

Interactive comment on “Efficient multi-objective calibration and uncertainty analysis of distributed snow simulations in rugged alpine terrain” by James M. Thornton et al.

Anonymous Referee #2

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The authors present a multi-objective calibration strategy for a distributed snowcover and hydrology model WaSIM. This approach used Landsat 8 derived snow cover area as well as reconstructed snow water equivalent derived from observed snow depth and modelled and statistical density estimates to calibrate the snowmodel. This model was then applied to a basin in the Swiss Alps.

Although the authors have done a lot of work, I cannot recommend that this paper be published in its current form. The bulk of this manuscript relies upon the analysis of a temporally and spatially variable simulated snowcover in complex terrain. However, the snow processes used in this model do not reflect the current state of the art in cold

C1

regions hydrology. That is, the modelling approach lacks the cutting-edge advances that a reader would expect to see in The Cryosphere for snowcover simulation in complex terrain. Further, there are many missing details regarding, for example, the initial conditions of critical components of the hydrological system, such as soils. Without these details it becomes difficult to fully evaluate the model and results.

Below I detail my specific concerns with the overall methodology.

The snow/rain phase determination relies on a calibrated air temperature threshold method. These air temperature threshold methods are known to be less reliable than methods that consider atmospheric humidity or hydrometeor energy balances, e.g., Harder, et al (2013); Marks, et al (2013); Harpold, et al, (2017); Jennings, et al (2018). This is critically important for the mixed-phase conditions, which appear to likely occur at the lower elevation site as evinced by the mid-winter melts. If I understand correctly, wind undercatch is done in eqn1, and phase in eqn 3, however different thresholds are used, and the phase via eqn 3 does not inform the phase choice in eqn1 for undercatch. This seems counter the standard way of accounting for undercatch and phase. Phase determination should be done first, then the correct undercatch correction applied. The method presented here is unreferenced and without verification. Precipitation gauge undercatch has established correction factors (e.g., SPICE intercomparison project (Nitu, et al, 2012; Rasmussen, et al. 2012)) which should be used separately from phase determination.

A one-layer snowpack model is generally considered insufficient for deep mountain snowpacks, such as those in excess of 2000 mm presented here, e.g., Debeer, et al, (2010); Jennings, et al, (2018). Deep snowpacks develop vertical thermal gradients that are not represented with a one-layer model. Exchange layers are critical to accurately estimate outgoing longwave radiation (e.g., skin temperatures Essery, et al, (2015); Pomeroy, et al, (2016); Lafaysse, et al, (2017)), sensible heat fluxes, heat conduction, and ablation (i.e., cold content estimation with positive surface fluxes, Jennings, et al (2018)). The temperature at the base of the deep snowpack should not

C2

directly inform the surface exchanges. I am not suggesting that the authors must use a many-layer snowpack model like SNOWPACK or Crocus. A few layers would be sufficient. I suspect this omission is why the energy balance and longwave calibration is required to correct for these missing processes.

During the energy balance model description, the text states that “. . .and [if] air temperature is favourable, melt can occur”. This must not actually be what the authors intend. The entire purpose of an energy balance model is that air temperature does not determine melt, but rather that the surface energy balance does. If this is what is meant, then there is a misinterpretation of energy balance snowmelt models and the processes surrounding them. As it stands, this and the one-layer model are major limitations and deficiencies of the methodology.

I understand from the text that blowing snow was not included in the final simulation. However, they describe blowing snow as a simple precipitation correction based on Winstal's S_x parameter. This is incorrect. Rather, blowing snow mass is transported downwind through a fetch dependent turbulent advection-diffusion process due to entrainment of snow particles from the surface into the saltation layer once surface threshold shear stresses are surpassed. Significant mass loss is experienced with via blowing snow sublimation of in-transport particles due to their well-ventilated nature. The decrease in lee-side windspeeds causes entrained particles to deposit on the surface. I would encourage the authors to read the recent review by Mott, et al (2018). Note that this is a separate process from preferential deposition process as described by Lehning, et al (2008); Mott, et al, (2014).

Albedo decay is critical for energy balance snow cover modelling (Krinner, et al, 2018) and this is not described or cited. Albedo impacts the cold content of layers in the snowcover, in turn affecting outgoing longwave calculation and turbulent heat fluxes.

Incoming longwave radiation is impacted by cloudiness. There are many established parameterizations for this process based on T and RH that surpass pure calibration

C3

with respect to transferability and physical basis. It seems that the approach herein is to calibrate the meteorological data via the correction factors? How is sub-canopy longwave handled?

