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Hydrology and Earth System Sciences
Discussions
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Interactive comment on "How to simulate radiative inputs in complex topographic areas, an analysis on 115 Swiss Alps weather stations" *by* Philippe Riboust et al.

Anonymous Referee #1

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General comments:

The paper deals with, in my view, an important topic; "the introduction of more physically based (snow) models while acknowledging a lack of forcing data". I believe this is the way forward to make progress both for PUB and for predicting hydrology under a changed climate. As I read the paper, the study tries to improve the transmissivity algorithm of Bristow and Campbell by taking into account elevation and topography. This would obviously improve the SW estimation and, through the Sicart formula for emissivity, the improved transmissivity would improve the estimation of atmospheric LW. The chosen method involves a lot of parameters that need to be calibrated (i.e.

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needs a lot of location specific (?) data, which we are trying to avoid) and even if the method gave improved SW- estimates for high altitudes, the results for lower elevations and for LW were not as was hoped for. In its current state, this is not a method I would implement or can recommend. I do think, however, that using all relevant, easily obtainable information (for example elevation and topography) to improve the algorithms for more physically based models, such as energy balance (EB) modelling, is a good idea. I believe the paper needs major revision, and possibly some revisions in the method as well before it can be considered for publication. The following points need to be addressed:

1) I think, but I am not sure as it is not clearly stated, that the temporal scale is daily. For snow models, the potential gain in using EB models is for finer temporal scales. At daily time scales well calibrated (against snow data) degree-day models does the job quite nicely. I believe this was the conclusions of Anderson (1976-77). This point needs to be addressed quite early and revisited in the discussion.

2)You introduce a lot of parameters to be calibrated and state yourself that the model is overparameterised. If some physically based reason (model without calibration parameters) for why, for example τ_max increases with altitude could be introduced, then the model will have less freedom and be easier to diagnose (why doesn't it work). The dependence on calibration is also a problem for using the models for other temporal resolutions (where to find the data?).

3)I think the paper has a serious problem with notation. It is very confusing to have the letter "T" surrounded by some many super- and subscripts. I quickly lost track over what was what and this is especially difficult in 4.2. Notations like ãĂŰ∆TãĂŮ_param and T_(s,ref)^+ are not very helpful. Have the units in [] instead of (). Explain the variables directly after an equation (see deltaTref at page 4 L20 and Eq. 9). In Eq. 14 you have three different epsilons, are all emmisivities?

4)I kept wondering why you have 4.2. It comes as an extra exercise at the end of the

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paper and I did not think it helped to clarify matters.

5)Can you discuss your method in relation to what has been done internationally in the field?

Specific comments:

Title: “How to simulate..” appears a bit too confident. How about the somewhat more humble “Empirical modelling of radiative . . .

Abstract„ L3 : change scarce for coarse? and ..fluxes in areas of complex topography Introduce the temporal resolution in the abstract.

P1.L13: what is retro-action, do you perhaps mean feedback??

P2.L33: spell out UEB and this sentence need reformulation, . . .which signal?

P3.L1 You say that errors on LW have a great impact on SWE(P2.L34), and conclude that it doesn’t?

P3.L14 Here is the temporal resolution first introduced. Should be much earlier. I think you can have a more thorough outline of the study in the introduction.

P4.L1 with Rpot being the. . .

P4.L13 space dependent

P4.L20: reformulate.. a parameterization for the reference parameter for the daily temperature range.

P5.L11. start the paragraph with We want to ..

P5.L12 Fig3b?

P5.L20 . . .parameter is proportional to the mean..

P5.L22 Figure 3a?

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P5.L23 it appears quite linear to me..

P5.L26 ..range, and mountain..

P5.L30 the differences in elevation is denoted δ

P6.L4 point instead of area? And the average elevation difference noted.. How did you test?

P6.L7 If δ ÌĚ. . . and so on for the next lines

P6.L19 which is the average

P6.L21 We do not know that the range depends on. . .

P7.L30 reformulate sentence

P8.L18.. humidity is needed.

P8.L24 What happened to Tetens equation?

P9.L8 Pvap not defined

P9.L10 ..point, as humidity is the ..

P9.L15. Be specific about the stations, not “others” and “these”..

P9.L27 mu(mu)???

P12.L1 u_prec??

P12. Eq. 24,25 no wind speed?

P12.L25.. is assumed to be negligible compared to other energy fluxes

P15.L3 retro-action again

P15.L17 generality

Figure 6. Eq. 16 does not yield vapor pressure

Figure 8 what are the circles. What does the letter indicate? There are no two identical letters. Do you really need to compare so many models? We lose track of which is which with the uninformative abbreviations.

Figure 12. Lots of unexplained dots

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