., 2004). For instance, many models use a constant yet adjustable lapse rate for interpolating temperature with elevation (Holzmann et al., 2010; Koboltschnig et al., 2008). Besides temperature, precipitation gradients are often adjusted to fit observed and modelled target variables (e.g. snow patterns or runoff) (Huss et al., 2009b; Schöber et al., 2014). Justification for doing so is the general lack of gauging stations in the summit regions (Daly et al., 2008, 1994) *along with the high error of precipitation gauges (Rasmussen et al., 2011; Williams et al., 1998). An approach presented by Jackson (1994) defining a precipitation correction matrix was successfully applied in several studies (Farinotti et al., 2010; Huss et al., 2009a). Scipión et al. (2013) however identified significant discrepancies between precipitation patterns obtained by a Doppler X-band radar and the final seasonal snow accumulation which may serve as a proxy for seasonally accumulated precipitation on the ground.*

Models trying to deal with accumulations apart of input corrections may be classified into two major approaches. One is to model snow distribution patterns process-oriented the other approach is empirical. Examples for process oriented model are SNOWPACK (Bartelt and Lehning, 2002) used in avalanche research or SnowTran3D (Liston and Sturm, 1998; Liston et al., 2007)*. Empirical models use the fact, that snow patterns resemble each other every year* (Helfricht et al., 2014, 2012)*. The presented paper concentrates on the empirical approach.*

"Helfricht et al. (2012) used airborne LiDAR measurements to determine snow accumulation gradients for elevation bands in the Ötztaler Alps. These could be used to improve hydrological models regarding snow cover distributions and subsequently to achieve better runoff predictions. LiDAR data, however, are relatively expensive. Often wind speed and -direction are used to model snow drift (e.g. Bernhardt et al., 2009; 2010; Shulski and Seeley, 2004; Winstral et al., 2002; Liston and Sturm, 1998). Also the physical based SNOWPACK model (Bartelt and Lehning, 2002) used in avalanche research uses wind to determine redistribution of snow. Kirchner et al. (2014) concluded from LiDAR measurements in combination with meteorological stations in a catchment in California, USA, that wind measurements from only one meteorological station are of too poor quality for a useful description of wind fields for snow transport. Unfortunately, wind fields generated by regional circulation models (RCM) for climate change scenario studies are prone to errors, too (Nikulin et al., 2011). In addition models using wind have in common that they are

computationally intensive as they require data in high spatial resolution 100 to 1000s of square metres. Schöber et al. (2014) combined gravitational and wind induced snow transport using a distributed energy balance model with a resolution of 50 m x 50 m."

(ii) Most common approaches to empirically parameterize wind-induced snow redistribution depending on topographical features use curvature, sky view factors, aspect, shelteredness/exposedness etc. Slope is a good indicator for the original transport route, but neither for the erosion nor the deposition areas. A detailed argumentation why You use slope, and why You use it in the way You do ("The model redistributes snow only to grid cells providing the steepest slope (acceptor cell) in the direct neighbourhood of the raster cell it searches from (donor cell)."), is missing in Your manuscript. If I understand correctly, then steep slopes are deposition areas in Your model. Observation suggests, however, that wind-blown snow is deposited where the wind speed drops, i.e. behind obstacles, and most snow is accumulated onto flat areas (best example: glacier accumulation areas, which are mostly flat! The glaciers in the Ötztal Alps are a very good example). Maybe You best begin with a visualization of the slope distribution with elevation for the basin.

We agree on a detailed argumentation why we use slope as an indicator for the transport route and for mobilization capacity. However, steep slopes are no deposition areas in the model. It uses slopes to determine in which of the eight possible directions snow is being redistributed. Since the amount of snow being redistributed is a function of slope and density of snow, more snow will be distributed via steep slopes than on rather flat terrain. The redistribution routine is organized in form of a loop starting at the highest point in the catchment and ending at the lowest cell. That assures that no snow can be transported into an already processed adjacent cell. Snow will be transported downhill as long as the slope (and density) is great enough to allow transportation. Therefore snow accumulates rather in flat regions in the catchment.

We agree however that the model should be described in higher detail.

We added and edited text at several parts of section 2.2 in the paper:

"Several authors reported the slope angle having an important influence on snow depths (Bernhardt and Schulz, 2010; Kirchner et al., 2014; Schöber et al., 2014)."

[…]

"Since other geomorphological properties than slope angle influencing snow patterns are most important on scales smaller than the grid size of COSERO (see section 1.1), slope was selected as driving force for the model. One has to be aware that this is a simplification and under realistic conditions snow might not necessarily be transported only on the steepest route (Bernhardt and Schulz, 2010; Winstral et al., 2002)."

[…]

"The model is organized in form of a loop starting at the highest grid cell (summit region) and ending at the lowest cell (outlet of the catchment). That assures that snow cannot be redistributed into already processed grid cells. Snow will be transported downslope as long as the slope is great enough to allow for transportation given that the density of snow is low enough. Therefore snow accumulates rather in flat regions of the catchment. A similar approach was used in the SnowSlide model (Bernhardt and Schulz, 2010)."

(iii) Any topography-related parameterization is very much depending on the scale (i.e., size of the grid cells in a raster-based model). Since a 1 km resolution is very coarse for the high Alpine topography of the Ötztal Alps, You have to include a comprehensive discussion of the scale effect, including sensitivity analysis of Your approach to the resolution of the DEM used. Actually, You should prove that the parameterization You develop produces valid results for right reason. Can Your model transport snow uphill (wind-induced uplift is a common redistriubutin phenomenon)? If not: why, and how do You avoid this? If yes: following You model, snow can be eroded from the flat glacier accumulation areas and deposited on the steep mountain summit slopes around … ?

It is true that topography-related parametrization depends on the scale. It is also true that 1x1 km is very coarse for alpine regions. However, since many hydrological models operate on that scale of raster cells (Bookhagen and Burbank, 2010; Cornelissen et al., 2013; Marke et al., 2011; Mauser and Bach, 2009) it is important to account for the problem of "snow towers" existing on that spatial resolution, too. In fact, even at lower spatial resolutions, e.g. when applying semi-distributed RR models like PREVAH to alpine terrain, this problem occurs as was shown by Koboltschnig et al. (2008), for instance. Scaling issues determine the degree of complexity of a model. On the size of 1x1 km grid cells, small scale ridges cannot be pictured and therefore a physical consideration of dropping wind speeds at their lee sides is not only not necessary but not possible. See also comment on paragraph (ii).

