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Hydrology and Earth System Sciences Discussions

Interactive comment on "Estimation of future glaciation and runoff in the Tanimas basin, Eastern Pamirs" *by* W. Hagg et al.

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I read the responses of the authors with interest and would like to comment them. As the authors state in their answer, this paper "provides results from an underreported and extremely important and scientifically, ecologically and politically interesting region". This means that it is even more important to present evidence that their approach provides good estimates of current and future water balance terms.

Estimation of water balance terms

In their answer, the authors say " We cannot see how we should check the other terms of the water balance. There are no measured values of basin precipitation or evapotranspiration. Remote sensing products are not available for the period with observed

data."

An important amount of research in hydrology is dedicated to find solutions to this important problem (prediction in ungauged basins). While I do not expect the authors to find miraculous solutions, the above answer is to my viewpoint very difficult to defend. Are we at least sure about the order of magnitude of evapotranspiration in this basin? And of precipitation ? And of glacier melt? The glacier melt is indicated as -1.1 m/yr of glacier melt during 5 years in the 1980ies. Is this reasonable? Is this a moderate mass loss? (see the paper "In any case, moderate mass losses compared to more maritime mountain ranges were also confirmed by repeat photographs in the Tanimas valley (Braun and Hagg, 2009) and by a new GPS survey on Fedchenko glacier in 2009 (Lambrecht et al., 2010). As a consequence, the only frame we had to constrain parameters in terms of icemelt was to avoid strongly negative mass changes, balanced conditions or a gain in glacier mass.)

The meteorological station measures around 150 mm/yr, the estimated mean catchment precipitation is indicated as around 300 mm/yr for the control period (not the same period). Could it also be 600 mm/yr?

In their answer, the authors give the very important indication that the non-glaciated catchment area shows almost no vegetation and that this pattern is unlikely to change. The HBV evapotranspiration (ET) routine is designed for vegetated areas since it computes ET as a function of soil moisture filling (according to the information given in this paper). Only vegetation can empty the soil moisture store, pure soil evaporation can take place only in the first few mm of the soil and would to my view need some other, soil-interception-based approach (Savenije, 2004).

The paper says that 72% of the precipitation falls during the period when PET is lower than ETMAX/2 (Dec - May). Ignoring that most of this precipitation falls in solid form, this anti-cyclic precipitation - PET pattern can only lead to around 200 mm/yr evaporation per year if it rains on average every day throughout the year. This value is

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obtained by applying eq. 12 of (Gerrits et al., 2009), assuming monthly precipitation evenly distributed within the wet season (average 35 mm/month) and within the dry season (14 mm/month) and a ground interception threshold = ETMAX. Assuming that the expected rainfall on a rainday is equal to the average daily precipitation within a month (i.e. it rains on average every day), this leads to a total amount of interception on the ground, I, of 282 mm/yr, of which 185 mm/yr are re-evaporated if PET is limiting (computed as E(t)=min[I(t),PET(t)], where t is the month).

Any stronger rainfall intermittency would reduce the evaporation (and the real evaporation is certainly overestimated with the above approach ignoring the presence of snowfall). In light of this, are the estimated area-average precipitation, evaporation and potential evaporation reasonable? Given that all these terms are estimated based on observed discharge, how can we be confident that the water balance is not completely different? (It is noteworthy that the authors say in their answer that the chosen PET formulation is "applicable in head watersheds, where actual evapotranspiration is of minor quantitative importance for the water balance", which is contradiction to the case here).

Finally, even if conceptual models simulate water balance terms in a lumped way through parameters with limited meaning, the resulting simulated water balance terms have of course to be comparable to observed values, otherwise the whole modeling exercise becomes absurd (see the authors response "Evaporation losses depend on this mean humidity, which makes it impossible to relate modeled values to data derived from other sources.")

Model calibration

The aim of calibration is not just to "pick" a parameter set but to understand which parameter sets can reproduce (mimic) the observed reference data sets. The topic is extremely vast and I do not expect to see an exhaustive treatment of the question for a study like the one at hand. However, making predictions about future water balance

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requires at least to give insights about how different parameter sets assign water to the different mass balance terms instead of presenting just single numbers corresponding to the single parameter set that has been chosen. In a case like the one here where all water balance terms are calibrated, such an analysis is crucial and likely to show that very different water balance terms can lead to a good discharge.

References

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