The turbulent transfer approach is not detailed in the manuscript but looking at the model documentation suggests that there is no stability correction. Although issues with MO parameterizations causing excessive stability are known, none of these issues are touched upon nor the approach taken discussed (I assume it is a neutral stability assumption?).

Potential evapotranspiration is meaningless for this area due to there not being an infinite supply of water. Again, looking at the model code suggests that calculated PET is scaled by a soil water content. However, no soils information is provided, nor is there any indication as to what the initial conditions for the soils are. Further, why is PET even calculated for snowcovered surfaces?

Frozen soil infiltration is a critical component of cold regions runoff (Gray et al, 2001), however this process is not described, and it is not clear if it is used. This again relates to the initial conditions for the soil moisture and type. As well, it impacts the runoff values presented later.

There appears to be vegetation in this basin, however this is not particularly clear – Figure 1 is small. There is no description of canopy interception processes. Intercepted snowfall in tree canopies can lead to high sublimation rates, which is not detailed or considered here.

Estimation of complex terrain wind velocities cannot simply be done by lapsing and spatially interpolating windspeeds. Windspeeds near crests increase, which is an important aspect of midwinter snowpack sublimation and blowing snow. Indeed, this can result in snow-free ridges in many snowpack models. Even the most rudimentary of wind speed interpolation models such as Liston, et al. (2006) would have better success in preserving the spatial pattern of wind velocity.

C4

The calibration ranges as presented in Table 1 suggests a complete lack of physical basis and identifiability. For example, the air temperature lower-range is 0.000001C. The lower limit on “scmd” is equally nonsense – 0.0000001 kg/m² is measuring micrograms! As a modeller, I understand the desire to force the calibration scheme to behave. However, if this is to be presented as a physically based work, using identifiable parameters and values, such nonsense values cannot be used. Even a parameter like scmd, clearly a calibration parameter, should be constrained to reasonable values – values representing 0.00001% of a snowpack suggests that either this value is not relevant to the calibration or the model is unduly sensitive to tiny values, further undermining the claim on a physically based approach.

The radiation balance is not well described but it also raises further concerns. How is sub-canopy radiation calculated? Line 275 on page 11 suggests self-shadowing is considered, e.g., cosine slope correction of radiation. In complex terrain, horizon shadows (terrain shadowing other terrain) are a key process that results in late lying snowpacks and impacts the surface energy balance on the snow (e.g, net longwave, turbulent fluxes). There are many algorithms for this, such as Dozier and Frew (1990). I am sympathetic to not adding new process to a model being used, however I cannot help but expect that the longwave calibration factors are compensation for this process. Yet, there is no description of it in the text. Given all the other ignored processes that are key processes of high mountain hydrology, I worry the calibration process is compensating for major conceptual issues throughout the model. Without a discussion on this, it is difficult to fully quantify the magnitude of this.

The snowcover maps presented in Figure 3 concerns me. The authors first note an “automated calibration scheme”, but then note a “bespoke” per-image NDSI threshold. As noted, this greatly limits the applicability of this approach. I am perplexed with some of the sub-figures in Fig 3. For example, 2017.11.14, 2018.02.02, 2018.03.22 have what seems to be entirely complete snowcover. Certainly, this is possible. But I am surprised that it occurs with such regularity and that no mass transport or sublimation

C5

processes quickly cause there to be snow-free pixels. This will have enormous weight in the calibration scheme, especially if these are used with a vertical mass transport model. Further to the point about canopy interception, on line 327 p 14 the authors state “under dark forest”. However, there does not seem to be any effort to distinguish between snow on the ground or snow in the canopy during NDSI calculation. I would expect this to impact calibration efforts. Were these areas masked out? This is a large uncertainty that does not seem to be addressed.

The SWE presented in Fig 5 suggests that the model does not perform adequately. During 2015 the GRY site peak SWE appears to be under-estimated by ~90%. This seems rather egregious if the goal is to provide water managers accurate and timely SWE estimates, as stated in the introduction. Similarly, 2016 has a ~2x error at COR and a 5x error at GRY. Certainly, models do not get every hindcast year right, but given the uncertainty in the reconstructed SWE as well as vast number of seemingly missing processes in the model, it is not clear that this error is down to uncertainty in reconstruction only. A 25% success rate on predicting SWE at the low elevation suggests significant deficit in the model. Given the mid-winter melt at this site, and that mid-winter melts are considered to become more likely (Musselman, et al 2017) it suggests this model is likely of limited use with future climate change scenarios.