Added to section 2.1:

"[…] COSERO uses five snow classes per cell to approximate this sub-grid log-normal distribution under accumulation conditions (see Fig. 2 b)), i.e. snowfall is distributed log-normally into snow classes. This distribution can be interpreted as a statistical description of snow distribution processes taking place at smaller scales than the 1x1 km grid (Pomeroy et al., 1998), i.e. influence of curvature, shelter or vegetation (Hiemstra et al., 2006)."

The paper addresses an important problem, but I am not (yet) convinced that it presents the best solution to the problem. It is fairly obvious that transporting snow from elevations that *have little melt to elevations with substantial melt will work, but addressing the problem of too much snow by just moving it appears as too simplistic.*

The issue of spatial scale is very important in a paper like this. The authors state that "no model for redistributing snow on a 1X1 km grid sixe exists" (p.611. l.16). There may be very good reasons for why it is so. Redistribution by wind is considered an important process for the spatial distribution of snow on rather modest spatial scales (up to some 100 meters ?). My feeling is that this is not yet a closed issue, but the authors need to discuss scales (quantitatively, not "small" and "large") and present a review on what is considered the important processes for the spatial distribution of snow at what scales. The Liston (2004) paper may serve as a starting point. After such a review I am not at all certain that the proposed method is a natural choice on a 1X1 km grid. "Scale" is often mentioned in the paper, but seldom quantified. Include this in the introduction.

The model does not intend to give a fully detailed description of physical based processes leading to (re)distribution of snow. This is hardly possible using a spatial resolution of 1x1 km. However the problem of heavy snow accumulation exists on the 1km² scale, too. We agree that wind influences snow patterns on rather smaller scales of several 100s of meters. The snow distribution on scales smaller than 1x1 km, i.e. sub-grid scale, is included in the model by a log-normal distribution of snow depths. The model should be considered as conceptual approach (which fits into the hydrological model being conceptual in its kind) to deal with snow accumulations. Since the only way to enable snowmelt using a temperature-index approach is raising values of air temperature, snow needs to be transported into warmer regions (or editing meteorological input, what we don't want to do).

Edited and added text at several positions in the paper (see also comments on specific remarks):

Section 1.1:

"During the accumulation period, according to Liston (2004), primarily three mechanisms are responsible for these variations: (i) snow-canopy interactions in forest covered regions, (ii) wind induced snow redistribution and (iii) orographic influences on snow fall. These mechanisms influence snow patterns on scales ranging from the plot scale (i. e. several square metres) to the catchment scale (i. e. one to several square kilometres).

Spatial snow cover variability beneath canopies is mainly affected by different tree species (deciduous vs coniferous trees) influencing LAI, height and density of the canopy and gap sizes (Garvelmann et al., 2013; Liston, 2004; Pomeroy et al., 2002).

Besides the impact of vegetation, wind is the most dominant factor influencing snow patterns in alpine terrain. Snow is transported from exposed ridges to the lee side of these *ridges, valleys and vegetation covered areas (Essery et al., 1999; Liston and Sturm, 1998; Rutter et al., 2009; Winstral et al., 2002). One has to be aware that besides of the physical transport of solid snow wind also stimulates sublimation processes (Liston and Sturm, 1998; Strasser et al., 2008). Wind influences snow depth distributions on scales of some 100s to 1000 square metres (Dadic et al., 2010a).*

The third mechanism influences snow patterns on a larger scale of one to several kilometres (e. g. Barros and Lettenmaier, 1994). Non-uniform snow distributions are caused by interactions of the atmosphere (air pressure, humidity, atmospheric stability) with topography (Liston, 2004).

In addition to these processes, avalanches play a role in snow redistribution (Lehning and Fierz, 2008; Lehning et al., 2002; Sovilla et al., 2010). In steep terrain, avalanches depend mainly on the slope angle are capable of transporting large snow masses over distances of tens to hundreds of metres (Dadic et al., 2010b; Sovilla et al., 2010)."

Section 2.1:

"COSERO uses five snow classes per cell to approximate this log-normal distribution under accumulation conditions (see Fig. 2 b)), i.e. snowfall is distributed log-normally into snow classes. This distribution can be interpreted as a statistical description of snow distribution processes taking place at smaller scales than the 1x1 km grid (Pomeroy et al., 1998), i.e. influence of curvature, shelter or vegetation (Hiemstra et al., 2006)."

The presented model is very parameter-rich. I believe I counted some 10 calibration parameters just in the snow module. With such possibilities for equifinality problems and compensating parameters, how do the parameter uncertainty influence the validation of the method? Discuss this.

The model indeed is rich of parameters. However, many of them are estimated a priori. In fact 11 out of a total of 15 parameters in the snow module including the snow redistribution routine are estimated a priori and therefore are not included in the optimization procedure. Those parameters are either adjusted according to literature or previous work on the model (Fuchs, 2005; Kling, 2006; Nachtnebel et al., 2009). Consequently only four parameters (three without redistributing snow) do potentially lead to equifinality issues. In combination with parameters from other model routines, equifinality is an issue as it is basically in every conceptual RR model. Any additional parameter amplifies the potential of equifinality, of course. Implementation of additional (hydrological) processes in a model in most cases needs additional parameter(s). However, in the presented model consideration of snow redistribution allows for a more realistic estimation of snowmelt parameters in high mountain ranges. It also allows for the use of meteorological input data without applying correction coefficients. We think that the topic, how this exactly influences equifinality including uncertainty analysis and parameter interaction should not be part of the paper since this is a very broad topic. We agree the problem of equifinality has to be described in the paper, though.

We added following text to the calibration paragraph (3.3):

"Although the model is rich of parameters, the vast majority of them have been estimated a priori according to literature (Liston and Sturm, 1998; Prasad et al., 2001) and previous work *on the model (Fuchs, 2005; Kling, 2006; Nachtnebel et al., 2009). In the snow model including snow redistribution only three parameters have been calibrated: upper and lower boundaries of snow melt factors D_U and D_L, respectively and the calibration parameter for snow redistribution C. This limits problems due to equifinality issues."*

And to the conclusions:

"Even though the vast majority of parameters were estimated a priori in this work, equifinality remains an issue. However it should be also stated that redistribution of snow requires only two additional parameters but allows for narrower boundaries of the snow melt factors. More work needs to be carried out to account for that issue."

