**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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General comments:

This paper presents a model to assess the impact of glacier melt on water resources in the Rio Santa basin in the Peruvian Andes. Although I am inclined to disagree with some of the claims made by the authors about the scope of the problem (see below), the issue is timely and relevant. Unfortunately the manuscript presents several errors, inaccuracies, and unverified claims that obscure its scientific value. Many of which already highlighted by the first reviewer.

Most problematic, however, I find the author's claim that existing experimental methods

of the type of Mark and Seltzer (2003) are less useful for water resources management than model simulations of the type they represent in the paper. Their case is unconvincing. Straightforward relations between for instance the glaciated area and key hydrological variables such as the specific discharge and coefficient of variation of streamflow (e.g., Mark and Seltzer (2003), Figure 10), are very useful to assess the potential impact of glacier melting at a regional scale. Of course added value could be given by models that assimilate additional information and resolve transient processes, but in this case the modelling approach adopted by the paper is very problematic. Tropical mountain environments do present serious challenges in terms of modelling due to their data scarcity. It is therefore correct that a simple conceptual approach be used, which is supported by the available data. However, rather than a proper analysis of the complexity supported by the data, many decisions on the model application appear to be taken ad hoc and based on convenience rather than rigorous thought, with rather unconvincing arguments (e.g., p 881 - 882). The main issue of the model however is the extremely poor performance. Table 3 states Nash Sutcliffe efficiencies as low as 0.19 with only few values exceeding 0.6. Particularly given the monthly timesteps and the short calibration/validation period, this is clearly a model with very limited predictive capacity. Just looking at the model performance on the Artesoncocha catchment on which it was calibrated (Fig. 4) shows that the model severely and consistently underestimates the dry season flows (e.g., in 2004). This may indicate the model's inability to adequately present groundwater flows, which is one of the main aims of the paper. This of course also has direct implications for the usefulness of the model for water resources management.

There may be various reasons for the poor model performance but given the often large model bias, the water balance of the catchments is obviously a major issue. Better interpolation methods (see specific comments) should be considered, although the low density of rain gauges and extreme precipitation variability may make it nearly impossible to close the water balance of the catchment. Also, monthly streamflow data are insufficient to characterise the hydrological response of small basins such as

the upper catchments of the Santa river. The model's regionalisation may also be an issue. The calibration of the model is not clear (see specific comments) but it seems the authors have used parameter values calibrated on one catchment to predict the discharge of other catchments. In this case, the low model performance may indicate problems with identification of parameter sets or the applicability of these sets to other catchments. The issue may again be related to the data: monthly measurements are probably highly insufficient to calibrate catchment parameters for small catchments.

As a result, I do not think that the specific objectives of the study are fulfilled (871/17-22). Observed river flows are surely not simulated adequately, which casts doubts on the simulation of hydropower operations. Therefore it is also doubtful that the glacier and groundwater contribution in the Rio Santa can be quantified more adequately using a complex hydrological modelling approach than earlier field based studies have done, as the authors claim.

I strongly suggest the authors to rethink the concept of their paper. I agree with the first reviewer that the paper is very ambitious, but (therefore?) fails in many methodological issues and conclusions which undermine its scientific value. One option may be to focus on assessing the performance of the used hydrological modelling algorithms in tropical mountain environments such as the Santa basin. This can be done by evaluating different aspects of the model, including the degree-day method, the representation of groundwater flows, and the impact of uncertainties in boundary conditions such as precipitation on the model performance. Parameter sensitivity and a proper uncertainty analysis would be essential here. Evaluating the added value of distributed hydrological models in local water resources management would then be a logical next step. Alternatively, an evaluation of glacier melt and climate change impacts on local water resources may be done with simpler tools such as existing relations between glacier area and hydrological variables.

Finally, the paper suffers in places from unsupported attributions and exaggerated claims which undermine its scientific rigour. For instance, it is very unlikely that "roughly

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one million people" live in the upper portion of the Rio Santa watershed. The largest city in the upper watershed, Huaraz, has a population of only around 130K inhabitants. The entire Ancash region, which is around 3 times as large as the upper watershed (depending on how the latter is defined) has a population of just over 1 million, with the largest city (Chimbote) located at the coast. Similarly, the statement that "large populations rely on glacier melt for water and hydropower at the level of the Andes" is highly contested. It is still unclear how many people really crucially depend on water from glacier melt in the Andes (as opposed to catchments where glaciers contribute to streamflow but long-term changes in the regime because of glacier melt may be minimal). Glaciers do indeed have a runoff ratio that is higher than most other land covers and may buffer seasonal river flow, but both effects tend to dilute with basin size and may be dwarfed by changes in other ecosystems (e.g., land cover). There surely is an urgent need to understand and quantify these processes and I can see an important role for hydrological models, but one should be careful not to have the predictions obfuscated by poor parameter values and untested model representations.

specific comments

873/25: is this not a serious overstatement? The population of Huaraz is around 130K

876: How is effective precipitation defined here? It does not seem to be the common definition of precipitation that makes it into streamflow, but rather precipitation

881/16 to 882/2: this section is very confusing. State clearly how the existing model was adapted, and which parameters are calibrated. The section on data should then clarify which data are being used for calibration. The discussion on model complexity (generality) trade-offs sounds too much as a post-hoc justification rather than a balanced scientific consideration.

887/4 what are considered normal cases? With monthly data the main challenge of the model is to represents the seasonality which is largely driven by the precipitation input, so yielding NS efficiencies above 0 would be expected.

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889/16: "DEM issued by the maps": do you mean a DEM generated by contour line maps provided by IGM?

890/12: where does the estimate of 300l/day come from? This is rather small, given that typical urban water consumption is often of the order of 250l/p/day

890/24: given the strong impact of topography on precipitation (991/1) why was a method using covariables (such as co-kriging) not considered.

893: "The calibrated glacier parameters [...] were used to run the model for a calibration and a validation period": I am not sure how calibrated parameters can be used to run a model for a calibration period. I suppose the calibrated glacier parameters were used as initial parameters and further calibrated as streamflow? What method was used for calibrating the parameters? Were the parameters in Table 2 further calibrated? If not, what is the basis for the estimation of the values? No information about the soils in the region is given, so it is hard to determine whether values such as the root zone capacity are adequate.

897: section 4.4. is far to short. Which models were considered, what downscaling method was used (delta method?), how were extremes dealt with? What possible adaptation strategies are considered?

table 2: are these initial estimates or calibrated parameters?

Table 2: use English for land covers as in 990/1

Table 3: indicate in the caption what the stars in the Artesoncocha watershed mean

Table 5: what is the "lowest pour point"?

Technical corrections:

871/16: in which sense is the Cordillera Blanca "singular"?

871/26: the watershed is surely not "global"?!

872/5: such as Peru's

873/10: associated to

874/16: more than

874/19: did not include

887/10: clarify that these are provinces, and that they also include the lower basin and surroundings (e.g., Santa).

893/13: thin line

896/6: "we would like to be confident" -> reformulate

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