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Hydrology and Earth System Sciences Discussions

Interactive comment on "Prediction of future hydrological regimes in poorly gauged high altitude basins: the case study of the upper Indus, Pakistan" *by* D. Bocchiola et al.

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The manuscript is focused on the third pole region (Hindu Kush, Karakoram and Himalaya). The aim is to predict future hydrological regimes in poorly gauged, high altitude, basins. In particular, the case of the Shigar river (7000 kmˆ2, more that half glacierized, elevation ranging from 2000 to 8000 m asl) is presented. The basin is indeed a poorly gauged catchment: only two (daily) meteorological stations are available (at 3015 and 3926 m asl) and 8 stations with monthly weather data (all positioned below 2500 m). Only one discharge gauging station is available, located at Shigar bridge (7000 kmˆ2 upstream; 2200 m asl). Monthly mean discharges registered in this section over the period 1985-1997 are used to calibrate the model. The proposed exer-

cise, aimed at evaluating climate change effects on water resources in the study area, relies on a rather classical approach of feeding a hydrological model with downscaled GCM data. However, after reading the paper, given the extremely complex condition where the application is done, I remain mostly unconvinced of the appropriateness of the adopted modelling framework and feasibility of some of the hypotheses. The most critical points in my opinion are discussed hereinafter:

1) The Authors propose to "use yearly total precipitation from 8 [meteorological] stations to evaluate the presence [. . .] of monthly lapse rate of temperature and precipitation" (3752:20-24). My objection is that estimating a precipitation/temperature rate from two stations located at relatively low elevation and then extrapolating the trend up to 8000 m is not a "feasible hypothesis". The Authors seem to be somehow aware of this weakness when they state "At high elevation this may lead to an overestimation of precipitation. However, this may have a little impact on the hydrologic balance, because with increasing altitude contributing area decreases significantly" (3753: 1-5). The contributing-area effect is a well known effect, but it cannot be used as a justification in this case and, more in general, when the sensitivity of a snow- and glacier-driven regime is investigated (due to the direct effect of the snowline migration, induced by temperature increase, on the melted volumes and, hence, on discharge). In this sense, also the results shown in Figure 5, where SWE (snow water equivalent) in elevation band 6 is found to increase during the calibration period 1985-1997, could be an effect of an uncontrolled increase of precipitation at high elevation due to the use of Eq. 1.

2) The other weak point of the paper is related to the selection of the model time-scale. The Authors, despite having most of the data available at monthly time step (included discharge at the control section, used for calibration), decide to use a daily time step (and to disaggregate the data). The reason why disaggregation is adopted, rather then addressing the problem at the monthly time-scale, is not evident, especially in a "typical ground of application of PUB concepts, where simple and portable hydrologi-

cal modelling [...] is necessary" (3744: 10-12). Contrary to this purpose, in fact, the downscaling procedure introduces additional parametrization, both in the disaggregation step (described at page 3753) and in the (daily) modelling part (whose parameters are listed in Table 3). In addition, the verification of model outputs (versus monthly averages at Shigar bridge) appears a rather weak technique to validate model structure. As a consequence, the "additional information" resulting from disaggregation, e.g. the sensitivity of the flow duration curves to future climate scenarios, will be likely affected by high uncertainty and/or "inaccuracy", as stated by the same Authors at lines 3763: 12-13. I suggest the Authors should try to reframe their modelling approach, by evaluating the feasibility of adopting a monthly-scale process description.

To conclude I feel that, to make their contribution suitable for publication in HESS, the Authors should urgently tackle and discuss the two major methodological problems listed above. Moreover, while revising the paper, they must be particularly careful with paragraph 5.3 (Future hydrological cycle) which, despite being the main result of the study, appears rather confused (and confusing) in that: i) the difference between "unchanged" and "changed" glacier coverage scenario is not well explained, nor very intuitive, in the current paper version and ii) the reference to the "index flood" behaviour (3764:14) sounds somewhat unexpected and groundless in a paper aimed at the prediction of the sensitivity of water resources of high elevation basins to climate change. In addition, the Authors should make an effort to polish the language and style of the manuscript (see e.g. lines 3749: 15-19); as well as the accuracy of the exposition (e.g., the acronyms not always are explained, as in the case of the snow covered area SCA).

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