I think it is strange that there were no differences in simulated snow covered area (SCA) by the two models, and that model A did not compare better with the MODIS scenes. If you remove a lot of snow then you would expect areas to become snowfree earlier (even though some snow is, initially, retained by vegetation).

We have been curious about that behaviour, too. Snow holding capacity by vegetation or surface roughness (H_V) retains snow not only initially but generally. If the snow depth of a snow class (of which five exist per grid cell) is lower than H_V no snow can be transported to any other grid cell. Anyway, we had expected to see snow free cells in the realization which takes snow transport into account, too. A possible explanation is: A grid cell acts as donor if the snow depth of at least one of its snow classes exceeds H_V. A grid cell acts as acceptor, if it is the lowest neighbour of at least one other grid cell. It may act as donor and acceptor at the same time. As a consequence, grid cells with no uphill neighbour (peak regions) never receive snow. Grid cells in the intermediate part of the mountains accept and donate snow at the same time, given they are the lowest neighbour of at least one other cell. Since redistributed snow is considered in the same way as precipitation, it gets distributed according to the log-normal distribution of its acceptor cell. It is hard therefore for grid cells donating and receiving snow to become (partly) snow free. The cells in the peak regions only donating snow however they are mainly located at elevations where temperature values seldom rise above 0°C. It is hard for them to get snow free anyway. Snow mobilization therefore leads to a decrease in SWE in the upper regions of the basin but snow still remains in the grid cells. Snow cover patterns only differ slightly in the phase of depletion.

We agree this has to be discussed in the paper in more detail. We added to section 5.2:

"In Fig. 8 only little differences between model A and B can be distinguished. Grid cells covering the summits only donate snow to their respective acceptor cells. However, a certain amount of snow is held back according to threshold due to vegetation and roughness of the surface. Grid cells nested in the intermediate slope regions receive and donate snow at the same time. Thus their snow depth changes little if comparing model A and model B. In flat valley regions, grid cells only receive snow but are unable to donate it further downward. Here, temperature values often allow for melting."

Specific remarks

- P610 L11-12: ". . . the standard model without using snow tansport" should better be "the standard model without the parameterization for lateral snow redistribution"

Done. Replaced by: *"… the standard model without parametrization for lateral snow redistribution"*

- P611 L7: indicate here studies using conceptual approaches (e.g. degree-day) for snowmelt in which an attempt is made "to solve this problem"

Exact sentence has been removed. Instead the literature review has been extended. (See chapter 1.2)

- P611 L8: explain in this paragraph which approaches are conceptual (topography-dependent), and which are physically based (process representations); see general remark (i) above

Done. Examples given mainly in 1.2:

"Models trying to deal with accumulations apart of input corrections may be classified into two major approaches. One is to model snow distribution patterns process-oriented the other approach is empirical. Examples for process oriented model are SNOWPACK (Bartelt and Lehning, 2002) used in avalanche research or SnowTran3D (Liston and Sturm, 1998; Liston et al., 2007)*. Empirical models use the fact, that snow patterns resemble each other every year* (Helfricht et al., 2014, 2012)*. The presented paper concentrates on the empirical approach."*

- P611 L12: replace "afflicted" with "prone to"

Done: *"Unfortunately, wind fields generated by regional circulation models (RCM) for climate change scenario studies are prone to errors, too (Nikulin et al., 2011)."*

- P611 L21-25: what about avalanches? In steep terrain their effect with respect to redistribution (and, e.g., glacier mass balance) is significant.

We do not model avalanches explicitly for scaling issues (see section 1.1). However, as the model is a conceptual description of all processes regarding snow transport on scales of 100s to 1000s, we implicitly account for (dry-snow) avalanches as well.

Added: *"In addition to these processes, avalanches play a role in snow redistribution (Lehning and Fierz, 2008; Lehning et al., 2002; Sovilla et al., 2010)."*

- P612 L1-3: Avoid the term "gymnosperms": Spatial snow cover variability beneath canopies is mainly affected by different tree species (coniferous vs deciduous trees), LAI, canopy height and density, and gap sizes, all of them interfering with topographical features

Replaced by: *"Spatial snow cover variability beneath canopies is mainly affected by different tree species (deciduous vs coniferous trees), LAI, height and density of the canopy and gap sizes (Garvelmann et al., 2013; Liston, 2004; Pomeroy et al., 2002)."*

- P612 L4-12: newer literature is available (e.g., Strasser et al. 2008, Rutter et al. 2009, Warscher et al. 2013). It would be benefitial to distinguish between the wind-induced processes (i) preferential deposition of precipitation, (ii) redistribution by means of erosion/deposition, and (iii) sublimation from turbulent suspension

Added literature and rephrased in section 1.1:

"Besides the impact of vegetation, wind is the most dominant factor influencing snow patterns in alpine terrain. Snow is transported from exposed ridges to the lee side of these ridges, valleys and vegetation covered areas (Essery et al., 1999; Liston and Sturm, 1998; Rutter et al., 2009). One has to be aware, that besides of the physical transport of solid snow wind also stimulates sublimation processes (Liston and Sturm, 1998; Strasser et al., 2008).

The third mechanism influences snow patterns on a larger scale of one to several kilometres (e. g. Barros and Lettenmaier, 1994). Non-uniform snow distributions are caused by interactions of the atmosphere (air pressure, humidity, atmospheric stability) with topography (Liston, 2004)."

- P612 L13-17: incorrect English, this paragraph must be improved. Also better write " . . . snomelt rates from south-facing slopes . . . "

Rephrased:

"During the ablation period, spatial snow distributions are mainly influenced by differences in snow melt behaviour. On the northern hemisphere on south-facing slopes rates of snow melt are generally enhanced compared to north-facing slopes due to the inclination of radiation. Also vegetation influences melting behaviour. Shading reduces snowmelt compared to direct sunlight. Enhanced emitted long wave radiation due to warm bare rocks or trees increases it (Garvelmann et al., 2013; Pohl et al., 2014)."