I have concerns with the reconstructed SWE. Because the SWE is computed via a modelled density, this is essentially comparing the density model of SNOWPACK versus the density model of WaSIM + uncertainty in precipitation interpolation. The densification processes via compaction and settling are so complex that most models tend to have considerable uncertainty in simulated snowdepths. SNOWPACK is advanced in this regard due to its design as an avalanche hazard model, however it still has uncertainty in the densification processes. This is fine, however there is no discussion on this in the text. This comparison must consider this outside of an arbitrary (?) +/-20% uncertainty bound. The lack of in-basin observations is a major limitation. Without any in-basin values, it is not at all clear that the reconstructed SWE values are representa-

C6

tive of in-basin values.

I am missing what I would expect to be a standard description of the basin's instrumentation. This makes it difficult to follow the text. I am perplexed that the authors note that non-heated snowfall gauges were ignored. Non-heated snowfall gauges are the norm in every research basin except those with large mains power, which are a rarity. Perhaps it is just that the text is unclear, but this requires further elaboration.

I am further missing a complete description of the basin, of the observed climate, of the vegetation, soils, rivers, et cetera. I am missing a complete map of these in the main text. A better figure is present in the supplement, but this needs to be in the main text. Further, contour lines or a DEM would greatly aid in understanding the topography of the basin.

I am not convinced that the use of runoff without an overland routing model is appropriate. Mountain catchments do not instantly transmit surface waters to the rivers. There is soil infiltration, ground water recharge, and surface detention in ponds. I see WaSIM has an overland flow model for cell sizes <10 m – could this not have been used to route to the channel? Figure 7 b suggests, I guess, that the basin is flashy and that high melt rates are correlated with higher discharge at the weir, without considering any routing. Without any information on the soils, etc of the basin though, this is difficult to make much sense of or, importantly, transfer to other research sites. The note about integration into a 3D bedrock model suggest that the authors think there is groundwater recharge and groundwater contribution in the basin. This further suggests that no routing and overland flow is problematic.

The description of the interpolation is unclear; however, it reads as if no common elevation was used for the interpolation (e.g., Liston, et al 2006). That is, the stations' data were interpolated and then elevation corrected, thus incorporating an elevation dependence into the interpolation. This is not correct. This needs further description to ensure that this is not what is being done. In addition, IDW can cause significant

C7

“wells” around stations as well as being poorly numerically conditioned. I would like the authors to describe why they chose IDW versus a spline, for example, which I understand to also be in WaSIM.

Lastly, the glacier model comes out of nowhere in the description and is not well detailed. Dynamic glacier models open up an entirely new set of uncertainties and difficulties that does not seem to be addressed here. Without details on how exactly this model was initialized (i.e., what is the initial state of the ice? Depth? Firn? Etc), it's impossible to evaluate this aspect of the manuscript.

In summary, the model methodology as presented herein is missing critical processes known to be important for cold regions hydrology in complex terrain, and those that are included appear to be potentially misapplied. There is also a substantial lack of details on initial conditions, parameters, and limited site descriptions.

Detailed comments

In general, I found this paper to be quite long and difficult to follow. The English tends to be conversational and needlessly verbose.

P2. L.13 “they” should this be “these data”?

P2. L.13 “exploited” word choice

P2. L.14 remove “-based”

P2. L.18 “practically the full range of possible snow cover conditions” has this really been shown?

P2. L.21 pt i) is wordy. Further, i) is well known, ii) seems to be a model formulation limitation, iii) does not seem to be a novel finding

P3. L.35 “Under established climatic conditions” unclear what this means

P3. L.44 Add Musselman 2017, Viviroli 2007

C8

P3. L. 47 Add “changes to” patterns of liquid?

P3. L. 50 missing thesis statement, unclear what the paragraph trying to tell the reader

P3. L. Throughout, I would suggest avoiding conversational terms and expressions like “increasingly appreciate that”

P4. L.81 I am surprised by the references here. Missing a lot of the detailed Alpine3D work, iSnoal, CRHM, CROCUS, etc done by Lehning, Mott, Marks, Pomeroy, Layfasse and Vionnet to name but a few, as well as obs+model hybrids like ASO.

P4. L. 83 “Firstly”, then “Secondly”. It’s unclear what these are item enumerations are in reference to.

P4. L. 85 These are called sub-grid processes

P5. L.96 “certain applications” – such as?

P5. L. 97 “small” = what length scale ?

P5. L.104 Wayand, et al (2018) considered hundreds of images from Sentinel-2A and Landsat 8.

P5. L. 106 Standard in what context? There is a strong adoption of energy balance models to provide robust estimates outside of calibration periods, e.g., climate change predictions.

