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Hydrology and Earth System Sciences Discussions

Interactive comment on "Modelling the hydrologic role of glaciers within a Water Evaluation and Planning System (WEAP): a case study in the Rio Santa watershed (Peru)" *by* T. Condom et al.

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This paper proposes to use a semi-distributed hydrological model to compute the water flow from the Rio Santa watershed in Peru. Two reviewers have commented on the paper. Given that modeling the runoff from mountainous watersheds is my own field of specialization, I decided to not wait for a third.

Both reviewers give ample evidence for the shortcomings of this manuscript; while the more technical problems (insufficient, confusing or wrong methodological details) could certainly be addressed during a revision of the manuscript, the problems related to the poor hydrological model performance and the numerous ad-hoc decisions about the model structure and calibration seem impossible to be addressed in a reasonable delay.

In fact, the authors do not give satisfying answers to the main critics of the reviewers. This in particular applies to the comments about model performance but also to some model formulation choices.

The authors say in the paper and in the comments that the model has a satisfying performance. I agree with the reviewers that this is not the case. Overall, Nash-Sutcliff efficiencies are very low; a value of 0.6 might be acceptable for an hourly or daily model in a rainfall dominated catchment. For a monthly time step and for strong annual cycles, much higher values are expected (see also Schaefli and Gupta, Hydrological Processes, 2007). Besides, a performance decrease from e.g. 0.65 during calibration to 0.19 during validation with -18% bias (Quitarasca) calls for a model structure modification or a different calibration. A model with such strong performance variation cannot be assumed to have predictive power.

The presented time series consistently only refer to relatively good performing sub-watersheds (e.g. Fig. 6). Furthermore, the detailed time series for La Balsa (Fig. 5, among the better performances) does not provide sufficient evidence of "the ability of the model to represent the inter-annual variations." The simulation consistently underestimates the observed runoff and while the variations seem to follow the observed variations well at the end of the time series, the opposite is the case at the beginning; the standard deviation of the observed series is around 15, of the simulated only around 10 m3/s, which is a considerable difference. The bias is around 15 %, which is very different from the 3% reported in Table 3.

Furthermore, the authors provide no evidence that their snow and ice melt routine is useful in the given climate setting and namely that the degree-day method can account for sublimation. In addition, given the low model performance, applying the same parameter values over all sub-watersheds is not a convincing option (invoking model

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robustness is no real argument, why should a model be robust just because you use constant parameter values?). In a context where the model is supposed to provide insights into different origins of water flow components (glacier melt versus groundwater), blindly applying the same parameters to all sub-catchments rather than some degree of regionalization seems difficult to justify.

Invoking the "interest of creating a general modeling framework" is certainly not sufficient to justify such choices. For all these reasons, I cannot recommend this manuscript for publication in HESS.

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Interactive Comment