- P613 L9-10: Better "In the latter study, . . ."

Replaced by: "However, in Kling et al. (2014a) …"

- P613 L19: Fig. 2 does only show one snow class?! In which properties do the five classes differ, in swe? What do they have in common, albedo? How are they initialized? Can a cell partly melt out? What about snow transport between the classes? How is snow distributed amongst them in the case of (i) precipitation, (ii) erosion and (iii) deposition? Please explain in more detail. . .

Updated Fig. 2 (see section of figures below). Also added paragraph in main text in section 2.1:

"Numerous studies have shown that sub-grid variability of snow depths can be described by a two parameter log-normal distribution (e. g. Donald et al., 1995; Pomeroy et al., 1998). This distribution can be interpreted as a description of small scale snow distribution processes. COSERO uses five snow classes per cell to approximate this sub-grid log-normal distribution under accumulation conditions (see Fig. 2 b)), i.e. snowfall is distributed log-normally into snow classes. The properties of each class may be unique as Eqs. (1 to 12) apply to every snow class separately. Consequently the log-normal distribution within a grid cell may be disturbed by the processes of melting, sublimation, refreezing and redistribution to other grid cells. Redistribution between the snow classes within a single grid cell is not considered. A scheme of the composition of a snow class is illustrated in Fig. 2 a). …"

And

"Sublimation is considered only for snow classes actually covered by snow. Hence, if a grid cell is partly snow free (due to melting) sublimation is estimated for the snow covered part only. For the uncovered classes evapotranspiration according to the Thornthwaite method is applied."

Deposition already is explained in the manuscript: P617 L8-9: *"On acceptor cells redistributed snow is treated as fresh snow in the sense, that it is distributed to the snow classes according to the log-normal distribution."*

- P613 L20: "fluid" should better be "liquid"

Done. *"… where P_{Rt} and P_{St} are liquid and solid precipitation in mm"*

- P613 Eq. (2): indicate the time step of the model. is Tair a mean daily temperature?

Yes, T_{AIR} is mean daily temperature. Added to text: *"T_{AIRt} is the (mean) daily air temperature in °C"*

Corrected in all equations T_{AIR} is part of. Changed T_{AIR} to $T_{AIRt.}$

- P614 L5ff: give units, and indicate if values are averages or instantaneous?

Edited paragraph in section 2.1:

"… where J is the Julian day of the year [-], D_U and D_L are the upper and lower boundaries of D_f in mm °C^{-1}, respectively, and M_{RED} [-] is a reduction factor to account for the higher albedo caused by freshly fallen snow calculated by Eq. (4). S_{CRIT} is the critical snow depth of fresh snow in mm necessary to increase the albedo, whereas S_{fresh} is the actual depth of fresh snow in mm fallen within one time step. For fresh snow depth larger than S_{CRIT}, D_f is lowered to a reduced melting factor D_{RED} [-]."

The fragment *"within one time step"* indicates that values are instantaneous.

L16: replace "then" with "than"

Done.

- P616 L6-7: does that mean that snow is eroded from flat terrain and deposited in adjacent steep slopes? Observation suggests that snow is eroded from convex to concave terrain features?! Can it be that the reason to use this is an effect of Your resolution, i.e. Your highest pixels are flat, and such snow is removed downvalley? See general remark (iii)

See comment on general remark (iii).

- P616 L12: "snow depth on the cell"is no good English, better "in" or "of" the cell

Changed to "of".

- P616 L12: "lighter": actually, no snow gets "lighter"; its a change in density only

Deleted "lighter": *"The drier (less dense) the snow pack …"*

- P616 L15: use SI units (here kg m-3). "Acts" is not an appropriate word: density doesn't act. Maybe " . . .the value of 450 kg m-3 is used as threshold . . . "

Changed units, also in figures.

Rephrased sentence in section 2.2: *"Thus the maximum density of snow determines the threshold for snow redistribution."*

- P616 L18: delete comma bevore "where"

Done.

- P617 L2: "snow depth on the cell“ is no good English, better "in“ or "of“ the cell

Changed to “in”

- P617 L8: delete comma

Done.

P618 L4-5: "wind directory data“ should better be "wind direction“

Yes, was a typo. Done.

- P618 L8, L10: "Target of“, "Validation period“: sentences should not begin with subjects without article, see also the caption of Fig. 8 ("Reason of“): better re-arrange or add article

Rephrased and edited citation that was wrong.

“The target of the calibration was a good fit of runoff using the Kling-Gupta-Model-Efficiency (Gupta et al., 2009; Kling et al., 2012) as objective function.”

[…]

“The model was validated for the years 2009 and 2010.”

- P618 L15: "by Table 1“ should be "in Table 1“

Changed to “in”.

- P619 L17-19: The sentence "Note that in Fig. 9 only model results from 2005 to 2010 are shown while the warm-up period is missing due to a better perceptibility. Therefore snow depth does not start at zero in the figure while it does at the beginning of the modelling“ should be moved into the figure caption.

Done and rephrased: *“Note that model results are shown from 2005 to 2010 without the warm-up period for clarity reasons. Therefore snow depth does not start at zero in the figure while it does at the beginning of the modelling.”*

- P620 L13: better "in " the cell than "on“ the cell (same also in the caption of Fig. 8)

Done. Also edited error bars in Fig. 8. Due to following reason: MODIS detects snow and clouds. No information about snow can be derived from cloud covered areas which is why there may be an error. Snow covered areas however cannot be smaller than the area that has already been identified as covered by snow.

- P620 L17: "pronounces“ should be "pronounced“ - P620 L19:

Typo. Done.

- P620 L19: "". . . that transports more snow on greater slopes . . . ": unclear. Do You mean: that leads to deposition of more snow on steeper slopes"?