P5. L. 113 remove “much”

P5. L. 118 what does “explicitly physical fashion” mean?

P7. L. 167 the dynamic glacier model comes out of no where

P. 7. Study area. As stated above this is missing significant details such as veg cover, climatic information, etc.

P.8 Figure 1 caption: is © swisstopo really the correct way to cite these data? P.8 L.184

C9

“Abundant precipitation”, how much?

P.9 L. 205 What is IDAWEB? Citation is missing a date.

P.9. L. 214 “local station data” which stations?

P.9 L.215 How were these corrections actually done? What algorithms?

P.9 L.217 fix niVis citation, also unclear what this software really is

P10. L.239 “this parameter” what parameter does this refer to, specifically?

P11. L.274 I do not have Oke, 1987 handy, and I do not recall the temperature shading method used therein. Please detail.

P11. L.274 What is radc? Describe

P11. L.275 WaSIM comes out of nowhere, and without any detail. This model needs to be better described and introduced.

P12. L.300 Define “satisfactory”

P12. L. 303 Define EPSG

P14 L.325 Remove “The present study was no. . .”

P14 L. 327 “Dark forest”, first mention of this, what are implications for modelling (e.g., interception, radiation)? Landsat tiles? Etc

P14 L.328 “Very pleasing” what does this mean? Is this numerically quantified or purely subjective?

P15 L.335 “fuller information”, “interrelations” grammar

P15 L 339 “IMIS” write this out and fix n.d. citation

P15 L 346 “height” should be depth

P16 L 361 How often do these flows exceed the rating curve?

C10

P16 L 365 “thorough review” There are a ton of fantastic energy balance snow models. It is really not clear to me that WaSIM is better, or what the evaluation criteria were here.

P16 L.367 I thought redistribution was not used?

P16 L376 What determined $T_{trans}=1\text{degC}$?

P16 L388 What determined 2W m^{-2} ?

P17 L 389-391 As I described above, this is a fundamental problem with this model. Using an energy balance model with air temperature constraints on melt defeats the purpose of using an energy balance model.

P17 L398 the lw corrections for lw out are also compensating for a lack of skin temperature and the single bulk layer’s temperature being used for lw out. Further, I imagine this will also compensate for the errors in turbulent fluxes as a result of the homogenous temperature.

P17 410 The purpose of frss is not clear. Why can only 1% of the snowpack slide?

P18 L453 “observed extent map had been produced” these are the landsat data, correct?

P20 Figure 4, personally I find this figure confusing. I.e., what are the % values in the figure? They are not shown in the caption.

P21 L510 is “coupled” really what you mean? I am not familiar with PEST, but I presume it is a driver that calls WaSIM and is not numerically coupled to WaSIM (as may be interpreted from the use of “coupled”)

P21 L520 “predictions (this term is not used here in a future sense)” I do not understand this as predictions are always a future event/forecast.

P21 L526 What is a genuine future prediction?

C11

P22 L530 I assume GENLINPRED is “Generalized linear prediction” or similar? This should be stated, and not just the internal acronym used.

P22 L541 “to account for undercatch”, I understand this to mean eq1? I would state that explicitly or just remove it.

P22 L544 The glaciation treatment is vague. This should be better described. I.e., why is this glacier cover not described in the area section?

P23 L564 n.d. citations. Should also specify GDAL version

P23 L566 n.d. citation again (fix throughout)

P23 L566 remove “again”

P23 L569 “fuller example” grammar issue. Also unclear what “to this end” refers to. Suggest you remove this sentence.

P23 L571 Define “physically-based”, as so far most of these methods are heavily calibrated and empirical

P23 L577 No ground water? Routing?

P24 Table 3 would benefit from plain text col headers

P26 L609 There are no standard numerical quantification of Figure 5 such as RMSE, bias, etc. This section is very qualitative.

P26 L610 “perfect simulator” This is not clear, seems very out of place. Of course, we know models not to be perfect and to have uncertainty.

P27 L634 The mid-winter melt in Figure 5 suggests otherwise?

P28 Figure 6 may benefit from having units changed to mm to match other figures’ units

P38 L809 “Turning our attention to”, conversational verbiage

C12

P38 L811 Are not two of the data sets essentially the inverse of the other?

P39 L855 I don't think this was substantiated in the results that blowing snow is of negligible importance. Other authors have identified this process as critical in most mountain basins, including the Alps.

P39 S 6.6 This section misses a lot of the literature on the merits of not calibration snow models. Likely best to have this as part of the introduction to support the methodology taken in this paper?

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C13

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C14

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