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Hydrology and Earth System Sciences Discussions

Interactive comment on "Snow glacier melt estimation in tropical Andean glaciers using Artificial Neural Networks" *by* V. Moya Quiroga et al.

Anonymous Referee #1

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Review of "Snow glacier melt estimation in tropical Andean glaciers using Artificial Neural Networks" by Moya Quiroga et al.

General comments

The authors present the application of artificial neural networks (ANN) for the modeling of glacier melt in two Bolivian glaciers. The main objective is to assess the potential of ANN to simulate hourly melting rates using only air temperature and solar radiation data. The training dataset is generated using an energy balance model, which is forced by a set of meteorological data measured near the Zongo glacier. The authors

found that additional variables (relative humidity and melt from the previous timestep) increased the accuracy of the ANN. Then the ANN is applied to the Condoriri glacier where available meteorological data preclude the application of an energy balance model. The results are used to describe the temporal variability of snow and ice melt in this glacier. It is an interesting attempt to reduce the number of meteorological variables required to simulate glacier melt for many purposes (climate change impact studies, paleoclimate reconstruction, hydrological modeling, etc.). However I see several issues in this paper:

1) I did not understand if the melt model was applied to the glacier scale or pointwise: (i) the authors used annual glacier-scale melt measurements to validate the energy balance model (see comment 3. below) (ii) the input data for the energy balance model were obtained from the ORE GLACIOCLIM meteorological station, which is located on the lateral moraine of the Zongo glacier (5050 m). Apparently the authors did not extrapolate the meteorological data to the glacier elevation range (4900-6000 m) so that the computed melting rates are not representative of the whole glacier. In the end I was lost on what the authors intended to simulate with the ANN.

2) Lack of reference to previous work. It seems that some references cited in the introduction were randomly selected among the scientific literature dealing with snow and glaciers. A well-known similar study by Pellicciotti et al. (2005) was omitted. A careful review of the literature would help the authors to better discuss their results like: Why not distinguishing snow vs. bare ice? Why is relative humidity a critical variable in this area (maybe because air humidity is correlated to incoming longwave radiation, which is a key variable of the tropical glacier energy balance according to Sicart et al. 2005)?

3) In my opinion, the performance of the ANN should be compared to the performances of the “classical” temperature index model, and its variants incorporating shortwave radiation (Hock 1999, Pellicciotti et al., 2005); otherwise the results are hardly useful to the reader.

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4) Lack of validation data. I understand that the energy balance model is used as a reference to generate training data for the ANN, because continuous melting rates are virtually impossible to measure at the hourly timestep. However, the energy balance model should have been validated first, e.g. using ablation stake data (collected every month on the Zongo glacier). The only "validation" presented by the authors (Fig. 5) is not sufficient for two reasons : (i) annual data are used to validate a hourly model (ii) there is no explanation on the origin of these data (the IRD report by Perroy is not accessible to HESS readers). Note that Sicart et al. (2005) also applied an energy balance model to the Zongo glacier but used atmospheric stability correction factors.

5) The limitations of ANN should be discussed: (i) what are the assumptions required to transpose a calibrated ANN to another glacier? (ii) ANN have an inherent extrapolation problem (Hettiarachchi et al., 2005), hence I wonder if ANN are an appropriate tool to simulate glacier melt under changing climate conditions as indicated in introduction?

Minor comments

Avoid general statements such as: "Any human activity relates somehow to water, but unfortunately it is not a renewable resource etc..." (water is actually renewable at the global scale)

"This classical approach of calibration introduced by Carl Friedrich Gauss in the 18th century is not realistic" (I would personally not pretend to do a better job than Gauss)

Fig. 6-7 should be compared with data or discussed based on published work.

Fig. 8-12: I am not sure if all these scatterplots are useful, but at least they should be presented with equal axes.

Fig. 18 is redundant since the mean hourly values were already presented in Fig 14-17. However, the curves in Fig. 18 do not match the black line in Fig. 14-17 (maybe outliers were excluded to compute the mean?).

References

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Hock, 1999, A distributed temperature-index ice-and snowmelt model including potential direct solar radiation, Journal of Glaciology

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