- P620 L22-24: This sentence is no correct English

- P621 L4: "on low elevations" should be "in low elevations"

Paragraph concerning these three comments rephrased to:

"While using model B, the higher the elevation the more snow is situated on. However, model A shows less pronounced and in some time periods even contrary behaviour in the upper altitudes (see Fig. 9). This is a result of the slope dependency of the distribution model that the amount of snow distributed to other grid cells is greater with increasing vertical distance to the downward grid cell. In general and in the Ötztal as well mountains are steeper in the summit regions than at the bottom (see Fig. 5). Consequently in the summit regions snow will be preferably eroded while it accumulates at the rather flat valleys where the vertical distances between the grid cells are less than at the peaks. This does reflect snow accumulations that can be observed in nature where summits might be nearly snow free in spring while shallower parts are still covered with snow. While the raster cells covering peak regions act as donators only those cells located on slopes may receive and distribute snow at the same time (Fig. 10). Valley regions only receive snow. However, due to the binary nature of MODIS data, the spatial snow depth distribution cannot be validated with observed satellite based data."

- P621 L3-8: entire paragraph is unclear and no correct English. Clarify whether processes in nature or their modelling are discussed, and which model is used, if the latter. The amount of snow remaining in the catchment is no good argument; and what is "This information"?

Paragraph rephrased to:

"The smaller the portion of high altitude areas in a catchment compared to the total catchment area the less important is snow redistribution for modelling runoff. This ratio of summit regions to total catchment size is normally smaller for bigger catchments. The catchment of river Inn, for instance, covers an area of about 10000 km² yet only 733 km² are located at elevations where intensive snow accumulations and mobilizations occur (above 2800 m a.s.l.). In the Ötztal basin 204 out of 511 km² are located higher than 2800 m a.s.l. If model A is applied to the catchment of river Inn in five years of modelling about 15 mm SWE (with respect to the entire river basin) remain in the catchment due to snow accumulation processes instead of 300 mm in the Ötztal. This may be the main reason why snow redistribution is often not considered in hydrological models at larger scales."

Fig. 2: "binded" should be "bound"

Done.

Fig. 4: "an" should be "a"

Done.

Fig. 8: please reconsider if this figure is meaningful. You indicate the reason why results are so similar . . .

Although the results are quite similar, we think this figure still is meaningful. It demonstrates the general efficiency of both models (which is good) and gives the reader an idea of how the differences are.

Fig. 9: "on elevation" should be "in elevation"; I do see a clear positive trend also for Model A in the highes elevation zone. What about it?

"On" has been changed to "in".

Added paragraph to conclusions:

"Although snow accumulation behaviour of model A is more realistic than model B snow accumulation can still be observed in the highest elevations zone (see Fig. 9). This problem might be solved using higher correction coefficients for grid cells in this elevation level or by accounting for snow metamorphosis. The influence of the highest elevation class (> 3400 m a.s.l.) on both the hydrograph and snow covered area however is very small, since this elevation level is represented by only four grid cells. Consequently the objective function during calibration using an automated optimization routine like Rosenbook's routine does not differ much when underestimating the correction coefficient in these grid cells."

Fig. 10: "For visualisation the free available oe3d DEM (Rechenraum, 2014) was used". This is not of interest here. The duration for which net deposition is accumulated is missing . . .

Edited figure annotation:

"Figure 10. Net snow deposition in the catchment during the time period of one year. Negative values refer to a net loss, positive to a net gain of snow. Raster cells in the peak regions act as donor cells and do not receive any snow whereas lower cells may act as donor and acceptor in the same time. Note that, since only the net deposition of snow is shown, values cannot be linked to snow depths at the end of the time period."

p.611, l 5. It says humidity, you mean turbulent fluxes?

Yes, corrected: *"… like radiation or turbulent fluxes of latent energy."*

p.611, l 22. High variability especially on high-resolution scales, less variability on small scales (see Melvold and Skaugen, 2013, Annals of Glaciology)

Edited paragraph in section 1: *"Reasons for that are either wind or gravitationally induced lateral snow distribution processes (Elder et al., 1991; Winstral et al., 2002). Resulting snow depths are not uniformly distributed in space but vary greatly (Helfricht et al., 2014). When changing the focus from micro (e.g. several square meters) to macro scales (e.g. one to several square kilometres), variations become less (Melvold and Skaugen, 2013)."*

p.611, l 24. Quantify the scales in section 2

Edited paragraph in section 1.1: *"During the accumulation period, according to Liston (2004), primarily three mechanisms are responsible for these variations: (i) snow-canopy interactions in forest covered regions, (ii) wind induced snow redistribution and (iii) orographic influences on snow fall. These mechanisms influence snow patterns on scales ranging from the plot scale (i. e. several square metres) to the catchment scale (i. e. one to several square kilometres)."*

p.612, l 16. Shading and long wave radiation are not opposite entities? You have long wave radiation as long as you have a temperature above zero (Kelvin)

True. Rephrased to: *"On the northern hemisphere on south-facing slopes rates of snow melt are generally enhanced compared to north-facing slopes due to the inclination of radiation. Also vegetation influences melting behaviour. Shading reduces snowmelt compared to direct sunlight. Enhanced emitted long wave radiation due to warm bare rocks or trees increases it (Garvelmann et al., 2013; Pohl et al., 2014)."*

p.613, l 9...this study. . ., yours or That of Kling et al.

Edited: *"However, in Kling et al. (2014a) snow parameters were not calibrated and therefore the snow module is not fully explained in detail."*

p.613, l 13. Sub-grid, what scale (quantified) is that?

p.613, l 16. The five classes are not clear, neither from the text nor from the figure. In addition what is the size of the cell?

Rephrased paragraph in section 2.1:

"…The model uses 1x1 km grid cells."

[…]

"Numerous studies have shown that sub-grid variability of snow depths can be described by a two parameter log-normal distribution (e. g. Donald et al., 1995; Pomeroy et al., 1998). COSERO uses five snow classes per cell to approximate this sub-grid log-normal distribution

under accumulation conditions (see Fig. 2 b)), i.e. snowfall is distributed log-normally into snow classes. This distribution can be interpreted as a statistical description of snow distribution processes taking place at smaller scales than the 1x1 km grid. The properties of each class may be unique as Eqs. (1 to 12) apply to every snow class separately. Consequently the log-normal distribution within a grid cell may be disturbed by the processes of melting, sublimation, refreezing and redistribution to other grid cells. Once fallen, snow redistribution between the snow classes within a single grid cell is not considered. A scheme of the composition of a snow class is illustrated in Fig. 2 a). …"

p.615, l 16. Where is the "settling constant" defined?

True, was not shown. Added equation and edited text: *"Its density is calculated using a time settling constant (ρ_{set}, derived from Riley et al., 1973) until the maximum density is reached (Eq. 10)."*

$$\rho_{MAX} = \frac{\rho_{SET} \cdot \left(\frac{S_{SWE_t}}{\rho_{MAX}} - \frac{S_t}{2} + S_t\right)}{1 + \frac{\rho_{SET}}{2}} \qquad (10)$$

Snow pack instead of snow cover. The less dense snow pack, the higher the portion available for redistribution.. . .. . .

Changed snow cover to snow pack.

p.615, l 15. Is not 0.45 extremely dense snow?. Perhaps 0.3 or so is better? What does literature say?

True, 450 kg/m³ is dense snow. However Schöber et al. (2014) reported snow densities in the Ötztal up to that value. In the Swiss Alps, Jonas et al. (2009) reported snow densities up to 600 kg/m³ when compressed by avalanches.

Added "Maximum snow density was assumed 450 kg m^{-3} which matches long term snow measurements (Jonas et al., 2009; Schöber et al., 2014)."

p.617, l 1. S_SWE_A

Done.

p.618, l 4. ..wind speed or -direction

Done.

p.618, l 23. The figure has mm, not m3/s

Added information in mm but kept m³/s in addition:

"…between the two models reach up to 2 mm per day (which equals to 12.1 m³ s^{-1}) leading…"

p.619, l 18. Better perceptibility?, rephrase

Rephrased and moved to figure caption: *"Note that model results are shown from 2005 to 2010 without the warm-up period for clarity reasons. Therefore snow depth does not start at zero in the figure while it does at the beginning of the modelling."*

p.6120, l 3-5. rephrase

Paragraph rephrased:

"The smaller the portion of high altitude areas in a catchment compared to the total catchment area the less important is snow redistribution for modelling runoff. This ratio of summit regions to total catchment size is normally smaller for bigger catchments. The catchment of river Inn, for instance, covers an area of about 10000 km² yet only 733 km² are located at elevations where intensive snow accumulations and mobilizations occur (above 2800 m a.s.l.). In the Ötztal basin 204 out of 511 km² are located higher than 2800 m a.s.l. If model A is applied to the catchment of river Inn in five years of modelling about 15 mm SWE (with respect to the entire river basin) remain in the catchment due to snow accumulation processes instead of 300 mm in the Ötztal. This may be the main reason why snow redistribution is often not considered in hydrological models at larger scales."

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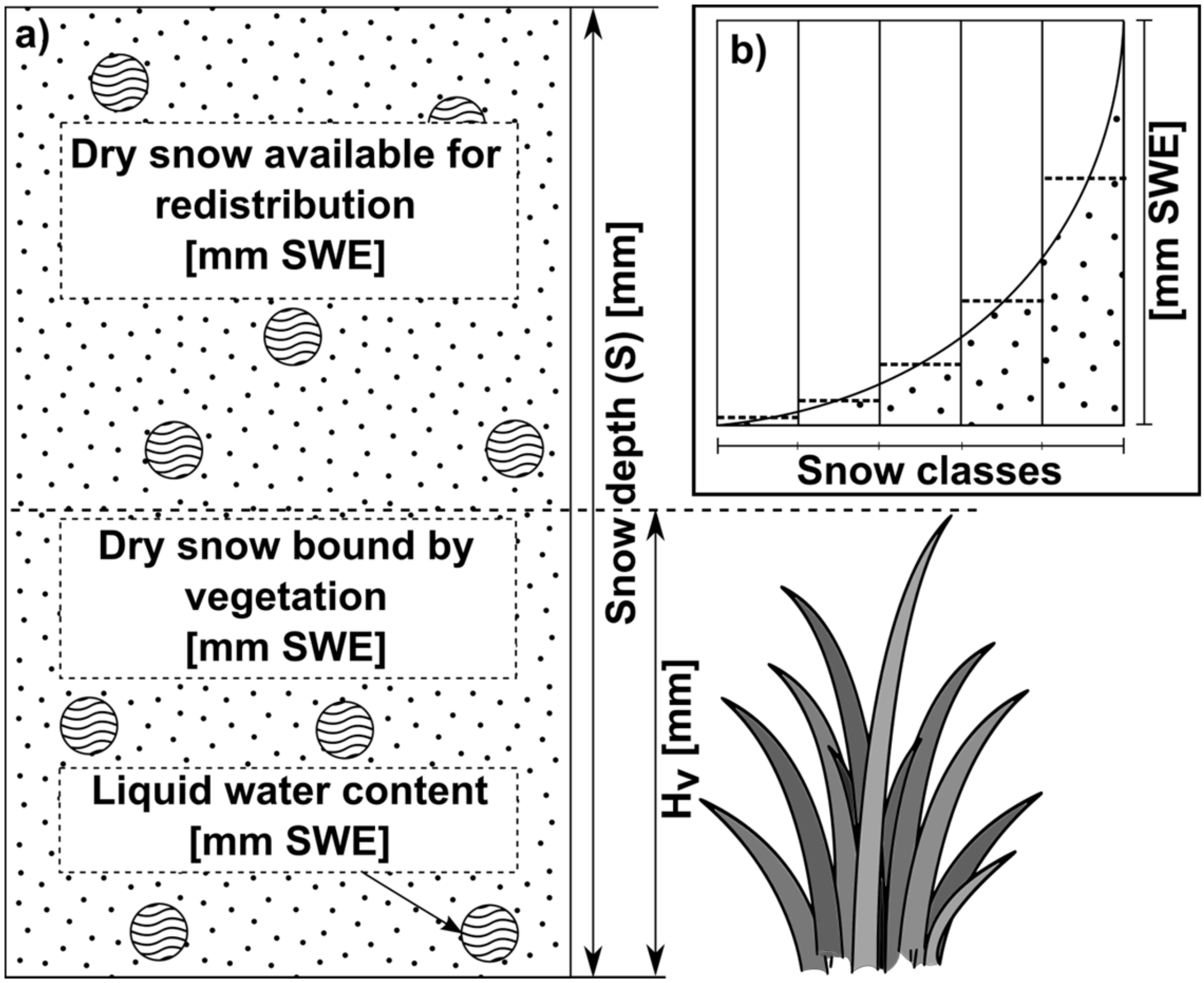

Figure 1. Schematic view of the snow cover in COSERO. a) Composition of one snow class. Vegetation or surface roughness defines the threshold value (H_V) to hold back an amount of snow. b) View of one grid cell including five snow classes each of which is composed in the way shown in a). Snowfall is distributed log-normally throughout the classes (dashed lines in b)). This distribution may be disturbed by subsequent processes of melting, redistribution to other grid cells and sublimation. Snow redistribution between the snow classes of the same grid cell is not considered. Note that snow depth S is given in mm while all other parameters regarding snow are given in mm SWE.

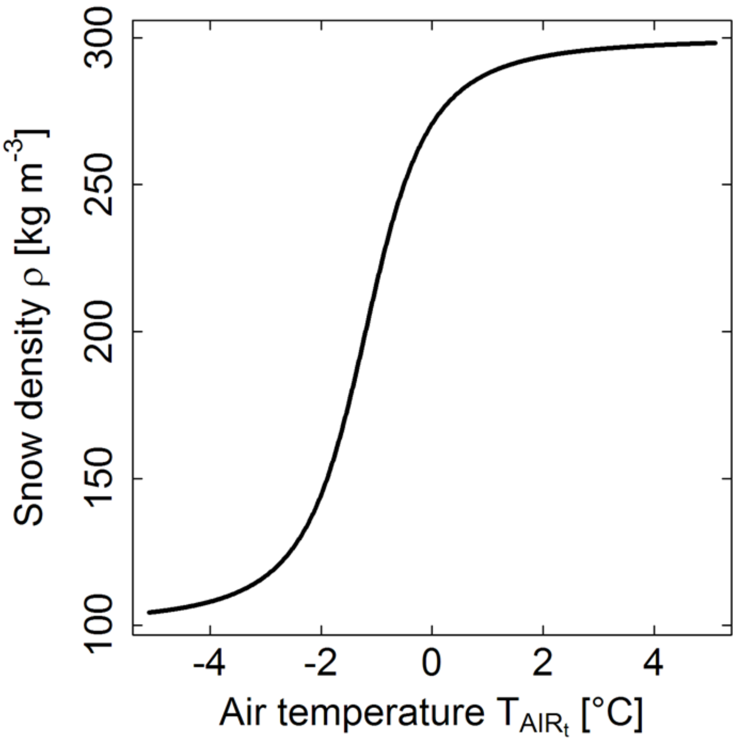

Figure 2. Estimation of the density of snow using Eqs. (8) and (9). Minimum and maximum densities of fresh snow are 100 and 300 kg m^{-3}, respectively. Standard values for ρ_{scale} and T_{scale} are 1.2 and 1, respectively.

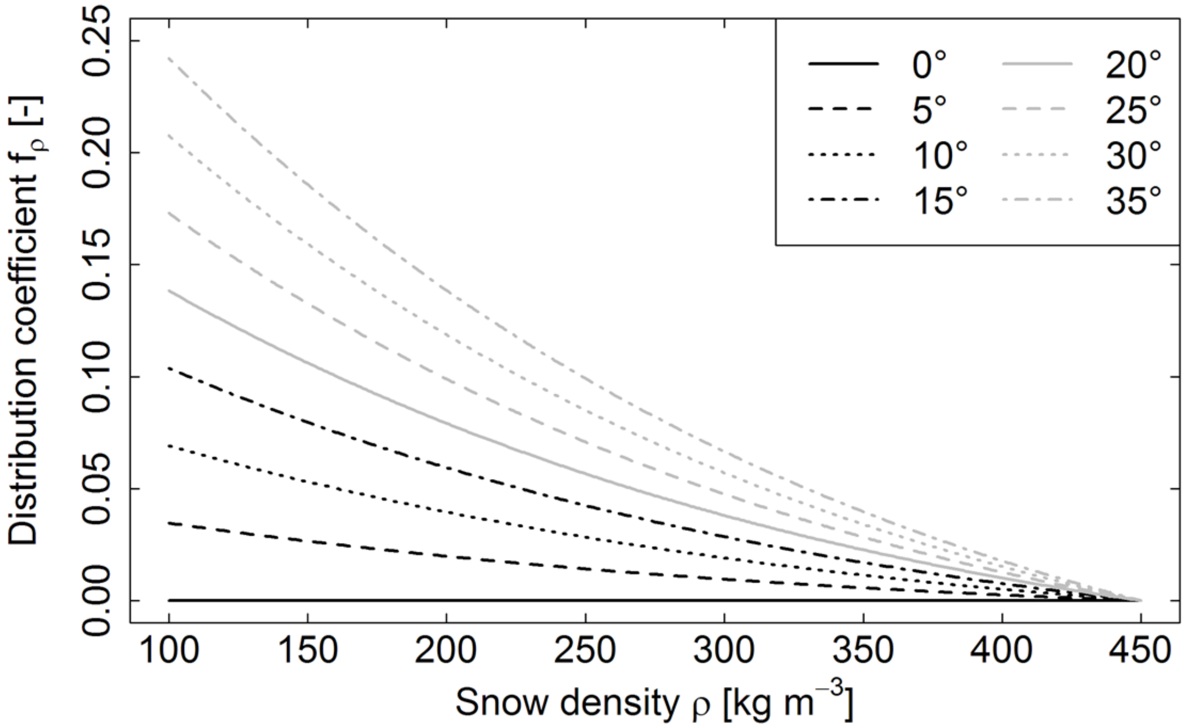

Figure 3. Shapes of the distribution coefficient in dependency of different slope angles and snow densities. If cold snow with a density of 100 kg m^{-3} is located on a slope of 35°, a portion of 25% of the available snow is transported to the neighbour cell. If the snow density reaches its maximum value, no transport occurs regardless of the slope angle.

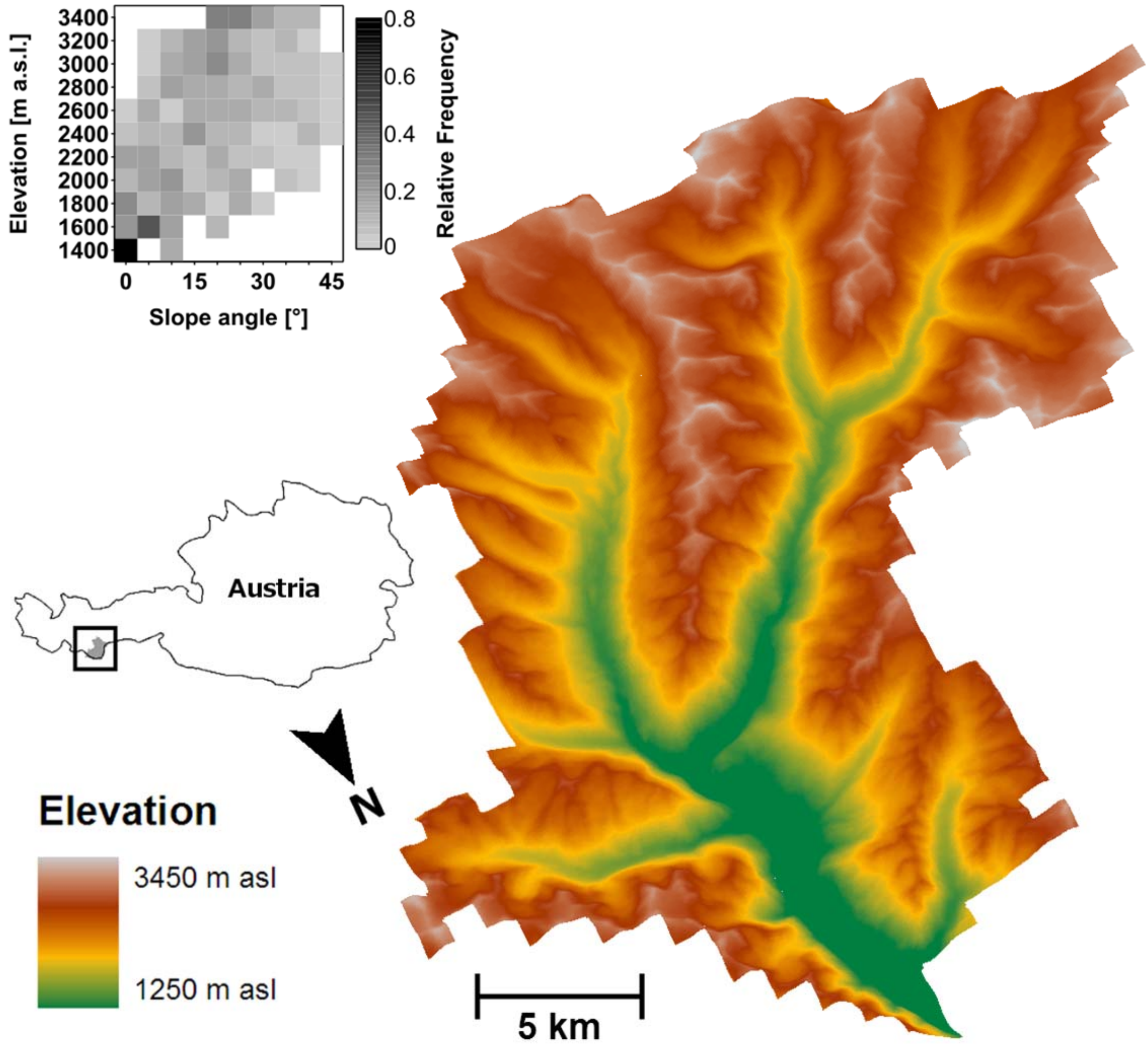

Figure 4. Elevation levels of the Ötztal using a 1x1 km grid. Frequency distribution of slope angles derived from 1x1 km grid are shown (upper left). Slopes in general are steeper in the summit regions than in the valleys. However, glacier covered areas at the summits are rather flat. Note that instead of the average slope of a grid cell only steepest vertical gradients are plotted.

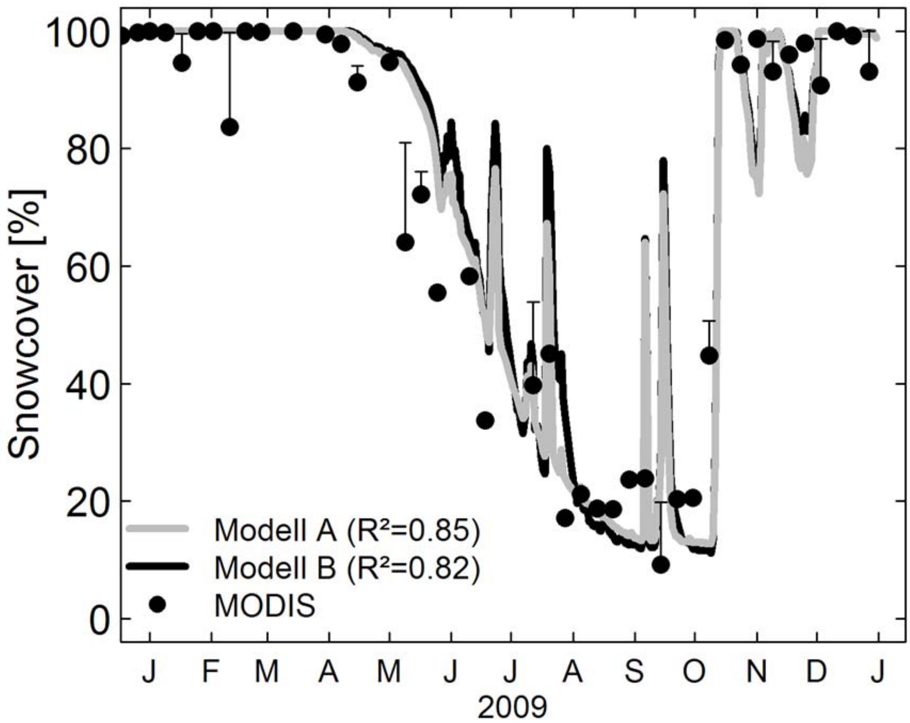

Figure 5. Snow cover in 2009 modelled by both model A and B compared with MODIS data. Reason of the little difference is the vegetation threshold. Even if snow is being transported, a residual of snow remains in the donor cell resulting in the cell marked as snow covered. Error bars refer to uncertainties due to cloud coverage.